

Biological Species: Natural Kinds, Individuals, or What?

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What are biological species? Aristotelians and Lockean agree that they are natural kinds; but, evolutionary theory shows that neither traditional philosophical approach is truly adequate. Recently, Michael Ghiselin and David Hull have argued that species are individuals. This claim is shown to be against the spirit of much modern biology. It is concluded that species are natural kinds of a sort, and that any 'objectivity' they possess comes from their being at the focus of a consilience of inductions.

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The status of biological species continues to attract attention and controversy. (See, for instance, Eldredge and Cracraft [1980]; Gould [1979]; Grant [1981a]; Levin [1979]; Mayr [1982]; Wiley [1978, 1980]; Splitter [1982]; Mishler and Donoghue [1982]; Holsinger [1984]; Kitcher [1984]; Eldredge [1985].) There is a strong feeling among biologists, at least there is a strong feeling among most zoologists and somewhat less of one among botanists (a difference to be discussed later), that species are somehow different from the other groupings of organisms we find (or make) in nature. Species, like *Drosophila melanogaster* or *Canis lupus*, are thought to be 'natural', in some way objective or existing independently of the classifier. In this, species differ from the groups (taxa) found at other ranks, for instance that of the genus. The classifier's own thoughts and aims have a much greater role to play in the delimiting of members of these other groups.

But wherein lies the naturalness of species? With the coming of evolutionary theory, traditional answers seem less than adequate. Recently, in a brilliantly innovative move, the biologist Michael Ghiselin [1966, 1969, 1974a,b, 1981; 1987], supported by the philosopher David Hull [1975, 1976, 1978, 1979, 1981], has argued that evolution shows us to have mis-

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construed the nature of species. They are not groups or classes of organisms, like hockey players on a team. Rather, they are integrated *individuals*, with organisms having the relationship to their species of part to whole rather than member to class. And thus properly seen, argue Ghiselin and Hull, the specialness or naturalness of species is self-evident. Species are natural or real in the way any biological individual is natural or real.

In this essay, I argue that, stimulating though the species-as-individuals (s-a-i) thesis may be, it runs counter to much accepted biological thinking, as well as to logic. We must rely on more traditional conceptual tools to establish the naturalness of species. But, with some exceptions and qualifications, this can be done, and the very exceptions and qualifications themselves establish the correctness of the overall approach.

I BIOLOGISTS ON SPECIES

Let us start with what biologists have to say about species. The most interesting fact is that the category of species can be and is characterised in so many different ways, with corresponding ways of defining particular taxa names. Here, four major species concepts will suffice. (See Mayr [1982] and Grant [1981a] for recent discussions of the multiplicity of species concepts.)

First, we have the most obvious and intuitive concept of all. We find the organic world broken up into groups of similar looking organisms, with gaps between the groups. The concept thus refers to some notion of overall similarity of appearance possessed by organisms within species taxa. In Charles Darwin's words, a species is 'a set of individuals closely resembling each other' (Darwin [1859], p. 52).

Physical nature or *morphology* is the key to this species concept, and it is therefore invoked when one deals with particular species taxa. To be a member of *Homo sapiens* you must be relatively hairless, capable of walking upright, with a large brain, and so forth. In fact, today it is recognised that invariably there is diversity, even within such morphologically delimited groups, so biologists frequently use polytypic or polythetic definitions: lists of features, a combination of which is sufficient for species membership, but no one of which is necessary (Beckner [1959]; Simpson [1961]; Hull [1965]).

Next, we have a concept which has, perhaps a little strongly, appropriated unto itself the title of *biological* species concept. This refers to breeding, or the lack of it. One well-known formulation, due to Ernst Mayr [1942], states that species are 'groups of actually or potentially interbreeding natural populations which are reproductively isolated from other such groups'. You do not normally find taxa name definitions using this concept, but I do not see in principle why not. Specify some individual, say Brigham Young, as your reference point, and then members of the same taxon are potential or actual interbreeders, with some obvious qualifications to take

account of sex, and so forth. (See Mayr [1982] for his attempts to give a more refined version of the concept.)

Third, we have a concept which deliberately refers to *evolution*. In the words of the paleontologist G. G. Simpson [1961]: 'An evolutionary species is a lineage (an ancestral-descendant sequence of populations) evolving separately from others and with its own unitary evolutionary role and tendencies.' A taxon name would get a related definition. If we suppose that humans first appeared about 1/2 million years ago, *Homo sapiens* is the name for the group which has descended from the original organisms. (This certainly seems to be the kind of definition that paleoanthropologists have in mind. See Johanson and Edey [1981]; Johanson and White [1979].)

Fourth and finally, we have a concept which does for the world of genes, what the first concept did for morphology. The category concept refers to overall *genetic* similarity clusterings, such clusterings, being separated from others by gaps. Mayr [1969] again: 'When an evolutionary taxonomist speaks of the relationship of various taxa, he is quite right in thinking in terms of genetic similarity, rather than in terms of genealogy.' A particular species name would be defined in terms of genes held in common (together perhaps with information about chromosomes, structure and so forth). For obvious reasons, you do not often see definitions of this ilk, but they do exist. With increasingly sophisticated methods of analysing genomes, their similarities and differences, we might expect to see more such definitions.

There are other concepts which could be and sometimes are invoked, for instance concepts based on ecology (Van Valen [1976]). But, we have enough for our purposes. The question to be asked now is why a taxon which falls into a category characterised in one of the above ways should be thought natural or real in some sense. Since this is a question *about* science, rather than *within* it, we turn to philosophy for guidance.

2 NATURAL KINDS

Traditionally, philosophers have treated the status of species as being part and parcel of a larger question about the reality of 'natural kinds'. Why do we think the whole physical world to be divided into different sorts of things: gold, water, stars, as well as *Homo sapiens* and *Drosophila melanogaster*?

Roughly speaking, there have been two main answers to this question (Ayers [1981]).¹ Credit for the first is given to *Aristotle*. He argued that the

¹ In my view, most of the modern supporters of natural kinds end up somewhere to the right of Aristotle (e.g. Kripke [1972]; Putnam [1975]; Wiggins [1980]). Frankly, I am not sure how far these modern thinkers really intend their ideas to apply to biology, since they generally do not bother to refer to the works of practising taxonomists, and at times show an almost proud ignorance of the organic world. Any comments I have to make against Aristotle apply equally against them. Dupré [1981] shows how ignorant most modern philosophical thinkers are about biological reality.

world—at least, the world of scientific inquiry—is made up of substances. Any particular substance, like a sample of gold or an individual man, results from the interaction between the substance's underlying matter and its form. This latter gives a substance its nature or essence. Objects of the same kind, like two men, are the same because they have the same form, which is embedded in different samples of matter. Substances have their form essentially, that is to say, one cannot be a substance of a particular kind without having the required form.

Crucial to the Aristotelian position is the distinction between a 'real definition' and a 'nominal definition'. The former enables you to define a natural kind name, including the name of an organic species, in terms of attributes which stem necessarily from the very essence of a substance (Aristotle called these attributes 'properties'). Thus, in the case of *Homo sapiens* the essence involves the notion of rationality. Unpacking, we get such properties as the power of speech. A real definition would consequently refer to this power. However, not all attributes of an individual stem from the essence. There are features which are possessed 'accidentally'. Although these features are non-essential, it might nevertheless be possible to distinguish a natural kind using only accidents. In the case of humans, both bipedality and featherlessness are accidents, and yet it so happens that the set of featherless bipeds is one and the same as the set of rational animals. Any characterisation in terms of accidents yields a 'nominal definition'.

The great rival to Aristotle's analysis came in the seventeenth century, from the pen of *John Locke*. He argued that reality lies in the underlying particles which go to make up any particular substance. Locke himself was, in fact, doubtful that we could ever truly know these basic units. But, any real definition would have to make reference to these building blocks, specifically to their shape, structure, motion, composition, and so forth. Surface definitions are simply marks of the structure beneath. Consequently, any surface definition could never be more than nominal. For Locke, a definition of humans in terms of rationality has no more and no less status than a definition in terms of bipedality.

But what of the underlying real structure? Even here Locke wanted to deny the absoluteness of Aristotelian essences. Shapes and so forth can change, taking a substance from one kind to another. Not to mention borderline cases:

There are Animals so near of kind both to Birds and Beasts, that they are in the Middle between both ... There are some Brutes, that seem to have as much Knowledge and Reason, as some that are called Men ... and so on till we come to the lowest and the most inorganical parts of Matter, we shall find everywhere, that the several Species are linked together, and differ but in almost insensible degrees (Locke [1975], III, vi, 12).

Hence, ultimately for Locke, even definition in terms of reality involves a conscious decision to divide. A Lockean definition is therefore never more than what an Aristotelian would label 'nominal'. Any difference between

men and changelings 'is only known to us, by their agreement, or disagreement with the complex *idea* that the name *Man* stands for' (Locke [1975], III, vi, 39).

In short, whereas for Aristotle natural kinds are ontological entities, for Locke they are at best epistemological concepts. You have the objective approach, *versus* the subjective approach. The approach which *finds* natural kinds, and the approach which *makes* them.

3 BUT ARE BIOLOGICAL SPECIES REALLY NATURAL KINDS?

Let us now try to put biology and philosophy together. Does either Aristotle or Locke capture the biologist's sense that species are real or natural?

The simple answer is that, as they stand, neither does. Take Aristotle. He would argue that species are real, because they are natural kinds. But, how then could one get a real definition? The morphological approach to taxa will not do, because if there is one thing that modern biology teaches, it is that evolution promotes morphological diversity. Species members are not all the same. Hence the need for polytypic definitions. But, for something to be an Aristotelian property (as opposed to an accident), it must be possessed by every member of the kind, and distinguishing the group from others. Polytypic definitions are not enough. Hence, morphology will not do. (Mayr [1963]; Dobzhansky [1970]; Dobzhansky *et al.* [1977], discuss variation within species.)

The same considerations apply to genetic features. Evolution promotes genetic diversity (Lewontin [1974]). And, similarly, the features relied on by other approaches to species fail the Aristotelian. Is the sterile worker ant even a potential interbreeder, and would one really want to say that an entirely artificially produced fruit-fly could never really be in *Drosophila melanogaster*?

In any case, after Darwin, strict Aristotelianism simply will not work (Hull [1965]; Mayr [1969, 1982]). Evolution says that you can take virtually any property you like, and if you go back (or forwards) enough in time then ancestors (descendants) did not (will not) have it. But, this is just what Aristotle cannot handle. The whole point about a natural kind is that its properties exist in perpetuity, like mathematical objects. And clearly such properties have to be passed on by (to) ancestors (descendants). Evolution denies this.

What about Locke? Initially, things seem very much more promising. Morphological criteria give you nominal definitions of species names. Alternatively, if you favour phylogenies, you use relationships of descent—something which Locke himself, incidentally, was not that enamoured with. ('[M]ust I go to the Indies to see the Sire and Dam of the one, and the Plant from which the seed was gather'd, that produced the other, to know whether this be a Tiger or that Tea', Locke [1975], III, vi, 23.) Then, genetic criteria give you the closest things you can get to real definitions.

And, contrary to Locke's own doubts, we can know quite a bit about these. Nary an issue of *Science* or *Nature* appears without fresh details of the genetic structure of some organism.

One often finds versions of a neo-Lockean proposal in the literature (for instance, Mayr [1963, 1969]). Unfortunately, you purchase your solution to the species problem at too high a price. You have to relinquish claims to the ultimate objectivity or reality of species. A Lockean natural kind is essentially subjective or arbitrary. And that is just what you do not want to concede, when it comes to species. In some sense, species are real!

We are caught in a dilemma. Evolution refutes Aristotelianism; but, Lockeanism is inadequate. According to Ghiselin and Hull, and an increasingly large number of supporters, we must go back to biology. When we do this, we see that Aristotle and Locke share a false premise. Reject this premise, and hope rises for a solution to the species problem. (Sympathisers with Ghiselin and Hull include Mayr [1976]; Wiley [1978]; Rosenberg [1980]; Sober [1980]; Beatty [1982]; Splitter [1982]; Eldredge and Cracraft [1980]. Intimations of the Ghiselin/Hull approach are to be found in Theodosius Dobzhansky's classic, *Genetics and the Origin of Species*.)

4 SPECIES AS INDIVIDUALS

Aristotle and Locke agree that species are natural kinds. The taxon *Homo sapiens* is a class, with individual humans like Michael Ruse and Charles Darwin as members. I qualify for membership in the class *Homo sapiens*, because I possess certain attributes, whatever they may be. So does Charles Darwin. My dog Spencer does not have these properties, and thus does not qualify. He has his own species, *Canis familiaris*.

Ghiselin and Hull argue that species are not natural kinds at all: They are not classes with members. Rather, species are *individuals*, just as particular organisms are individuals. Hence, just as the relationship between my arm and myself is one of part to whole, rather than member to class, so my relationship to the species *Homo sapiens* is one of part to whole. I, and Charles Darwin, are parts of the human species, just as Spencer is part of the species *Canis familiaris*.

The reformers argue that, once we see species in the true light, all of the problems about species start to fade. Of course, species are real. No one doubts the reality of Michael Ruse, or of Spencer. They are individuals. Real things. Part of the furniture of our world. So are species.

What kind of claim is the s-a-i thesis? It is not solely or even primarily an empirical claim. 'Look! There's an individual!' Rather, it is more of a conceptual claim, whose plausibility must be argued for. Consider a chess-board. You can think of this as an individual, made up of 64 parts, or as a class of 64 squares. It depends on your perspective as to which makes more sense—are you making chess-boards, or are you teaching someone the rules of chess. *The crux of the s-a-i thesis, therefore, is whether modern evolutionary*

biology inclines one to treat species more as individuals, or more as classes, as natural kinds.

There is of course the initial question as to what precisely one means by an 'individual'. Ghiselin and Hull point to the fact that, whatever else an individual may or may not be, we recognise organisms as paradigm examples of individuals. Organisms are not just diffuse, artificially created entities. They are integrated beings. They have internal organisation. Hence, if we can show that, in important respects, species are like organisms, we can reasonably say that species are individuals.

But, claim Ghiselin and Hull, from an evolutionary perspective species have the very marks of individuality that organisms have. Just like organisms, species come into being, exist for a period in space and time, and then go. And, they have at least some sense of organisation. As the leading evolutionist Ernst Mayr said (before he himself was converted to the s-a-i thesis): '*Species are the real units of evolution*, they are the entities which specialize, which become adapted, or which shift their adaptation' (Hull [1976], p. 183, quoting Mayr [1969]).

That evolutionary theory treats species as individuals becomes clear when we look at their uniqueness. Adolf Hitler was an individual—as such (unlike his diaries) he came uniquely and went uniquely. A copy of Adolf Hitler is not he. It cannot be. Similarly, we have this uniqueness for species. 'If a species evolved which was identical to a species of extinct pterodactyl save origin, it would still be a new, distinct species' (Hull [1978], p. 349). If you are a species, you simply cannot be born again, any more than Adolf Hitler can be.

What about change? Organisms can undergo major change, and still be the same organism. What counts is continuity. The limits of an organism are birth and death. The same is true of species. 'There is no limit to the genetic change that can take place in a species or population before it becomes extinct or speciates' (Hull [1976], p. 182). In fact, just like an organism, so long as the continuity persists, we have the same species.

Finally, let us mention one revealing point. Biologists take one specimen from a species, using it as the marker. The species name (which, as with all individuals is a proper name) is attached to this marker—the type specimen—by an act which is akin to baptism. This specimen does not have to be a 'typical' member of the species, whatever that might mean. And, of course it does not. The type is part of the whole, not a member in the class.

The fact that any specimen, no matter how atypical, can function as the type-specimen makes no sense on the class interpretation; it makes admirably good sense if species are interpreted as individuals (Hull [1976], 175).

All in all, whatever 'common sense' may say, modern evolutionary biology demands that species be regarded as individuals. Hence, the naturalness of species.

5 WHY SPECIES ARE NOT INDIVIDUALS: BIOLOGICAL OBJECTIONS

Ingenious though it is, the Ghiselin/Hull attempt to slice through the Gordian knot constraining the species problem fails. There are several significant reasons why species cannot properly be considered as individuals.¹

First, look at matters at the most basic biological level. We think organisms are individuals because the parts are all joined together. Charles Darwin's head was joined to Charles Darwin's trunk. But, in the case of species, this is not so. Charles Darwin was never linked up to Thomas Henry Huxley. Of course, you might object that although Darwin's head was never linked directly to his feet, they were linked indirectly through intermediate parts. Analogously, as evolutionists presumably we believe that Darwin and Huxley were linked by actual physical entities (namely, the succession of humans back to their shared ancestors). But, this objection fails, for the point is that these links have now been broken and lost. If (gruesome thought!) Darwin's head were physically severed from his feet, we would certainly have no biological individual.

Yet, with justice, Ghiselin and Hull will respond that these speculations are beside the point. The required condition for individuality is not mere spatio-temporal contiguity. It is rather some sort of internal integration or organisation. Because of such internal organisation, the U.S. is one country, even though Alaska and Hawaii do not touch any of the other states. The fifty states work together, in a way that (say) the forty-eight mainland states together with Ontario and Quebec do not. Analogously, Charles Darwin is an individual, not because of spatio-temporal contiguity, but because his parts are organised, working *together*. The same is true of other biological entities, even those which break into parts at some points in their life cycles, like slime molds. *And*, the same is true of species. They have an integrating organisation, with the parts contributing to the whole.²

But, this will not do, at least not in the light of much modern thought about the working of evolution. First and foremost, thinking of a species as an integrated individual goes flatly in the face of the way in which the major evolutionary mechanism of natural selection is generally regarded today. Selection leads to adaptations, features which help organisms in life's struggles for survival and reproduction. But who precisely benefits from

¹ Other critics of the s-a-i thesis include Caplan [1980, 1981] and Kitts and Kitts [1979]. Unfortunately, these critics revert to a modern-day, genetic, Aristotelian essentialism. I find myself agreeing with much in Hull's [1981] spirited response to them.

² Could spatio-temporal contiguity alone count as the criterion of individuality? We surely think of the planet Earth as an individual on these grounds. But, while this may be true, we do not think of Earth as a *biological* individual, which notion is the focus of the s-a-i thesis. Incidentally, however, given plate-tectonic theory and the consequent claims about Earth's organisation, a case might be made for Earth's geological individuality, transcending mere spatio-temporal contiguity.

adaptations? Is it the possessors alone, or do others benefit? In short, at what level of biological organisation does selection work? Is it between individuals, benefiting individuals, or is it between higher entities like species, benefiting species taken as a whole.

Until recently, most people casually assumed that selection could work at virtually every level of biological organisation. In particular, one could have selection between groups of organisms, including between species. The units of selection, in vital respects, were species. As Mayr (1969) said: 'Species are ... the entities ... which become adapted.' However, majority opinion today is that selection just does not work in this way. As Charles Darwin himself argued, ardently, selection works chiefly if not exclusively at least at the level of the individual organism. 'Group selection' at the level of the species does not work. A species is not adapted. An organism (or, at most, a limited number of organisms) is. Any species effects are just epiphenomena on individual effects, or at most, on population effects. (See Brandon and Burian [1984], for a review of this topic, and Ruse [1980] for Darwin's views on the subject.)

If this is all so, then there is something very odd indeed about speaking of a species as an individual. It is very far from being an integrated unit like an organism. The individual organisms of a species are all working for their own benefits, against those of others. Any species cooperation, any species integration, is secondary on the particular organism's self-interests. And, in any case, one is hardly likely to get species-wide secondary effects. Cooperation will, at most, be between relatives, or fellow population members. Generally, selection pits organisms against each other (although not necessarily in a crude 'nature red in tooth and claw' fashion).¹

Individual selection and the s-a-i thesis simply do not go together. What about obvious counters? Some biologists believe that group selection can work. This is true, but hardly makes the s-a-i-thesis again compelling. Group selection supporters think it works for populations, not species, and no one denies the importance of individual selection. (See Wilson 1975 and Wade 1978 for recent views on group selection.) Conversely, some biologists argue that the true 'individual' in individual selection is the gene, not the whole organism (Dawkins 1976). Does not my argument prove too much, suggesting that organisms should not be considered true individuals—which is clearly absurd? But, while this point does show that for some biologists the level of individuality does not necessarily stop at the whole organism level, no one denies that organisms (thanks to selection) are sufficiently well organised to be considered individuals in their own right. Richard Dawkins [1982], for instance, speaks of organisms as 'vehicles'

¹ One does get cooperation between organisms. But, the point is that ultimately, biology regards it as 'enlightened self-interest'. Hence, at root we have tensions—separate reproductive strategies—between organisms. At times, for instance where mates are involved, these tensions break right out. See Trivers [1971]; Wilson [1975]; Barash [1977]; Clutton Brock [1982]; and Ruse [1979b], for more details.

which carry within them the units of selection, 'replicators' or genes. An organism, to such a biologist, is no less an individual than a BMW is to a racing driver.

Continuing with biological counters to the above critique, what about Steven Stanley's (1979) notion of species selection, where it is suggested that trends are a function of the success or failure of species? Again there is little help for the s-a-i thesis. Even if one accepts species selection, and many would not, the key operation of natural selection is with the individual. Drawing attention to the trends that one often sees in the fossil record, Stanley suggests that there is nevertheless a randomness about the members of new species with respect to a trend. Although a trend may (say) be from smaller to bigger, a new species in the line of descent could well have small-bodied members. (No doubt, if one persisted, one could devise some form of species selection where the group as a whole was significant. But its realisation in nature is obviously another matter. See Arnold and Frisrup [1982].)

Perhaps the strongest biological case for the s-a-i thesis comes through the notion of a species as a number of organisms sharing a common 'gene pool', with shared types of genes being passed on to common ancestors (*i.e.* a kind of hybrid notion formed from several of the species concepts. Dobzhansky [1970]; Dobzhansky *et al.* [1977]). Here you might think we have the kind of integration required for individuality. Certain genes flow between the organisms of a species, and between no others. But, this hardly denies the key importance of individual selection. Moreover, there are today strong questions about the biological importance, at the species level, of such genetic sharing. It was once thought that gene flow, between populations, is a key factor in keeping the organisms of a species alike. Now, it seems more likely that normalising selection is the key causal factor. Species members sit on the top of the same 'adaptive peak'—if they vary too much from the species norm, then selection wipes them out.

This downgrading of the significance of gene flow is a most important point, because (being itself one which comes from modern evolutionary theorising) it strikes right at the heart of the claim that the s-a-i thesis (however counter-intuitive it may seem) must be accepted on the basis of modern biology. John Endler's [1977] already-classic study brings both theoretical and empirical evidence to bear demonstrating the restricted effects of gene flow. Basically, gene flow would be expected due to migration ('the relatively long-distance movements made by large numbers of individuals in approximately the same direction at approximately the same time', p. 182) and dispersal ('the roughly random and nondirectional small-scale movements made by individuals rather than groups, continuously, rather than periodically, as a result of their daily activities', p. 181). One would expect that migration would be a most effective way of uniting a species, even if widely dispersed; but, as Endler points out, this is rarely so, since migration is usually

accompanied by return migration and organisms give birth in the place where they were themselves born. Such 'philopatry' has obvious adaptive virtues—birds may migrate to winter feeding grounds but they return to already-established and proven breeding grounds.

Dispersal is *a priori* a less promising way in which gene flow might be greatly effective, and there are a number of reasons why its importance should not be over-estimated: ethological, ecological, and physiological inadequacy of hybrids between distant species members; random loss of new gene forms because they are rare; infrequency of long-distance travelers (as opposed to migrators); and more (pp. 28–9). All in all, therefore, one should not over-emphasise the unity of the species because of the supposed circulation of shared genes. (Similar points are made by Grant [1981a,b]; Levin [1981]; and Mishler and Donoghue [1982]. Although the point just made applies to both animals and plants, since the latter are spatially more fixed, expectedly the 'genetic integration' argument has always seemed less plausible to botanists.)

So, once again we come back to the individual organism and to its response to the environment (including fellow species members). If you take Darwinian selection seriously, you simply must reject the s–a–i thesis. Note that I am certainly not denying that the members of a species are frequently 'united' in having similar causal pressures, whether these be genetic or selective or whatever. Of course they do. That is what makes them part of the same species. The question is whether this 'unification' is significantly more than similar causes. The s–a–i-supporter has to say that it is, even to the point of the kind of integration we find in individual organisms, and this is what I deny. (Caplan [1980], rightly emphasised that same causes lead to same effects, and this accounts for species members being similar.)

6 WHY SPECIES ARE NOT INDIVIDUALS: CONCEPTUAL OBJECTIONS

Let us move on to more conceptual-type objections to the s–a–i thesis. Crucial to the thesis is the claim that, logically, a species can appear only once. If it dies, that is it. In this, species are just like paradigm individuals organisms. Adolf Hitler cannot be resurrected. Neither can extinct species of pterodactyl.

I will leave Christians to fight their own battles about human bodily resurrection. As far as species are concerned, time and technology have shown the s–a–i claim wrong. Today, through recombinant DNA techniques and the like, biologists are rushing to make new life forms. Significantly, for commercial reasons the scientists and their sponsors are busily applying for patents protecting the new creations. Were the origins of organisms things which uniquely separate and distinguish them, such protections would hardly be necessary. Old life form and new life form

would necessarily be distinct. Since apparently they are not, this suggests that origins do not have the status claimed by the s-a-i boosters. (See Wade [1979, 1980a,b].)

Relatedly against the s-a-i thesis, in crucial respects it seems that it really does not treat species and their organisms all that very differently from the old way of treating of species as classes, with members included according to the possession of certain required properties. Take an organism. How do you know that my hand is part of the individual, Michael Ruse? Because it is joined on—that's why! But my dog Spencer certainly is not joined on, in the same way, to the species *Canis familiaris*. So why do we want to say that he is part of the species? Because he descended from the original ancestors, along with the rest of the group—that's why!

Descent is starting to look very much like an essential property. Spencer is part of the group *Canis familiaris*. Indeed, we even seem to have a real/nominal distinction at work here. In Spencer's case, if challenged about his status, I can in fact produce papers attesting to parentage. But in the case of the other four-legged being that lives in my house, I have no such documentary evidence about origins. And yet, I am as sure that Sesame is a cat, as that Spencer is a dog. Why? Because she looks and behaves like one. She miaows, purrs, keeps aloof, jumps from heights, stays up half the night, and is fastidiously clean. In short, she has all the identifying marks of cattiness (*Felis domestica*).

Clearly what I, and everyone else, am doing here is employing morphological and related criteria. Of course, Ghiselin and Hull recognise and appreciate the use of such criteria. They simply refuse to give such use any significant theoretical status (see Ghiselin [1981]). What can this all mean, but that one is using nominal criteria, because real essence's descent relationships are unknown? Hence, for all the talk, the s-a-i thesis treats species as classes, with descent giving real essence and with morphology giving nominal definitions.

There are other objections you can raise against the s-a-i thesis. One is that it has controversial implications about the temporal limits of a species, and the possible evolutionary change that such a unit can encompass. So long as one gets no new group breaking off from an evolving lineage, one has one and only one species, whatever the change (just as one has one and only one organism, despite the change from caterpillar to butterfly). The cladistic school of taxonomy would accept this implication, but most biologists would not. They distinguish, for instance, between *Homo habilis*, *H. erectus*, and *H. sapiens*, despite the lack of branching. *Homo habilis* had a brain size of around 700 cc, much closer to that of a gorilla (*Gorilla gorilla*) at 500 cc, than to modern man at 1400 cc. Hence, species divisions are made—divisions which cladists and s-a-i supporters must ignore. (Cladism is discussed well in Eldredge and Cracraft [1980], and Wiley [1981].)

Indeed, the s-a-i thesis is more extreme than cladism. Cladists end a species as soon as there is any branching within the group. But, if just a

small population broke from a parent species, leaving the parent unaffected, one would have a situation very similar to an asexual organism budding off a small part. The parent remains. Similarly, the s-a-i thesis would have to count an analogous parent species the same original individual. Carried to the extreme, classification could become very difficult indeed.¹

And, finally, let me point to an implication which has rather drastic consequences for the social sciences. Laws of nature generally do not refer to specific spatio-temporally bounded objects. They are rather 'timeless regularities in nature'. But, if species are individuals, then any claims about the organism of species, restricted to the species, cannot be laws. In particular, any claims exclusively about human beings cannot possibly be laws. Hence, at one stroke, the social sciences, as they stand today, cease to be sciences in any worthwhile sense. To say the least, this is a somewhat drastic consequence.

The conclusion is clear. There is no absolute reason to treat species as individuals, and compelling reasons not to do so.

7 BUT WHAT THEN ARE SPECIES?

Either species must be groups, or they must be individuals. There is no third option. They are not individuals, hence they must be groups. But, we have seen that species cannot have the absoluteness of Aristotelian natural kinds. Evolution makes this impossible. The question which therefore remains is whether we can raise species above the rather subjective level of Lockean kinds? Are species more than just artificial collections of organisms?

They are indeed. Moreover, the reasons why we rightly think that species are more than artificial collections—why we think species are natural—are similar to reasons why we think there are natural groups encountered elsewhere in science, for instance in chemistry and geology.

The key reason why species are properly treated as natural kinds lies in that most distinctive fact noted earlier: the multiplicity of species concepts and of possible definitions of taxa names. To see the connection between naturalness and multiplicity, let us pull back for a moment and ask a general question about science. At what point is it in science that we feel we are onto something 'real'? When is it we accept that we are not just dealing with hypothetical figments of a creative scientist's imagination? The strong consensus is that the breakthrough comes when we put *together* two or more *different* areas of theory into one unified whole. If you have two different

¹ An escape would be to embrace the neo-saltationary theory of 'punctuated equilibrium', supposing that one gets periods of stasis, followed by abrupt switches from one species to another. See Eldredge and Gould [1972]. The fears just expressed vanish. Wake [1980] and Mishler and Donoghue [1982] note just how tied up the s-a-i thesis is with this theory, and significantly the s-a-i thesis has been embraced enthusiastically by Eldredge [1985]. But, there are serious queries about the position. See Gingerich [1976, 1977]; Maynard Smith [1981]; and Ruse [1982] for an overview.

subjects, and they are joined, so that the one complements the other, and *vice-versa*, then you are inclined to think that there's more than mere chance at work. Somehow, the unified theory tells you about the real. Such a coming together, could not be mere coincidence—especially, if you can spell out the unification in terms of some overall theory (Leplin, [1984]).

This unification, known philosophically as a 'consilience of inductions', was the one thing that Darwin always mentioned, when his theory was challenged. 'I must freely confess, the difficulties and objections are terrific; but I cannot believe that a false theory would explain, as it seems to me it does explain, so many classes of facts' (Darwin [1887], 1, 455). And, it remains important today. For instance, in the recent geological revolution, people accepted plate tectonics when they saw that different areas of geology are unified in the one theory (Ruse [1981]).

What about classification? Is there a possibility of some sort of consilience here, separating the natural or real from the merely arbitrary? William Whewell, who had the distinction of being both a professor of mineralogy and of moral philosophy, thought there was. A natural classification is one where different methods yield the same results. Particularly, if you have reasons for the coincidence, you feel sure that the classification cannot be just chance.

The Maxim by which all Systems professing to be natural must be tested is this:—that the *arrangement obtained from one set of characters coincides with the arrangement obtained from another set*. (Whewell [1840], 1, 521, his italics. For more details on how Whewell used his ideas in mineralogy, see Ruse 1976.)

And, modern philosophers agree with Whewell. Thus Hempel:

The rational core of the distinction between natural and artificial classifications is suggested by the consideration that in so-called natural classifications the determining characteristics are associated, universally or in a high percentage of all cases, with other characteristics, of which they are logically independent. (Hempel [1952], 53. See also Schlesinger [1963] for more references to this criterion.)

Coming back to organic species, we see that we have a paradigm for a natural classification. There are different ways of breaking organisms into groups, and they *coincide*! The genetic species is the morphological species is the reproductively isolated species is the group with common ancestors. Moreover, there are reasons for the coincidence. As the zoologist Mayr points out, bringing several of the definitions together:

The reproductive isolation of a biological species, the protection of its collective gene-pool against pollution by genes from other species, results in a discontinuity not only of the genotype of the species, but also of its morphology and other aspects of the phenotype produced by this genotype. This is the fact on which taxonomic practice is based (Mayr [1969], 28).

Note, moreover, that the coincidence between variously delimited species is not unexplained. Certain genes do lead to certain morphological effects, and so forth. The consilience fits within overall biological thinking.

This consilience then is the reason why it is reasonable to think of species as natural kinds. Like the natural kinds of other sciences, they demand our attention, not because they represent some ultimate essentialist ontological carving up of the real world, but because they unite different criteria of division. They may not be Aristotelian kinds. But they are more than Lockean kinds. (See also Ruse 1973.)

8 CONSEQUENCES

A number of questions arise. What about the real/nominal definition distinction? It vanishes—which is a good thing, because it is an outmoded Aristotelian holdover anyway. You might argue that genetic differences are more crucial than anything else. (Caplan [1980, 1981]; and Kitts and Kitts [1980] argue just this.) But, from an evolutionary perspective, the genes do not have this kind of privileged status. If organisms do not have the right morphology, they will fail, no matter how superior their genotype. And in any case, a consilience is like a quarrel or a tango. You must have at least two parties. Hence, it really does not make much biological or philosophical sense to say that genes are more essential than morphology. Or that any other single feature is the ‘true’ essence of a species.

Do we still have laws about species members? I do not see why not. The solution I am offering affirms the existence of natural kinds, albeit not Aristotelian kinds. This means that it is still open to everyone to make universal claims about the members of particular species. And, these claims can rise above the merely contingent or happenstance. This does not mean that every claim that has been made about human beings embodies genuine laws. I am not, for instance, defending the validity of every part of Freudian psychoanalytic theory. On the other hand, such theory is not being ruled out as a genuine science, on *a priori* grounds, before we even start.

Finally, let me make brief reference to some of the ongoing concerns that biologists have about species. I must emphasise that I am not trying to offer a quick and easy solution to every biological species query. Species which were difficult to evaluate before this essay, will be as difficult to evaluate after this essay. I am trying to show why biologists, generally speaking, think that species are natural. But, the obverse side of the coin is that when difficulties arise and biologists no longer feel anything like as convinced of the naturalness of certain groups, the analysis offered above should show why. In particular, doubts about the reality of species should arise when the various ways of defining species names come apart, and fail to coincide.

This is indeed the case. As mentioned earlier, botanists often find themselves less than convinced of the reality of many plant species. Why? Simply, because so often plant groups which morphologically and ecologically and in other ways seem to be good species, fail the test of reproductive isolation or some like thing. One just does not have the consilience required for naturalness. Conversely, when there is isolation, there are sometimes few

other differences. In such cases, and in analogous cases in the animal world, species—however drawn—are not considered that natural. It is interesting to note how, in the case of so-called ‘sibling species’, where members of different reproductively isolated groups are morphologically similar, morphological differences are eagerly sought. Much relief is felt when such differences are found. (Grant 1981a has an excellent discussion on the difficulties plants raise for species concepts. Mayr [1963], Dobzhansky [1970], and Dobzhansky *et al.* [1977], discuss in full detail difficulties arising in the animal world.)

Also, the analysis I have offered shows just why it is that biologists are far less convinced of the reality of taxa of higher levels, than they are of species taxa. There simply is not the required consilience. Reproductive barriers are irrelevant, and there are no measures of morphological difference to coincide with genetic difference, to coincide with evolutionary difference. If anything, the evidence is that such measures are impossible. Hence, although ‘species are made by God, higher taxa are made by man’.

9 CONCLUSION

Ghiselin and Hull are surely right when they argue that we must break with Aristotelian essentialism in biology. After Darwin, such a position is otiose. But they go too far when they deny that biological species are natural kinds of any sort. They are such kinds, and the reason why it is reasonable for us to accept them as such is that same reason which makes us think any scientific claim goes beyond the merely hypothetical. It is because species are consilient.

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