The Principles of Biological Classification: The Use and Abuse of Philosophy

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In recent years two groups of taxonomists have attempted to influence the general goals and methods of biological classification. The first group, which emerged in the late 1950's, has been called variously neo-Adansonian, numerical, computer and phenetic taxonomy. The founders of this school, Robert R. Sokal and P.H.A. Sneath, termed their unified approach to systematics "neo-Adansonian" because of the affinities which they saw between their views and those of the 18th-century botanist, Michel Adanson (1727-1806). Today little mention is made of Adanson. His ideas turned out not to be as prescient as Sokal and Sneath had at first thought. Besides, he had served his purpose as a patron saint ([79], p. 23). Sokal and Sneath also termed their approach "numerical" because they believed that taxonomists should make greater use of available mathematical techniques. Not only should taxonomic characters be coded quantitatively but also estimates of affinity should be made on the basis of explicitly stated clustering procedures. Because such computations are frequently both complex and tedious, computers must be used. Finally, the term "phenetic" was coined to emphasize the basic epistemological stance of this school. According to the pheneticists, organisms should be clustered on the basis of numerous, equally-weighted, theoretically neutral traits to produce a general-purpose, theoretically neutral classification. Differential weighting, theoretical speculations and special-purpose classifications are permitted if they are constructed explicitly and objectively on the basis of a phenetic, general-purpose classification. The numerical taxonomists have had considerable success. The use of computers and various mathematical techniques are now commonplace in taxonomy. Their phenetic philosophy, however, has been much less successful.

During this same period, another school of taxonomy was developing in Germany, the phylogenetic school of Willi Hennig [32]. Initially the only access which non-German-speaking taxonomists had to Hennig's views was through the criticisms of Simpson [76] and Mayr [47]. Even

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the appearance of Hennig's Phylogenetic Systematics [33] and R.A. Crowson's Classification and Biology [15] did not change the situation much. Not until Gareth Nelson took up the cause did Hennig's views begin to catch on among American systematists ([51], [52], [53]). Hennig termed his system "phylogenetic" because the goal of biological classification, as he sees it, is to represent explicitly and unambiguously the order of branching in phylogeny. He picked order of branching because he believed that it can be discerned with sufficient certainty in most cases and because the Linnaean hierarchy lends itself to representing such discrete phenomena. Hennig's opponents objected to his appropriating the term "phylogenetic" for his own use. Phylogeny is more than the branching off of separate lineages (or clades). Lineages also diverge from each other at varying rates. Degrees of divergence should also be represented in classifications. Sometimes the attainment of a general level of organization should take precedence over cladistic relations in biological classifications. The development of "grades" is also a characteristic of phylogenies. The Hennigian school should actually be called "cladistic", to emphasize its single-minded attention to clades [47].

Although the Hennigians themselves are less than enthusiastic about the term, they have accepted it for lack of a better alternative. The principles of cladistic analysis as set out by Hennig and other early cladists ([5], [9], [32], [33]) were closely connected to a particular view of the evolutionary process. Later cladists have gradually detached the principles of cladistic analysis from any particular theory of evolution. All that is necessary is that species evolve and evolution be largely diverging ([6], [13], [19], [24], [58]). Certain cladists are willing to generalize these principles beyond biological evolution to apply to any system which changes via descent with modification [59]. Finally, Nelson [55] is attempting to extend cladistic analysis to apply to all patterns regardless of their genesis.

Early in the development of cladism, Colless ([10], p. 291) argued that Hennig's system was really nothing but a "form of statistical, phenetic taxonomy." Cladists and non-cladists alike responded that this claim was nonsense ([2], [11], [12], [75]). One chief difference is that pheneticists cluster taxa into nested fuzzy sets on the basis of various estimates of overall similarity. Numerous characters taken at random are used. Cladists use only those traits which produce nested sets of discrete taxa. Sometimes only a single character distinguishes one taxon from another. A second difference is that most cladists are attempting to reflect order of phylogenetic branching in their classifications, while pheneticists exclude all phylogenetic considerations from their classifications. However, this second difference has been gradually eliminated as certain cladists have severed all connections between their classifications and phylogeny. The first difference remains [41].

As different as the pheneticists and cladists may be on a variety of counts, they are similar in at least two respects: their basic principles are largely methodological and they argue for these principles on the basis of fundamental epistemological beliefs. Advocates of both systems of classification cite the works of philosophers to bolster their claims that their methods and resulting classifications are objective, repeatable and hence genuinely scientific, while those of their opponents are not. The pheneticists trace their philosophy through the writings of J.S.L. Gilmour ([27], [28]) to P.W. Bridgman ([7], [8]). The cladists for their part have leaned heavily on the writings of Karl Popper ([63], [64], [65], [66]).

The importance which philosophy and the works of particular philosophers have had in recent controversies in biological systematics might come as a surprise to philosophers. For example, five of Popper's major works have been reviewed at some length in the journal Systematic Zoology ([60], [61]). Additional reviews are in press. During a full-day session at the 1978 meetings of the American Society of Zoologists, six biologists and two philosophers debated the relevance of Popper's views on the nature of science for biological classification. Philosophers might well be pleased by all this attention but complain about the particular choices which these practicing scientists have made in selecting philosophical authorities. Neither Gilmour nor Bridgman is a professional philosopher, and the combination of phenomenalism and operationalism urged by these two men was long ago rejected as untenable by professional philosophers. Popper at least is a widely respected philosopher whose views, though not the last word on any of the subjects which he treats, are still worth taking seriously. Popper is not the only philosopher writing about the nature of science. He may not even be the best philosopher writing on the subject. But his views cannot be dismissed out of hand.

Platnick and Nelson in this volume ask, "What might it [taxonomy] teach the philosopher?" ([62], p.126). The question addressed in this paper is just the opposite. Perhaps taxonomists have not made the best possible choices in picking philosophers to read. Perhaps they have gone to philosophers for weapons with which to bludgeon their opponents rather than for genuine understanding. But have philosophers written anything about the nature of scientific classification which would have helped interested taxonomists had they read it? I attempt to answer this question first for two 19th-century philosophers who published their major works immediately prior to the appearance of Darwin's Origin of Species [16] -- Whewell (1794-1866) ([81], [82]) and Mill (1806-1875) ([49], [50]) -- and then for Jevons (1855-1882) [43], who developed his philosophy immediately after the evolution of species had become widely accepted. Because both Whewell and Mill rejected biological evolution, they were spared the necessity of attempting to reconcile their views on the nature of biological species with Darwin's theory. Jevons was unable to avoid this task. Evolutionary theory had considerable impact on such philosophical issues as materialism, free will, and teleology. It hardly touched essentialist interpretations of natural kinds. Not until the second half of this century did such philosophers as Beckner [1], Gasking [25] and Hull [34] take seriously the implications of evolutionary theory for species as natural kinds.

If Bridgman and Popper did not loom so large in the taxonomic literature, little notice would have to be taken of their work. The weaknesses of operationism are too well known among philosophers to warrant rehearsing them once again [36]. Popper himself has never been especially interested in problems of classification and outright hostile to questions of definition and meaning. However, because taxonomists have found the writings of Bridgman and Popper relevant to their own work, some attention must be paid to them. Popper's works are relevant to taxonomy for a second reason. From the beginning, biological species have been interpreted as natural kinds, universals, secondary substances, spatiotemporally unrestricted classes, and the like. Popper notices that species, as they function in the evolutionary process, cannot be interpreted as unrestricted classes. Instead, he argues, they are numerical universals. On these same grounds, several biologists have claimed that species are not classes at all but spatiotemporal individuals ([26], [30], [46]). The paper concludes with an investigation of this bizarre suggestion.

1. Natural Kinds and the Evolution of Species

The evolution of species is important to the philosophies of William Whewell and John Stuart Mill because the notion of natural kinds played a central role in their systems and biological species were among the most frequently cited examples of natural kinds. Mill divided laws of nature into two types, laws of succession and laws of co-existence. Laws of succession dealt with regular sequences of kinds of natural events; e.g., planets revolving around stars, balls rolling down inclined planes, and gases expanding. Laws of co-existence dealt with the constant conjunction of the traits used to define natural kinds; e.g., malleability, ductility, and mass in the case of substance terms like 'gold', the possession of webbed feet, white feathers, and a long neck in the case of biological species. It is worth noting here that Mill was attempting to discover traits which all and only the members of a natural kind possessed. He would have not been in the least content with the sort of "traits" suggested by defenders of present-day essentialism. The claim that the essence of swan is swanness would not have impressed him at all. In addition, in this discussion Mill is concerned with the essences of secondary, not primary, substances.

For Mill the goal of all forms of scientific classification is the discovery of natural kinds:

> The ends of scientific classification are best answered when the objects are formed into groups respecting which a great number of general propositions can be made, and those propositions more important, than could be made respecting any other groups into which the same things could be distributed. The properties, therefore, according to which objects are classified should, if possible, be those which are causes of many other properties ([49], pp. 466-467).

Although the Aristotelians were wrong on a host of counts, they were

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right about natural kinds:

> Every class which is a real Kind, that is, which is distinguished from all other classes by an indeterminate multitude of properties not derivable from one another, is either a genus or a species ([49], p. 81).

> [Natural kinds] are parted off from one another by an unfathomable chasm, instead of a mere ordinary ditch with a visible bottom ([49], p. 80).

> The universe, so far as known to us, is so constituted, that whatever is true in any one case, is true in all cases of a certain description; the only difficulty is, to find that description ([49], p. 201).

> The problem is, to find a few definite characters which point to the multitude of indefinite ones. Kinds are Classes between which there is an impassable barrier; and what we have to seek is, marks whereby we may determine on which side of the barrier an object takes its place ([49], p. 471).

At bottom Mill justified all laws of succession by reference to a universal law of causation. He was unable to find a comparable justification for laws of co-existence. The best he could do was to refer to the original creation of the universe. Whenever and however the universe was created, entities were so constituted that they fell into natural kinds ([49], p. 381). For an idealist who believed in the independent existence of some sort of ideal forms, horses could come and go while the Ideal Horse remained eternal and immutable, but for an empiricist like Mill, the evolution of natural kinds was equivalent to the evolution of a law of nature. Admitting that species evolve was comparable to countenancing variation in the gravitational constant in Newton's laws.

Mill was aware that both laws of succession and laws of co-existence seem to have exceptions. For both he argued that the exceptions were only apparent, not real ([49], p. 470). To be genuine laws, they had to be exceptionless. For example, Mill was aware that sometimes objects can be arranged in a series such that the first has some quality in common with a second, the second has some quality in common with a third, the third with a fourth, and so on, but that distant members of the series might have no quality in common with each other. (See later discussion of Gasking's serial relations.) Mill argued that such series either had to be analyzable into discrete natural kinds or else could play no role in science ([49], p. 442).

For Mill science is a deductive hierarchy of laws. For Whewell it is a deductive hierarchy of conceptions. Conceptions are prior to laws. Whewell remarks that "any one can make true assertions about dogs," but he asks, "Who can define a dog?" ([81], p. 475). According to Whewell, "our persuasion that there must needs be characteristic marks by which things can be defined in words, is founded on the assumption of the necessary possibility of reasoning."([81], p. 476). A stronger justification is difficult to imagine. Whewell admits that the definitions presented in natural classifications have to be "considered as temporary and provisional only," not because natural kinds change, but because our knowledge improves through time ([82], p. 424). Scientists gradually approach ultimate truth by superinducing conceptions on phenomena.

In mathematics and certain areas of physics, Whewell notes that natural kinds are absolutely discrete, but in other areas, he concedes that the boundaries between natural kinds are none too sharp. In such cases, natural kinds can not be defined in terms of essential characteristics but only by means of the Type Method:

> But we may here observe, that though in a Natural group of objects a definition can no longer be of any use as a regulative principle, classes are not, therefore, left quite loose, without any certain standard or guide. The class is steadily fixed, though not precisely limited; it is given, though not circumscribed; it is determined, not by a boundary line without, but by a central point within; not by what it strictly excludes, but by what it eminently includes; by an example, not by a precept; in short, instead of Definition we have a Type for our director ([81], p. 494).

According to Whewell, the Type must be a typical member of its class, it "must be connected by many affinities with most of the others of its group; it must be near the center of the crowd, and not one of the stragglers"([81], p. 495). Nor did he think that the existence of a few intermediaries totally nullified the Type Method. "And even if there should be some species of which the place is dubious, and which appear to be equally bound to two generic types, it is easily seen that this would not destroy the reality of the generic groups, any more than the scattered trees of the intervening plain prevent our speaking intelligibly of the distinct forests of two separate hills" ([81], p. 495). For Whewell natural kinds did not have to be absolutely discrete, but they had to be separable. According to one of Whewell's methods of induction, the Method of Gradation, any two classes of phenomena which can be connected by a continuous gradation of properties must be considered a single class and not two ([82], p. 246).

Whewell was correct when he noted that natural kinds with less than perfectly sharp boundaries "are so contrary to many of the received opinions respecting the use of definitions and the nature of scientific propositions, that they will probably appear to many persons highly illogical and unphilosophical" ([81], p. 493). Mill ([49], p. 472) and later Jevons ([43], p. 723) agreed. For most philosophers of the day, the only real boundaries were sharp boundaries. The conviction persists to the present ([31], p. 158).

Neither Whewell nor Mill was much impressed by the claims of early evolutionists that species evolve and justifiably so, but in 1859 Darwin published a book that rapidly converted a large percentage of scientists and non-scientists alike [40]. Whewell and Mill remained unconvinced. The likelihood that William Whewell, author of one of the Bridgewater Treatises and a staunch opponent of Lyellian uniformitarian geology, could have been converted to a belief in the gradual transformation of one species into another before his death in 1866 was slight. Theistic teleology played too central a role in his philosophy for that to occur. But, as Ruse has suggested, Whewell's adoption of the Type Method might make it appear as if a belief in "essentialism was probably not a crucial factor in his opposition."([73], p. 251). Nowhere in his writings have I been able to find a passage in which Whewell explicitly rejects the evolution of species because it was incompatible with his conception of natural kinds, but when his Method of Gradation is combined with a belief in gradual evolution, species cease to exist as separate natural kinds. If Darwin was right, Whewell would be forced to consider all species as a single natural kind.

Mill was somewhat more receptive to Darwin's theory. In the 8th edition of his Logic (1872), he lists "Mr. Darwin's remarkable speculation on the Origin of Species" as an "unimpeachable example of a legitimate hypothesis."([49], p. 328). But it was only an hypothesis, an unproven hypothesis. When he died in 1873, fourteen years after the publication of the Origin, he was forced to conclude that the situation had not changed much. Regardless of what a majority of biologists might think, Creation by Intelligence was still the more probable hypothesis ([50], p. 172). Once again, design seemed to be the major impediment to the acceptance of evolution. Species might evolve but not in the haphazard way proposed by Darwin. How significant the incompatibility between Mill's conception of natural kinds and gradual evolution was for Mill's rejection of Darwin's theory is difficult to say.

Because both Whewell and Mill rejected Darwin's theory, neither was faced with the problem of reconciling the gradual evolution of species with their own belief that species were paradigm examples of natural kinds. Later philosophers were not spared this problem, though most studiously avoided it. William Stanley Jevons was a direct descendant of Mill. He also had the good fortune to study under Augustus de Morgan (1806-1871) and to have access to the work of George Boole (1815-1864). Jevons accepted the evolution of species and acknowledged that natural classifications in biology must be in some sense genealogical:

> It is true that in the biological sciences there would be one arrangement of plants or animals which would be conspicuously instructive, and in a certain sense natural, if it could be attained, and it is that after which naturalists have been in reality striving for nearly two centuries, namely, that arrangement which would display the genealogical descent of every form from the original life germ ([43], p. 680).

Classification was as central to Jevon's notion of science as it had been for Mill and Whewell:

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> Science, it was said at the outset, is the detection of identity, and classification is the placing together, either in thought or in actual proximity of space, those objects between which identity has been detected. Accordingly, the value of classification is co-extensive with the value of science and general reasoning.([43], p. 673-674). The purpose of classification is the detection of the laws of nature.([43], p. 675). Science can extend only so far as the power of accurate classification extends.([43], p. 730). ...the limits of exact knowledge are identical with the limits of classification.([43], p. 731).

According to Jevons, logical inference requires identity:

> By the higher faculties of judgment and reasoning the mind compares the new with the old, recognizes essential identity, even when disguised by diverse circumstances, and expects to find again what was before experienced. It must be the ground of all reasoning and inference that <u>what is true of one thing will be true of its equivalent</u>, and that under carefully ascertained conditions <u>Nature repeats herself</u>. ([43], pp. 1-2).

But if species are supposed to be natural kinds and evolution is gradual, Nature does <u>not</u> repeat herself! Rather she is continually telling a new story. Jevons acknowledges the problem but makes no attempt to solve it. Classification requires constancy of character, but if Mr. Darwin is right:

> ... we must no longer think that because we fail in detecting constancy of character the fault is in our classificatory sciences. Where gradation of character really exists, we must devote ourselves to defining and registering the degree and limits of that gradation. The ultimate natural arrangement will often be devoid of strong lines of demarcation. ([43], p. 721).

Nor will Whewell's Type Method help in the least:

> It is either not a real method of classification at all, or it is merely an abbreviated mode of representing a complicated system of arrangement. A class must be defined by the invariable presence of certain common properties. ... Even a single exception constitutes a new class by itself. ([43], p. 723).

All that Jevons was able to say in the face of the problems posed by the gradual evolution of species is that in biology, a "certain laxity of logical method is thus apt to creep in" ([43], p. 724). Science can extend only so far as the power of accurate classification extends, and accurate classification can never extend to gradually evolving species. Hence, if Darwin is right, biology can never be a genuine science.

2. Natural Kinds as Cluster Concepts

If species evolve very gradually, one character being transformed im-

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perceptibly into another, new traits becoming distributed through populations only very slowly, then the names of species as temporally extended lineages cannot be defined in terms of single sets of conditions which are severally necessary and jointly sufficient for membership. Some characters may be universally distributed among the members of a species, but these same characters are likely to be possessed by organisms in other species as well. Some characteristics may be possessed only by the organisms belonging to a particular species, but these characteristics are likely not to be possessed by all the relevant organisms, especially when these characters first appear.

One solution to this problem is to treat the names of particular taxa as Wittgensteinian cluster concepts. Morton Beckner [1] was the first philosopher to investigate this solution to the species problem in depth, followed by Gasking [25] and Hull [34]. According to these philosophers, one way to view species is as classes defined in terms of the possession of <u>enough</u> of the <u>more</u> important defining characteristics developed to a <u>sufficiently</u> high degree. Not all plants possess chlorophyl and some non-plants do, but that does not mean that the possession of chlorophyl is irrelevant to an organism's being a plant. Biological taxa at all levels are "polytypic", to use Beckner's term.

Although Gasking's discussion has gone all but unnoticed, it is the most original and detailed on the subject. He distinguishes between sets and classes. A set in Gasking's usage is an extensional notion: a set cannot change membership without becoming a new set. Gasking treats classes intensionally. They are defined in terms of property qualifications. Of all the various ways in which classes can be defined intensionally, Gasking emphasizes those defined by means of a relation and a focus. His chief examples are fleets of ships, Whewell's forests, and biological species. In this context he distinguishes between simple transitive relations and non-transitive serial relations. Simple clusters are defined in terms of relations which each member of the cluster can have to any other member. For example, all the organisms which comprise a species at any one time are likely to be morphologically quite similar to each other. Followed through time, simple clusters form serial clusters. Each ancestral population can be quite similar to its immediately ancestral population, but distant populations might be morphologically quite different. They are, however, serially similar to each other.

If species evolve as gradually as Darwin and later "gradualists" have maintained, then the case for treating species as clusters of some sort is quite compelling. More recently, "saltationism" of a limited variety has staged a comeback. According to Eldredge and Gould [20], speciation usually (possibly always) occurs by means of the isolation of a small, peripheral population which succeeds in the space of a few generations in establishing a new gene complex. Genetically and ecologically the process is reasonably continuous, but from the perspective of geological time, it is saltative. On this view, a new species might come quite rapidly to be characterized by a new trait which distinguishes it sharply from all other species. (For this reason cladists have been partial to this model of speciation.) The extent to which evolution is gradual or saltative is an empirical matter. To the extent that it is saltative, taxonomists will be able to discover essential traits to characterize particular taxa; to the extent that it is gradual, they will fail.

Thus far, we have discussed appropriate ways of defining the names of particular species such as Canis familiaris. Of greater importance is the question of how to define the species category itself. Operationally-oriented taxonomists, like the pheneticists, have opted for a definition in terms of some degree of phenetic similarity. Organisms belong to the same species if they are similar enough to each other. Most theoretically-oriented biologists, however, seem to opt for a definition which will define species as significant units in the evolutionary process. The biological species definition is so important because it deals with gene flow, and gene flow in turn is supposedly very important in the maintenance of evolutionary cohesiveness. Because gene flow tends to promote overall similarity, applications of the morphospecies concept tend to coincide with applications of the biospecies concept. In fact, Ruse [70] has suggested that one excellent reason for considering species real units in nature is the extensive extensional overlap when these two logically independent criteria are used. The problem is what to do when the results of these two definitions do not overlap, e.g., in the case of sibling species and polytypic species. In such cases, most taxonomists opt for some version of the biospecies concept.

Beckner [1] and Hull [34] have not been content to claim that the names of particular species are cluster concepts. They have also argued that 'species' itself is a cluster concept. No one character, not even potential interbreeding, is adequate for defining species as units of evolution. Beckner ([1], p. 60) suggests two alternative criteria for defining 'species,' one the usual potential interbreeding, the other an historical criterion. All members of a species must be capable of mating successfully with each other to produce viable offspring (proper allowance being made for the different sexes) or be first-generation descendants of such a population. Hull [34] lists four alternative criteria: actual interbreeding, serial interbreeding with synchronic populations, descent without appreciable divergence from ancestral populations, or exhibition of "analogous" degrees of similarity and dissimilarity in asexual forms.

The definitions suggested by Beckner and Hull differ from traditional definitions because the elements are disjuncts, not conjuncts. The fulfillment of only one of the criteria is sufficient for a group of organisms being considered a species. Hull's definition is peculiar in a second respect: it is indefinite. Additional criteria might have to be added as our knowledge of the evolutionary process increases. The arguments for treating particular species as clusters are quite compelling if evolution is largely gradual. The arguments for treating the species category in the same way are not nearly so compelling, no more so than those for any theoretically significant term in science. Just because Canis familiaris evolves, it does not follow that species as

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the class of all species also evolves. (As I argue elsewhere, classes are not the sorts of thing that can evolve [42].)

The fact that the definitions of the term 'species' set out by Beckner and Hull are presented in a non-traditional form is of some significance, but another feature of their definitions which is even more important has been overlooked until quite recently. Hull's definition mentions actual gene exchange (not just potential interbreeding), and both definitions mention descent. Gene exchange in interbreeding and gene transmission in all forms of descent both require spatiotemporal contact. As Gasking [25] was the first to notice, if species are serial clusters defined in terms of spatiotemporal relations to spatiotemporally localized foci, species themselves become spatiotemporal particulars (individuals) and cease being natural kinds. The consequences of this line of reasoning will be discussed in the final section of this paper.

3. Operationism and the Role of Theory in Classification

The great conflict between science and philosophy in the 19th century was between evolutionary theory and the twin tenets of teleology and essentialism. The tension between evolution and teleology was reduced somewhat by the rise of theories of directed, progressive evolution. Essentialists in turn were strongly disposed to theories of saltative evolution. Perhaps species are not eternal and immutable, but at least they are discrete. In the 20th century, philosophers were jolted just as rudely by two physical theories -- relativity and quantum theories. Modern positivists proposed to take science seriously. That meant taking relativity theory and quantum theory seriously. The most remarkable outcome of this enterprise was the operationist philosophy of P.W. Bridgman.

As Bridgman saw it, Einstein, Heisenberg and other contemporary physicists had shown that earlier physicists had assumed all sorts of things about the empirical world which they had no right to assume, for example, that moving a yardstick does not affect its length. Bridgman was determined to put "physical thinking on such a secure basis that this sort of thing may not happen again."([8], p. 5). Bridgman's solution to the fallibility of science was his infamous notion of operational definitions. "In general," so Bridgman claimed, "we mean by any concept nothing more than a set of operations; <u>the concept is synonymous with the corresponding set of operations</u>."([7], p. 5). For example, the "concept of length involves as much as and nothing more than the set of operations by which length is determined."([7], p. 5). Bridgman included in his notion of operations, both physical operations, such as laying down rulers end to end, and paper-and-pencil operations. The problem with paper-and-pencil operations was to distinguish those which were legitimate from those which were not, a problem which Bridgman never solved.

Because Bridgman's operationism seemed so ill-conceived, philosophers tended to ignore a second feature of Bridgman's philosophy which was just as important -- his advocacy of phenomenalism and its resulting solipsism. All a scientist really knows anything about are his own sense-data. It was this feature of the Unity of Science movement which attracted J.S.L. Gilmour. According to Gilmour, reason constructs external reality by clipping together sense-data. "In any consideration of scientific method it is essential to distinguish between these 'clips' and the sense-data which they hold together. The latter are given, once and for all, and cannot be altered, whereas the former can be created and abolished at will so as the better to give a coherent picture of the ever-increasing range of sense-data experienced." ([28], p. 464). On the basis of such considerations, Gilmour produces the following characterization of biological classification:

> To sum up, starting from basic epistemological considerations, we are led to the view that a natural classification of living things is one which groups together individuals having a large number of attributes in common, whereas an artificial classification is composed of groups having only a small number of common attributes; further, that a natural classification can be used for a wide range of purposes, whereas an artificial classification is useful only for the limited purpose for which it was constructed; and lastly that both types are created by the classifier for the purpose of making inductive generalizations regarding living things. ([28], p. 468).

The main goal of the positivists was to distinguish metaphysics (which they thought bad) from science (which they thought good) so that it could be purged from science. The instrument devised to fulfill this goal was the Principle of Verifiability. The faults of this principle, both as a criterion for meaningfulness and as a principle for distinguishing science from non-science, are too familiar to run through again here. For our purposes, its most important weakness is that it makes all scientific laws meaningless. Scientific laws refer to indefinitely many instances, all of which cannot possibly be tested.

As Popper has remarked recently, "Everybody knows nowadays that logical positivism is dead", but no one goes on to ask the question, "Who did it?" Although he did not do it on purpose, Popper is forced to admit responsibility. He did it ([68], p. 88). The main argument against positivism in general and operationism in particular is the role of theories in science. As Popper sees it, "Operationism and instrumentalism must, I believe, be replaced by 'theoreticism', if I may call it so: by the recognition of the fact that we are always operating within a complex framework of theories and that we do not aim simply at correlations, but at explanation." ([67], p. 63). Or, as Carl G. Hempel remarks, in concept formation "clear and objective criteria of application are not enough: to be scientifically useful a concept must lend itself to the formulation of general laws or theoretical principles which reflect uniformities in the subject matter under study, and which thus provide a basis for explanation, prediction, and generally scientific understanding." ([31], p. 146).

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Even though Popper has figured prominently in the recent taxonomic literature ([3], [5], [22], [44], [83]), he has said very little about classification, and everything which he has said has been negative. The first philosophical conviction reached by Popper (at the age of fifteen) was that arguments about words and their meanings are specious. Throughout the remainder of his life he has held fast to the following maxim: "Never let yourself be goaded into taking seriously problems about words and their meanings. What must be taken seriously are questions of fact, and assertions about facts: Theories and hypotheses; the problems they solve; and the problems they raise."([68], p. 19). For Whewell, questions like "What is life?" are central to science. Periodically philosophers and scientists still ask such questions, but they are mistaken to do so. Questions of fact, not definition, are important in science ([63], p. 211; [64], p. 29).

The reason for taxonomists paying so much attention to the works of Popper is that they think that they can use his Principle of Falsifiability to show that their classifications are truly scientific, while those of their opponents are not. However, Popper has certain views on the nature of taxonomic statements which make his Principle of Falsifiability inapplicable to them. Popper's attitude toward evolutionary theory has always been equivocal. One source of his scepticism is his strong antipathy toward "historicism", the belief that laws of inevitable historical development can be discovered in human history. Because early historicists like Marx and Lenin attempted to justify their position by reference to evolutionary theory, Popper is led to denigrate it. If biologists claim that evolution occurs along predetermined paths, Popper argues, then they are as mistaken about biological evolution as Hegel, Lenin and Toynbee are about human history. At the time, many biologists did believe in orthogenesis. Contemporary versions of evolutionary theory are antithetical to this view. Popper no longer needs to oppose evolutionary theory because of any putative support it might give to the historicists.

Popper's change of attitude has a second source as well. Although Popper still thinks that modern evolutionary theory could stand some improvement (and suggests some himself), he recognizes evolutionary theory as a genuine scientific theory. He better, since at bottom his entire philosophical system rests on his own "evolutionary" metaphysical research program [67]. Because Ruse [74] and Olding [56] have already commented extensively on Popper's understanding of the evolutionary process, I won't go into the matter here, but one consequence of biological evolution which Popper notices is crucial for our main topic of investigation. As Popper recognizes, neither biological taxa nor the traits used to characterize them are unrestricted universals, because taxa must be monophyletic and traits must be evolutionary homologies. In spite of the appearance of the word 'all' in statements like "All vertebrates have one common pair of ancestors", none of these statements are genuine laws of nature; they are singular statements ([64], pp. 106-107).

Although taxonomists are not unanimous on the subject, most agree that all higher taxa must be monophyletic at the level of species; i.e.,

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all species included in a single higher taxon must be descended from a single ancestral species (but see [41]). Furthermore, when a taxon becomes extinct, it cannot come into existence again. Hence, taxa are not genuine natural kinds, "collections of particulars, definite in kind but indefinite in number."([49], p. 186). The same conclusion follows from the requirement that biological traits be evolutionarily homologous to each other. The wings of birds and the wings of bats are homologous to each other as wings because they resulted from the same evolutionary novelty; the wings of insects are not because they had a separate origin. Thus, in the statement "All mammals have hair", neither 'mammals' nor 'hair' refers to an unrestricted universal (see Platnick and Nelson [62] in this volume).

Popper devised his Principle of Falsifiability to distinguish between genuine laws of nature and such pseudo-scientific theories as Freudian theory. For Popper, laws of nature are strictly universal statements. However, for Popper, taxa are numerical universals and taxonomic statements are thus singular in form. In consequence, Popper's Principle of Falsifiability cannot be applied to taxonomic statements which refer to particular taxa or particular traits. Perhaps taxonomists can use Popper's philosophy in general in their continuing war over proper scientific method, but in the case of taxonomic statements, not his Principle of Falsifiability strictly interpreted. Although some disagreement persists, taxonomists have come to recognize these distinctions ([6], [14], [45], [54], [57]).

4. Species as Individuals

If contemporary philosophers of science agree on anything, it is that scientific classifications cannot be theoretically neutral. Nor can there be any prescribed order in which theoretical considerations are introduced into classification. One cannot begin by producing a theoretically neutral classification and then only later add theoretical interpretations. Theory and observation go hand in hand throughout the classificatory process ([21], [35], [36], [69]). More specifically, Hempel argues that the "morphological basis of classification" must be replaced "by one more deeply imbedded in theory, namely a phylogenetic basis."([31], p. 147). Even if one were willing to grant that all scientific classifications must be "imbedded" in scientific theories, one is still left with the task of expanding on this metaphor. What does it mean to say that biological classification must have a "phylogenetic basis"?

Numerous biologists have claimed that biological classifications are "theories" ([3], [13], [14], [47], [80]; but see also [9], [44], [45]). By this claim they seem to have several characteristics of biological classifications in mind. Phylogenetic reconstructions could be mistaken. Hence, any classifications based on phylogeny are equally hypothetical. Evolutionary theory also plays a role in phylogeny construction. Biologists who hold different views about the evolutionary process are likely to reconstruct phylogeny differently. For example, gradualists are likely to search harder for missing links than saltationists. Taxonomists produce systems of nested taxa by studying a small fraction of the organisms which comprise the groups and an equally small fraction of the traits which characterize the organisms under study. In constructing a classification, taxonomists tacitly predict that additional organisms and additional traits will conform to the pattern which they have already recognized. But none of these senses of "theory" is the one intended by philosophers when they claim that classifications must be "imbedded" in scientific theories. Rather they mean that classifications must divide up the empirical world into classes which function in scientific theories. Classifying physical bodies according to their mass is appropriate because 'mass' functions in current physical theories. Classifying them into sub-lunar and super-lunar bodies is not because the theory in which that distinction made a difference has long since been abandoned.

What makes a physical element an "element" is determined by current versions of atomic theory. What makes a gene a "gene" is determined by current versions of transmission, developmental, and biochemical theories. Finally, what makes a species a "species" is determined by current versions of genetic and evolutionary theories. The reason that gene exchange and descent are important in defining 'species' is that these phenomena are supposed to be so central to the evolutionary process. If the selective retention of new genes is as important to the evolutionary process as most biologists think, then continuity from one generation to the next is required. If gene exchange is as important in maintaining evolutionary cohesiveness in sexual species as many biologists think it is, then it too must be acknowledged in any theoretically significant definition of 'species'. However, if 'species' is defined in this way, species can no longer be interpreted as genuine natural kinds. Nor can they be interpreted merely as numerical universals as Popper claims. Instead they take on every appearance of being spatiotemporal individuals.

Although this particular way of conceptualizing species has surfaced in the biological literature only quite recently ([26], [30], [46]), hints of this position can be found even earlier in the philosophical literature. For example, several biologists objected to John R. Gregg's [29] characterization of species as abstract sets. Instead, they argued, "species are composed of organisms just as organisms are composed of cells: according to this argument a species is just as much a concrete, spatiotemporal thing as is an individual organism, though it is of a less integrated, more spatiotemporally scattered sort." ([29], p. 425).[2] J.H. Woodger also noted that certain biologists seem to conceive of species as concrete entities, not abstract sets ([85], p. 21). Both Gregg and Woodger dismiss the notion out of hand.

Gasking is the first philosopher to pursue in any detail the possibility of treating species as individuals. He notes that morphospecies defined in terms of serial similarity pose no real problems, because no spatiotemporal connections are presupposed. A morphospecies is a "chain-cluster of forms, not one of individuals" ([25], p. 30). Even a biospecies defined in terms of serial crossability is not a spatiotem-

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porally localized individual because the organisms need be only potentially crossable with each other. They need not actually come into contact with each other. However, if actual descent and actual gene exchange are taken as the integrating relations and the entities being integrated are actual organisms (and not "forms"), then all sorts of problems arise. As Gasking notes, "Considered (wrongly) as a chain-cluster of individuals, a species is highly unstable. Old members are constantly dying and decaying, and so leave the cluster. New members are as constantly being added, so that every few months or years there is a complete turnover of membership."([25], p. 30).

Hence, Gasking concludes that species are not individuals the way that organisms are. However, if organisms are viewed as chain-clusters of cells, they are even more unstable than species. Old cells are constantly dying and leaving the cluster, while new members are constantly being added to it, so that every few days, months or years there is a complete turnover in membership. If Gasking's argument were sufficient to reject species as individuals, it would also be sufficient to reject organisms as individuals. But as it is, he takes the notion of species as individuals no more seriously than did Gregg and Woodger before him. Gasking does note, however, that if species are treated as individuals, their names would have to be treated as proper names ([25], pp. 15, 19). Gilmour also made a similar observation with respect to the taxonomic practice of giving a name to a species before classifying it, "implying that names of species are given, not on account of the possession of certain characters by the individuals concerned, but in the same arbitrary way as the christening of a baby."([28], p. 467). A generation later Ghiselin [26] would own up to this very implication of treating species as individuals.

More recently, J.J.C. Smart [77] rediscovered the spatiotemporal character of taxa as monophyletic units, arguing that taxa as monophyletic units lack the necessary generality to function in genuine laws of nature. Hence, statements like "Albinotic mice always breed true" cannot count as scientific laws. Hence, biology contains neither laws nor tightly-knit theories the way that physics does ([77], pp. 53, 59). Ruse's [71] solution is to abandon the principle of monophyly. In most cases, organisms breed only with similar organisms and give rise to equally similar offspring. When they do not, similarity must take precedence over such "biologically more significant" traits as gene exchange and transmission. In this way taxa can be treated as spatiotemporally unrestricted classes and statements which utilize them as laws of nature.

I have two objections to Ruse's solution. First, as long as descent and gene exchange are used in defining 'species', one can see why species tend to be made up of similar organisms. Statements like "All mammals have hair" seem to be more than just accidental correlations. Abandon such criteria, as Smart points out, and "now we have no reason to suppose our laws to be true."([77], p. 54). Second and more importantly, none of the major theories in biology refer to individual taxa anyway. Mendelian genetics refers to genes, dominance and recessiveness, epistatic genes, penetrance, etc. Mendel himself humbly referred to his so-called laws as if they applied only to Pisum. If they had, no one today would ever have heard of him. Similarly, evolutionary theory refers to such classes as dominant species, cosmopolitan species, peripheral isolates, and so on -- not to particular species. Hence, Smart's argument has no implications for the existence of biological theories, and laws and Ruse's abandoning monophyly is not necessary.[3] On Ruse's view, taxa are classes which might function in significant biological laws. In fact, they do not. Statements like "All swans are white", even if general and true are hardly central to any important biological theories.

In the face of such difficulties, certain biologists have suggested abandoning the species category altogether as not being important in the evolutionary process. All that really matters are genes [18]. Another alternative is to look for evolutionary regularities at higher levels of analysis, not at the level of particular taxa, but at the level of kinds of taxa. As Olding remarks, "Against Popper, then, I would urge that in attempting to formulate general evolutionary laws we do not mention individual animals, or even species, at all. The laws may well operate at a quite different level." ([56], p. 141).

I have pursued the implications of treating species as individuals at great length elsewhere ([37], [38], [39]), but one implication is worth discussing briefly. Spatiotemporal individuals have unique beginnings in time. When such an individual ceases to exist, that same individual cannot come into existence again. Although Hitler arose through the union of cells from two different organisms, he had a unique origin in time. By now we can safely conclude that he is dead and his cells completely deteriorated. Another human being may appear identical in every other respect to Hitler save origin, but Hitler cannot be born again.

Although I cannot pretend that species have been viewed by biologists in Darwin's day or today as individuals, or even that very many biologists were aware of the distinction between natural kinds and spatiotemporal individuals, periodically they can be found reasoning as if they viewed species as individuals. For example, soon after the appearance of the Origin, Charles Lyell wrote to Darwin laying down the following challenge. "I wish you could give the slightest reason why [Mammalia] should not begin more than once in more than one place. I incline to think it has not, but why?" ([84], p. 475). Darwin's response is instructive. "If every vertebrate were destroyed throughout the world, except our now well-established reptiles, millions of ages might elapse before reptiles could become highly developed on a scale equal to mammals; and, on the principle of inheritance, they would make some quite new class, and not mammals." ([17], pp. 136-137).

5. Conclusion

Darwin's theory introduced a radically different view of species, a view which has impressed itself upon biologists and non-biologists alike at a pace so leisurely that even a philosopher might be pushed to impa-

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tience. Whether Whewell and Mill saw the threat posed by gradually e-volving species to their own views concerning natural kinds is a moot question. Jevons did and reasoned that the conflict counted against biology, not traditional notions of natural kinds. Later philosophers opted for the other alternative. The existence of such clear-cut counter-examples to the traditional notion of natural kinds implied that this notion was inadequate and needed reformulation. The solution which was proposed was that species are natural kinds of a peculiar sort -- clusters. Taxa names can be defined but only as cluster concepts. The most recent interpretation of species as individuals and their names as proper names is only now making itself felt. Thus far, the interplay between professional philosophers and practicing biologists has been at best a mixed blessing.

Notes

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[2]The biologists whom Gregg ([29], p. 435) lists in his acknowledgments as having read and commented on his paper are Theodosius Dobzhansky, George Gaylord Simpson, A.J. Cain and Ernst Mayr, four of the most theoretically sophisticated biologists of the day. Gregg would have been wiser not to have dismissed their views so lightly.

[3]In response to the criticisms leveled at his early views, by Ruse [72] and Hull [37], Smart [78] has recently tempered his criticisms of biological theories.

References

[1] Beckner, M. The Biological Way of Thought. New York: Columbia University Press, 1959.

[2] Bock, W.J. "Nonvalidity of the 'Phylogenetic Fallacy.'" Systematic Zoology 18(1969): 111-115.

[3] ---------. "Philosophical Foundations of Classical Evolutionary Classification." Systematic Zoology 22(1973): 375-392.

[4] ---------. "Foundations and Methods of Evolutionary Classification." In Major Patterns in Vertebrate Evolution. Edited by Max K. Hecht, P.C. Goody and B.M. Hecht. New York: Plenum Publishing Corporation, 1977. Pages 851-895.

[5] Bonde, N. "Origin of 'Higher Groups': Viewpoints of Phylogenetic Systematics." In Problèmes Actuels de Paléontologie-Évolution des Vertébrés. Paris: Colloque Paléontologiques, No. 239, 1975. Pages 293-324.

[6] --------. "Cladistic Classification as Applied to Vertebrates." In Major Patterns in Vertebrate Evolution. Edited by Max Hecht, P.C. Goody and B.M. Hecht. New York: Plenum Publishing Corporation, 1977. Pages 741-804.

[7] Bridgman, P.W. The Logic of Modern Physics. New York: Macmillan Company, 1927.

[8] -------------. The Nature of Physical Theory. New York: Dover Publishing Company, 1936.

[9] Brundin, L. "Evolution, Causal Biology, and Classification." Zoologica Scripta 1(1972): 107-120.

[10] Colless, D. "The Phylogenetic Fallacy." Systematic Zoology 16(1967): 289-295.

[11] ----------. "The Interpretation of Hennig's 'Phylogenetic Systematics' -- A Reply to Dr. Schlee." Systematic Zoology 18(1969): 134-144.

[12] ----------. "The Phylogenetic Fallacy Revisited." Systematic Zoology 18(1969): 115-126.

[13] Cracraft, J. "Phylogenetic Models and Classification." Systematic Zoology 23(1974): 71-90.

[14] -----------. "Science, Philosophy, and Systematics." Systematic Zoology 27(1978): 213-215.

[15] Crowson, R.A. Classification and Biology. New York: Atherton Press, 1970.

[16] Darwin, C. On the Origin of Species, A Facsimile of the First Edition (1859). Cambridge, Mass.: Harvard University Press, 1966.

[17] Darwin, F. (ed.). The Life and Letters of Charles Darwin. New York: D. Appleton and Company, 1899.

[18] Dawkins, R. The Selfish Gene. New York: Oxford University Press, 1976.

[19] Eldredge, N. "Cladism and Common Sense." In Phylogenetic Analysis and Paleontology. Edited by J. Cracraft and N. Eldredge. New York: Columbia University Press, 1979. Pages 165-197

[20] ----------- and S.J. Gould. "Punctuated Equilibria: An Alternative Model to Phyletic Gradualism." In Models in Paleobiology. Edited by T.J.M. Schopf. San Francisco: Freeman, Cooper and Company, 1972. Pages 82-115.

[21] Enc, B. "Spiral Dependence between Theories and Taxonomy." Inquiry 19(1974): 41-71.

[22] Engelmann, G.F. and Wiley, E.O. "The Place of Ancestor-Descendant Relationships in Phylogeny Reconstruction." Systematic Zoology 26(1977): 1-11.

[23] Farris, S.J. "A Methodological Solution to a Philosophical Problem." Paper read at the 1978 meeting of the Philosophy of Science Association.

[24] Gaffney, E.S. "An Introduction to the Logic of Phylogeny Reconstruction." In Phylogenetic Analysis and Paleontology. Edited by J. Cracraft and N. Eldredge. New York: Columbia University Press, 1979. Pages 79-111

[25] Gasking, D. "Clusters." Australasian Review of Psychology 38(1960): 1-36.

[26] Ghiselin, M. "A Radical Solution to the Species Problem." Systematic Zoology 23(1974): 536-544.

[27] Gilmour, J.S.L. "A Taxonomic Problem." Nature 139(1937): 1040-1042.

[28] --------------. "Taxonomy and Philosophy." In The New Systematics. Edited by J. Huxley. Oxford: Oxford University Press, 1940. Pages 461-474.

[29] Gregg, J.R. "Taxonomy, Language, and Reality." American Naturalist 84(1950): 421-433

[30] Griffiths, G.C.D. "On the Foundations of Biological Systematics." Acta Biotheoretica 23(1974): 85-131.

[31] Hempel, C.G. Aspects of Scientific Explanation. New York: The Free Press, 1965.

[32] Hennig, W. Grundzüge einer Theorie der phylogenetischen Systematik. Berlin: Deutscher Zentralverlag, 1950.

[33] ---------. Phylogenetic Systematics. Chicago: University of Illinois Press, 1966.

[34] Hull, D.L. "The Effect of Essentialism on Taxonomy." The British Journal for the Philosophy of Science 15, 16 (1965): 314-326, 1-18.

[35] ---------. "Certainty and Circularity in Evolutionary Taxonomy." Evolution 21(1967): 174-189.

[36] ---------. "The Operational Imperative -- Sense and Nonsense in Operationism." Systematic Zoology 17(1968): 438-459.

[37] ---------. Philosophy of Biological Science. Englewood Cliffs, New Jersey: Prentice-Hall.

[38] ---------. "Are Species Really Individuals?" Systematic Zoology 25(1976): 174-191.

[39] ---------. "A Matter of Individuality." Philosophy of Science 45(1978): 335-360.

[40] ---------. "Planck's Principle." Science 202 (1978): 717-723.

[41] ---------. "The Limits of Cladism." Systematic Zoology 28(1979): 416-440.

[42] --------. "The Units of Evolution." In The Philosophy of Evolution. Edited by U.J. Jensen and R. Harré. New York: St. Martin's Press, 1981.

[43] Jevons, W.S. The Principles of Science (1892). New York: Dover Publications, 1958.

[44] Kitts, D.B. "Karl Popper, Verifiability, and Systematic Zoology." Systematic Zoology 26(1977): 185-194.

[45] ----------. "Theoretics and Systematics: A Reply to Cracraft, Nelson, and Patterson." Systematic Zoology 27(1978): 222-224.

[46] Löther, R. Die Beherrschung der Mannigfaltigkeit. Jena: Gustav Fischer, 1972.

[47] Mayr, E. Principles of Systematics. New York: McGraw-Hill, 1969.

[48] -------. "Evolution." Scientific American 239 #3(1978): 46-55.

[49] Mill, J.S. A System of Logic. 8th ed. London: Longmans, Green, and Company, 1872.

[50] ---------. Three Essays on Religion (1874). New York: The Liberal Arts Press, 1958.

[51] Nelson, G. "'Cladism' as a Philosophy of Classification." Systematic Zoology 20(1971): 373-376.

[52] ---------. "Comments on Hennig's 'Phylogenetic Systematics' and Its Influence on Ichthyology." Systematic Zoology 21 (1972): 364-374.

[53] ---------. "Classification as an Expression of Phylogenetic Relationship." Systematic Zoology 22(1973): 344-359.

[54] ---------. "Classification and Prediction: A Reply to Kitts." Systematic Zoology 27(1978): 216-218.

[55] ---------. "Cladistic Analysis and Synthesis: Principles and Definitions." Systematic Zoology 28(1979): 1-21.

[56] Olding, A. "A Defence of Evolutionary Laws." The British Journal for the Philosophy of Science 29(1978): 131-143.

[57] Patterson, C. "Verifiability in Systematics." Systematic Zoology 27(1978): 218-221.

[58] Platnick, N. "Cladograms, Phylogenetic Trees, and Hypothesis Testing." Systematic Zoology (1977): 438-442.

[59] ------------ and Cameron, H.D. "Cladistic Methods in Textual, Linguistic, and Phylogenetic Analysis." Systematic Zoology 26(1977): 380-385.

[60] ------------ and Gaffney, E. "Systematics: A Popperian Perspective." Systematic Zoology 26(1977): 360-365.

[61] ------------ and Gaffney, E. "Evolutionary Biology: A Popperian Approach." Systematic Zoology 27(1978): 137-141.

[62] ------------ and Nelson, G. "The Purposes of Biological Classification." In PSA 1978, Volume 2. Edited by P.D. Asquith and I. Hacking. East Lansing, Michigan: Philosophy of Science Association, 1981. Pages 117-129.

[63] Popper, K.R. The Open Society and Its Enemies. London: G. Routledge and Sons, 1945.

[64] ----------. The Poverty of Historicism. London: Routledge and Kegan Paul, 1957.

[65] ----------. The Logic of Scientific Discovery. New York: Basic Books, 1959.

[66] ----------. Conjectures and Refutations. New York: Basic Books, 1962.

[67] ----------. Objective Knowledge. Oxford: At the Clarendon Press, 1972.

[68] ----------. Unended Quest: An Intellectual Autobiography. LaSalle, Illinois: Open Court Press, 1976.

[69] Pratt, V. "Numerical Taxonomy." Journal of Theoretical Biology 36(1972): 581-592.

[70] Ruse, M. "Definitions of Species in Biology." The British Journal for the Philosophy of Science 20(1969): 97-119.

[71] -------. "Are There Laws of Biology?" Australasian Journal of Philosophy 48(1970): 234-246.

[72] -------. The Philosophy of Biology. London: Hutchinson University Press, 1973.

[73] -------. "The Scientific Methodology of William Whewell." Centaurus 20(1976): 227-257.

[74] -------. "Karl Popper's Philosophy of Biology." Philosophy of Science 44(1977): 638-661.

[75] Schlee, D. "The Interpretation of Hennig's Systematics, an 'Intuitive, Statistico-Phenetic Taxonomy'?" Systematic Zoology 18 (1969): 127-134.

[76] Simpson, G.G. Principles of Animal Taxonomy. New York: Columbia University Press, 1969.

[77] Smart, J.J.C. Philosophy and Scientific Realism. London: Routledge and Kegan Paul, 1963.

[78] -----------. "Under the Form of Eternity." The Walter and Eliza Hall Institute of Medical Research Annual Review (1976-7): 36-42.

[79] Sneath, P.H.A. and Sokal, R.R. Numerical Taxonomy. San Francisco: W.H. Freeman and Company, 1973.

[80] Sokal, R.R. and Sneath, P.H.A. The Principles of Numerical Taxonomy. San Francisco: W.H. Freeman and Company, 1963.

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192466 Published online by Cambridge University Press

[81] Whewell, W. The Philosophy of the Inductive Sciences, Vol. I. London: John Parker, 1847.

[82] ----------. The Philosophy of the Inductive Sciences, Vol. II. London: John Parker, 1847.

[83] Wiley, E.O. "The Evolutionary Species Concept Reconsidered." Systematic Zoology 27(1978): 17-26.

[84] Wilson, L.G. Sir Charles Lyell's Scientific Journals on the Species Question. New Haven: Yale University Press, 1970.

[85] Woodger, J.H. "From Biology to Mathematics." The British Journal for the Philosophy of Science 3(1952): 1-21.

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192466 Published online by Cambridge University Press