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DARWINIAN METAPHYSICS: SPECIES AND THE QUESTION OF ESSENTIALISM

ABSTRACT. Biologists and philosophers of biology typically regard essentialism about species as incompatible with modern Darwinian theory. Analytic metaphysicians such as Kripke, Putnam and Wiggins, on the other hand, believe that their essentialist theses *are* applicable to biological kinds. I explore this tension. I show that standard anti-essentialist considerations only show that species do not have *intrinsic* essential properties. I argue that while Putnam and Kripke do make assumptions that contradict received biological opinion, their model of natural kinds, suitably modified, is partially applicable to biological species. However, Wiggins' thesis that organisms belong essentially to their species is untenable, given modern species concepts. I suggest that Putnam's, Kripke's and Wiggins' errors stem from adopting an account of the *point* of scientific classification which implies that relationally-defined kinds are likely to be of little value, an account which is inapplicable to biology.

1. INTRODUCTION

The nature of biological species is a topic that continues to generate considerable controversy among biologists and philosophers of biology. However on one point, there is a large measure of consensus: that *essentialism* about species is incompatible with both Darwinian theory and modern taxonomic practice. This view was first articulated by the evolutionist Ernst Mayr, who launched a powerful attack on the essentialist or "typological" species concept of pre-Darwinian biology, recommending in its place his famous "biological species concept" (Mayr 1963, 1970). Though Mayr's positive views on species have been much criticised over the years, and rivals to the biological species concept have proliferated, his attack on essentialism has met with almost universal acceptance among both biologists and philosophers of biology. Thus John Dupré (1999) writes: "it is widely recognised that Darwin's theory of evolution rendered untenable the classical essentialist conception of species" (p. 3). And Elliott Sober (1994) says: "essentialism about species is today a dead issue" (p. 163).

This anti-essentialist consensus will no doubt strike many general analytic philosophers as somewhat surprising. For in general philosophy, essentialist ideas have enjoyed a revival over the last two decades, thanks

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to the influential writings of Kripke (1980) and Putnam (1975). And both Kripke and Putnam routinely appeal to biological examples to illustrate and defend their essentialist claims. (Kripke's favourite example is 'tiger'; Putnam uses both 'tiger' and 'lemon'.) So while biologists and philosophers of biology are unanimous in rejecting essentialism as incompatible with our best biological theories, Kripke and Putnam think that their essentialist ideas are as applicable to biology as elsewhere.

There are at least three possible explanations of this strange situation. Firstly, Kripke and Putnam may be ignorant of developments in modern biology. Secondly, those who see essentialism about species as refuted by Darwinism may be ignorant of developments in modern metaphysics. Or finally, there may be different, non-equivalent concepts of essentialism at work in the two domains. In what follows I try to determine which of these three explanations is correct. First some metaphysical preliminaries.

2. INDIVIDUAL ESSENCE AND KIND ESSENCE

Essentialist claims are sometimes made about individuals, sometimes about kinds. An example of the former is the claim that my gold ring is essentially made from gold – that it could not have existed and been made of anything else. 'Being made of gold' is part of the essence of my ring, on this view. An example of the latter is the claim that gold essentially has atomic number 79 – that anything with a different atomic number could not be gold. 'Having atomic number 79' is the essence of the kind gold, on this view. Claims about kind essence and about individual essence are logically independent of each other.[1] To see this, suppose the essence of kind K is property P – necessarily all and only members of K have P. This is equally compatible with P being an essential or an accidental property of the individuals that possess it. If the former, then the members of K are essentially members of K – in any possible world where they exist, they are members of K. If the latter, then the members of K are accidentally members of K - in other worlds they are members of different kinds. The claim that the essence of kind K is P is also compatible with the view that questions about individual essence do not make sense at all.[2]

Essentialist claims of both sorts have been advanced in relation to biological species (and taxa of higher rank). Thus David Wiggins (1980), following Aristotle, claims that particular organisms are essentially members of the species to which they belong. There is no possible world in which a particular human being, e.g., Bill Clinton, exists but is not a human, according to Wiggins - and likewise for other organisms. This is a claim about *individual* essence. But consider Putnam's (1975) claim that the essence of lemonhood is having the "genetic code" of a lemon (p. 240). This is a claim about kind essence. Putnam is saying that necessarily, all and only those things with the "genetic code" in question are lemons. This claim implies nothing about the individual essence of particular lemons; specifically, it does not imply that particular lemons essentially have the "genetic code" they do. I discuss essentialist claims of both types below.

According to a widely held view, the *epistemologies* of claims about individual essence and kind essence are very different. Claims about kind essence, e.g., that water is essentially H_2O, are responsible to empirical scientific facts. For the truth of this claim requires that all samples of water do in fact have that molecular structure, and that this be a matter of natural law – and whether that is so is for science to tell us. Thus A. McMichael (1986) describes questions about kind essence as "straightforward scientific questions" (p. 47). Whereas claims about individual essence, e.g., that the wooden table I am writing on is essentially made from wood, are not responsible to empirical science, or at least not in the same way. It is of course an empirical question whether this table *is* made of wood. But the relevant question is, given that it is, could it have been made of something else? Answering this question is a job for the armchair metaphysician – she needs to determine whether there is any possible world in which my table exists but is not made from wood – and that is to be done by consulting her modal intuitions, not by looking to empirical science. Or so the story goes. This story is not *entirely* wrong, but as we shall see, nor is it the whole story.

At this point a potential complication should be noted. In the analytic metaphysics literature, it is usually assumed without question that biological species are natural kinds – entities ontologically on a par with water, gold etc. Kripke, Putnam and Wiggins all make this assumption. But in the philosophy of biology, it is widely held that species are not kinds at all, but *individuals* (Hull 1976, 1978; Ghiselin 1974). According to this view, *Homo Sapiens* is not a kind which contains Bill Clinton as a member, but an individual, extended in space and time, of which Bill Clinton is a part. The relation between an organism and its species is not kind-membership but mereological inclusion, according to advocates of the "species are individuals" thesis. Other philosophers of biology have argued that species are neither natural kinds nor individuals but "historical entities" (Ereshefsky 1992a; Sober 1993).

My own view, which I cannot defend here, is that it is largely a matter of convention whether species are conceptualized as individuals, kinds or historical entities.[3] (See Kitcher (1984) and Dupre (1993) for defences of this conventionalist view.) In any case, the issues about essentialism to be discussed here do not depend on which view of the ontological status of species we favour. To illustrate this, consider again Putnam's claim that the essence of lemonhood is having a particular "genetic code". Putnam's claim is meant to answer questions such as "in virtue of what is the piece of fruit in my fruit bowl a lemon not an orange?". Now clearly, that question does not go away, if we thinks that *Citrus Limon* is an individual rather than a kind. If *Citrus Limon* is an individual, Putnam's essentialist claim needs re-formulating as a claim about the properties in virtue of which particular lemons are *parts* of that species. The same applies to Wiggins' thesis that an organism is essentially a member of its biological species. If species are individuals not kinds, Wiggins needs only to re-formulate his thesis as the claim that an organism is essentially a *part* of its species – it could not have existed and been a *part* of a different species. In what follows, I continue to refer to species as kinds, and the relation between organisms and species as kind-membership, but none of my conclusions depends essentially on this; each could be re-formulated to fit the view that species are individuals, or historical entities.

3. KRIPKE AND PUTNAM ON THE ESSENCES OF KINDS

I turn now to Kripke's and Putnam's views on kind essence. I start by outlining the Lockean position to which Kripke and Putnam were reacting. Locke (1689) distinguished between the nominal and the real essence of a kind. By nominal essence, Locke meant the operational criteria which we use to decide whether a given object is a member of the kind or not. Thus the nominal essence of 'gold', for Locke, was 'shiny malleable yellow metal' – these were the criteria used in his day to determine whether or not something is gold. By the real essence of a kind, Locke meant the underlying microstructure which explains why members of the kind have the nominal essence they do. Locke held that real essences were unknown and would always remain so, a not unreasonable view in the 17th century. He also held that real essence was semantically inert – a kind's extension depends only on the nominal essence, not the real essence. The semantic inertness of real essence followed straight from its unknowability, for Locke, for he regarded all kinds as arbitrary human classifications whose membership criteria could not therefore depend on anything beyond human ken.

In effect, Kripke and Putnam accept Locke's nominal/real essence distinction, but hold that real essences, far from being unknowable, are being discovered by modern science. Thus science has taught us that the real essence of gold is "having atomic number 79", according to Kripke and

Putnam – this is the underlying microstructural property which explains why all samples of gold are shiny, yellow and malleable. Kripke and Putnam also depart company with Locke over the semantic inertness of real essence. Even before real essences have been discovered, there is an implicit agreement among users of a natural kind term that the term really refers to whatever underlying microstructural properties are causally responsible for the observable properties used in classifying, they argue. Thus Putnam (1975) writes: "when Archimedes asserted that something was gold he was not just saying that it has the superficial characteristics of gold; he was saying that it had the same general hidden structure (the same "essence" so to speak), as any normal piece of gold" (p. 235). Kripke and Putnam admit, of course, that it is an empirical matter whether such common "hidden structures" do actually exist in nature, but insist that where they do, the kind term refers to the hidden structure and not to the superficial characteristics initially used to delimit the kind. This semantic thesis represents a major departure from the operationalism implicit in Locke's doctrine of nominal essence.

Unsurprisingly, Kripke and Putnam reject Locke's view that all kinds are arbitrary human classifications, advocating instead a robustly realist viewpoint. Classifying on the basis of real essences "carves nature at its joints" they argue – such classifications reflect the underlying nature of reality, not the pragmatic interests of the classifier, and are thus very far from arbitrary. The discovery of real essences is one of the fundamental aims of scientific enquiry, Putnam and Kripke maintain. This picture is certainly attractive as far as chemical kinds are concerned, but its applicability to biology is more controversial.

4. THE ANTI-ESSENTIALIST CONSENSUS IN PHILOSOPHY OF BIOLOGY

I turn now to the arguments of those who see essentialism about species as incompatible with contemporary biology. What concept of essentialism is at work in such claims? Ernst Mayr (1982) describes the essentialist or "typological" species concept of pre-Darwinian biology as the view that each species is constant through time, and consists of similar individuals which share a common, unchanging essence (p. 260). Clearly, if we accept Mayr's characterisation, then essentialism about species is flatly incompatible with the theory of evolution – for the latter asserts that current species have evolved from ancestral ones and thus that species are not constant through time. But as Sober (1993) argues, the fact that oxygen can be transmutated into nitrogen is not usually taken to undermine essentialism about chemical kinds, so the fact that species are mutable should not count against essentialism about biological species either. A better characterisation of essentialism, and one that would be accepted by nearly all philosophers of biology, is David Hull's: "each species is distinguished by one set of essential characteristics. The possession of each essential character is necessary for membership in the species, and the possession of all the essential characters sufficient" (Hull 1994, 313). So characterised, essentialism is not immediately refuted by the mere fact of evolution, but is it true?

Virtually all philosophers of biology agree that the answer to this question is "no": species are not individuated by essential characters. The reason for this is partly empirical and partly conceptual. Empirically, it simply is not true that the groups of organisms that working biologists treat as con-specific share a set of common morphological, physiological or genetic traits which set them off from other species. It is true that for many groups of organisms, the use of a variety of diagnostic criteria, usually based on the organisms' gross morphology, allows them to be assigned to distinct species relatively unambiguously. (Though there are many problem cases too, particularly in botany, where taxonomists disagree about species boundaries.) But even in the non-problematic cases, species taxa are distinguished by clusters of phenotypic traits that tend to co-vary (known as "phenetic clusters"), not by essential characters. The cases of sibling species and polytypic species illustrate the impossibility of defining species in terms of necessary and sufficient characteristics very clearly. Sibling species are morphologically indistinguishable (or very nearly so) but treated as distinct because they form separate reproductive communities – they engage in little or no interbreeding. Polytypic species comprise populations of organisms that are phenotypically very different from each other, but treated as con-specific because they interbreed freely. Even among species not usually classed as polytypic, wide amounts of intra-specific variation in phenotype are usually found. (Think for example of the enormous differences found between different varieties of the domestic dog species *Canis Familiaris*.) The patterns of variation actually found in nature do not fit easily with the idea of an essentialist definition of species.

The situation does not change when we consider genetic properties. Intra-specific genetic variation is extremely wide – meiosis, genetic recombination and random mutation together ensure an almost unlimited variety in the range of possible genotypes that the members of a sexually reproducing species can exemplify. It simply is not true that there is some common genetic property which all members of a given species share, and which all members of other species lack. On the contrary, members of closely related species typically share the vast majority of their genes, and *within* each species there is much genetic variation. Indeed intra-specific genetic variation is believed to be much greater that intra-specific variation in morphology (Ridley 1993, ch. 4). This is not to deny, of course, that there are important genetic similarities between members of a single species. The vast majority of humans have 23 chromosome pairs, for example, while the primates most closely related to us normally have 24. But not *all* humans have 23 chromosome pairs – sufferers from Down's syndrome and other genetic diseases have additional chromosomes, but are still clearly human. As it is at the level of morphology, so it is at the chromosomal and genetic levels – species taxa are distinguished by clusters of covarying traits, not by shared essences. The idea that species can somehow be "defined in terms of their DNA" has no basis in biological fact, despite what many non-biologists appear to think.

Of course, variation among the members of a kind is not in itself incompatible with essentialism about that kind – so long as the variation is confined to properties that are accidental rather than essential to kind membership. But modern biology offers no grounds whatever for supposing that intra-specific variation is confined to some particular set of "accidental" traits, leaving an invariant shared essence. On the contrary, Darwinism leads us to expect variation with respect to all organismic traits, morphological, physiological, behavioural and genetic. For genetically-based phenotypic variation is essential to the operation of natural selection. If selection is to cause a species to evolve adaptations, and eventually to evolve into different species, as Darwinian theory asserts, then there must be variation within the species for selection to operate on. Intra-specific variation with respect to all organismic traits, and thus the lack of species-specific essences, is fundamental to the Darwinian explanation of organic diversity. It is for this reason that Darwinism is generally regarded as at odds with the postulation of species essences.

The conceptual argument against species essentialism is as follows. Even if there were a species whose members all shared a set of characteristics, phenotypic or genotypic, not shared by the members of any other species, this would not make those characteristics essential to membership of the species. For if a member of the species produced an offspring which lacked one of the characteristics, say because of a mutation, it would very likely be classed as con-specific with its parents. So even if intra-specific phenotypic and genetic variation were not the norm, this would not automatically vindicate the essentialist. As Sober (1994) says: "characteristics possessed by all and only the extant members of a species, if such were to exist, would not be species essences" (p. 184).

The foregoing is a brief summary of the considerations underpinning the anti-essentialist consensus in philosophy of biology. The question arises: do these considerations conflict with anything that Kripke and Putnam say? With regard to the absence of essential properties defined in terms of organisms' morphologies, Putnam and Kripke are in full agreement with the philosophers of biology. Both argue that there are no "superficial characteristics" which are essential for membership in a given species, where "superficial characteristics" are meant to be those observable traits which are initially used to delimit the kind. Thus Kripke (1980) writes: "something may have all the properties by which we originally identified tigers and yet not be a tiger" (p. 121); Putnam makes the same point in regard to lemons. However, both Putnam and Kripke appear to believe that essential properties of species can be found if we penetrate beyond the "superficial characteristics" of organisms into their "hidden structure". Thus Putnam (1975) claims that the true criterion for being a lemon is having the "genetic code" of a lemon – this, rather than any observable traits, is the essence of lemonhood, he claims (p. 240). Similarly, Kripke (1980) maintains that having the right "internal structure" is the true criterion for being a tiger – this shared "internal structure" is the essence of tigerhood, he thinks (p. 121).

Putnam's and Kripke's views do therefore clash with received biological opinion. Both fail to realise that the within-species variability that Darwinism teaches us to expect applies to the genetic and "internal" properties of organisms too, not just to their gross morphologies. Putnam's assertion that there is a shared "genetic code" which all and only lemons share is simply not the case. The same applies to Kripke's assertion that what makes an organism a tiger is having the appropriate "internal structure". (It is unclear whether by "internal structure" Kripke is thinking of genetic make-up or physiology; whichever, his claim is equally indefensible.)

Should we conclude from this that Putnam's and Kripke's views are inapplicable to classification in biology, whatever about their merits in other areas of science? (John Dupré (1981) argues for this in relation to Putnam.) In my view this does not follow. I believe that Kripke's and Putnam's views do afford insight into biological classification, though not in quite the way they imagine.

5. CONTEMPORARY SPECIES CONCEPTS AND RELATIONAL ESSENCES

It is natural to feel somewhat puzzled by the failure of species essentialism. For if we cannot find a set of properties in virtue of which my pet dog

Rover is a member of *Canis Familiaris* and not some other species, then how can we account for the facticity of assertions like "Rover is a member of *Canis Familiaris*"? Anyone who denied that assertion would presumably just be wrong, and we can hardly treat "being a member of *Canis Familiaris*" as a brute irreducible property that some organisms have and others do not, so how can there not be a set of essential properties which define that species? I suspect that this line of thought underlies the view expressed by Kitts and Kitts (1979), among others, that species must have essential genetic properties, even if no-one knows what they are.

But in fact there is no real puzzle here, nor any need to posit unknown genetic essences. For the anti-essentialist considerations reviewed above only show that species cannot be defined in terms of essential properties, if those properties are meant to be *intrinsic* properties of the species' members. It has not been shown that species-specific essences do not exist, if the essences can include relational properties of organisms. And in fact, on the most popular accounts of the species concept found in contemporary evolutionary biology, organisms are assigned to species on the basis of relational properties. These accounts do answer the question "in virtue of what is my pet dog Rover a member of *Canis Familiaris*?", but the answer does not cite intrinsic aspects of Rover's genotype or phenotype, but rather his relations to other organisms and/or to the environment. To illustrate this, a brief look at contemporary ideas about species is required.

There is a baffling array of species concepts in contemporary biology, which can be grouped into four broad categories: phenetic concepts, interbreeding concepts, ecological concepts, and phylogenetic concepts.[4] Phenetic concepts are the least popular, and are often accused of being essentialist by proponents of the other views. The basic pheneticist idea is to identify species, and higher taxa, by investigating a very large number of phenotypic traits, and constructing a measure of the "overall similarity" of any two organisms, based on how many of these traits they share. Species are then defined as the largest groupings whose members bear a certain minimum degree of overall similarity to each others. Advocates of pheneticist views, e.g., Sokal and Crovello (1970), insist that their species concept is fully "operational" – the criteria that are used to construct the overall similarity measure, and thus to delimit species, are criteria that taxonomists actually use to determine species membership in the field. The major problem with pheneticism is that by weighting traits differently, different measures of "overall similarity" can be constructed, leading to incompatible taxonomies. Given the lack of principled grounds for choosing one similarity measure over another, phenetic concepts enjoy little popularity today.

Interbreeding concepts, the most well-known of which is Mayr's "biological species concept", are probably the most widely favoured. Mayr (1969) defined species as "groups of interbreeding natural populations that are reproductively isolated from other such groups" (p. 26); at times he has permitted potentially as well as actually interbreeding populations to count as con-specific. The basic rationale behind Mayr's concept is that the discontinuities we find in nature, and the clusters of phenetic traits, are the result of restrictions on gene flow. If gene flow between natural populations were totally unrestricted, the species divisions that we see all around us would not exist. Given this theory, dividing organisms into species on the basis of interbreeding makes good sense, for it identifies units which play a fundamental role in the evolutionary process, and thus in the explanation of biological diversity. A variant of the biological species concept is the "mate-recognition" concept of Paterson (1985), which treats the capacity to recognise organisms as potential mates, rather than to interbreed successfully with them, as the criterion for con-specificity.

Ecological species concepts attempt to define species in terms of ecological rather than reproductive criteria. Thus van Valen (1976), for example, defines species in terms of ecological niches – two organisms are members of the same species if they share the same ecological niche, he maintains, i.e., if they exploit the same set of environmental resources and habitats. The rationale behind the ecological concept stems from a different view about what explains the existence of discontinuities in nature. Advocates of the ecological concept argue that gene flow is in fact of relatively little importance in the explanation, and thus that criteria based on interbreeding do not identify theoretically important units. Identifying species by occupancy of ecological niches does pick out important units, they claim, for it is the existence of these niches that explains why phenetic traits are clustered as they are. Variants of this idea take exposure to a common selective regime to determine whether two organisms are con-specific.

Phylogenetic species concepts identify species in terms of evolutionary history – they treat species as particular chunks of the genealogical nexus, bounded by speciation events and extinction events. Species come into existence when an existing lineage splits into two, on this view, and go extinct when the lineage divides, or when all members of the species die. (Lineages are ancestor-descendent sequences of populations.) Organisms belong to the species they do, according to the phylogenetic concept, in virtue of their position in the branching tree-of-life. You and I are members of *Homo Sapiens*, therefore, because we both belong to the segment of the genealogical nexus which originated in Africa some 300,000 ago (on current estimates), and which has not budded off any daughter species since that point. Any organism which does not belong to that segment is not a member of *Homo Sapiens*, however similar to us it may be. Phylogenetic species concepts normally go hand-in-hand with a phylogenetic approach to identifying taxa of higher rank too.

It is worth stressing that phylogenetic concepts are not necessarily incompatible with phenetic/interbreeding/ecological concepts, for they serve a different function. Phylogenetic concepts are intended to apply over evolutionary time, while the other three types of concept are meant to apply to contemporaneous organisms. Indeed, a phylogenetic concept will have to rely on a concept of one of the other types to yield an account of speciation events, i.e., of one lineage splitting into two (Sterelny and Griffiths 1999; Dupré 1993). Thus for example a phylogenetic concept might well make use of an interbreeding criterion, by identifying species with chunks of the genealogical nexus between speciation events, and taking a speciation event to have occurred when one part of a lineage has diverged so much that successful interbreeding with the rest of the lineage breaks down. Alternatively, the phylogenetic concept could rely on phenetic or ecological criteria to provide an account of lineage-splitting.

The serious shortcomings of the phenetic approach were mentioned above. However, the other three types of concept have well-known drawbacks too. Interbreeding concepts have no obvious application to assexual organisms, and suffer the additional problem that "can interbreed with" is not always a transitive relation; ecological concepts are compromised by unclarities in the concept of a niche; phylogenetic concepts are highly "non-operational", owing to the difficulties involved in reconstructing evolutionary history, and may lead to species classifications radically different from those traditionally accepted. A full discussion of the pros and cons of each concept, and the extent to which they diverge in extension, cannot be undertaken here.[5] The point I wish to stress is this. On all modern species concepts (except the phenetic), the property in virtue of which a particular organism belongs to one species rather than another is a relational rather than an intrinsic property of that organism. On the interbreeding concept, the property is "being able to interbreed successfully with one group of organisms and not another"; on the ecological concept the property is "occupying a particular ecological niche"; on the phylogenetic concept the property is "being a member of a particular segment of the genealogical nexus". Clearly none of these properties is intrinsic to the organisms possessing them, nor supervenes on any of their intrinsic properties. Two molecule-for-molecule identical organisms could in principle be members of different species, on all of these species concepts.

These considerations suggest that instead of saying that Darwinism shows species not to have essential properties, we should really say that Darwinism shows the essential properties of species to be relational rather than intrinsic. (Sterelny and Griffiths (1999) favour this way of putting things; see also Griffiths (1999).) These two assertions are only equivalent on the assumption that the essential properties of a kind must be non-relational. Are there any grounds for that assumption?

Historically the answer is "yes". It is clear that the concepts of essence employed by both Locke and Aristotle would not permit relational properties to figure in the specification of a kind's (real) essence. Locke was explicit about this, describing real essence as "that particular constitution which every Thing has within its self, without any relation to any thing without it" (1689, 442).[6] Relative to this notion of essence, the idea that biological species could have relational essences certainly sounds odd. But history aside, there is no obvious reason why essences should not be relational. For if the essence of a kind is simply supposed to be that set of properties which are jointly sufficient and individually necessary for being a member of the kind, then there is no particular reason why those properties should be required to be intrinsic. From this perspective, regarding contemporary species concepts as theories about the relational essences of species taxa appears perfectly reasonable. The significance of this will become apparent.

6. KRIPKE AND PUTNAM WERE HALF-RIGHT

In section 4 we saw that Kripke and Putnam wrongly believe that essential properties of species can be found by looking at the "hidden structure" of organisms, a belief which has no basis in accepted biological fact. But strangely enough, Kripke's and Putnam's account of natural kinds *can* be applied to biological species, simply by replacing their "hidden structure" with whatever relational property we take to determine species membership. Thus suppose we advocate a phylogenetic species concept. In place of the "hidden structure" which Kripke and Putnam regard as the true determinant of species membership, we should read "belonging to a particular chunk of the genealogical nexus". Their views then fit fairly well. The "superficial" morphological traits which taxonomists use to delimit species in practice are not the ultimate determinants of species membership, but rather fallible indicators, not of organisms' "hidden structure", but of their position in the tree-of-life, which is the real criterion. Furthermore, Kripke's and Putnam's account of how users of a kind term can "implicitly agree" that the kind term refers not to the superficial characteristics initially used to apply the term but rather to underlying “hidden structure”, also fits the species case well, when “hidden structure” is replaced by the relevant genealogical property. The claim then becomes that although taxonomists initially divided organisms into species on the basis of morphological criteria, their real intent (perhaps implicit) was to produce classifications which reflect the branching tree-of-life – just as chemists wished to classify by “hidden structure” even before atomic theory had been discovered. Interestingly, this claim is strikingly close to a remark of Darwin’s: “all true classification is genealogical; that community of descent is the hidden bond that naturalists have been unconsciously seeking” (1859, 404). Kripke and Putnam are not wrong to regard morphological criteria as indicative of something deeper; their error lies only in a mistaken view of what that “something deeper” is.

Can then the Kripke/Putnam model of natural kinds be salvaged in its entirety for biological species, simply by replacing their “hidden structure” with whatever relational property we take to determine species membership? Unfortunately not - this only salvages half their model. For in the Kripke/Putnam model, the concept of hidden structure in fact plays two quite distinct roles. On the one hand, “hidden structure” is what users of a natural kind term are “really” trying to refer to, i.e., what they hope their superficial characteristics are reliable indicators of. On the other hand, “hidden structure” is meant to be causally responsible for the presence of those superficial characteristics (or in some versions, to constitute their supervenience base). Thus in the case of the chemical elements, “having atomic no. 79” is both the true criterion for something’s being gold, i.e., it is what the term “gold” really refers to, and is *also* what explains why all samples of gold have the superficial characteristics – yellowness, malleability etc. – that they do. “Hidden structure” plays both a semantic role and a causal-explanatory role, in the Kripke–Putnam story.

But there is no *a priori* reason why the same thing should play both of these roles. It is perfectly possible that the extension of a kind term should be determined not by superficial characteristics but by “something else”, just as Kripke and Putnam say, without it being true that that “something else” causally explains the presence of the superficial characteristics. Simply because atomic structure performs both roles in the case of chemical elements does not mean that the two roles must always be played by the same thing. And if we apply the Kripke/Putnam model to biological species in the way I have recommended – by replacing Kripke’s and Putnam’s “hidden structure” with whatever relational property we think determines species membership – we *do* sever the semantic and causal/explanatory roles. To see this, suppose we adopt an interbreeding species concept – we treat the ability to interbreed successfully as the determinant of con-specificity and we use morphological similarity as a fallible indicator of that ability. Now clearly, the causal explanation of why an organism has the particular morphological traits it does will cite its genotype and its developmental environment, not its ability to interbreed with certain other organisms. Morphology is indicative of that ability, but not the causal outcome of it. The same applies on a phylogenetic concept – morphology can provide good evidence that an organism belongs to a particular chunk of the genealogical nexus, but its belonging to that chunk is not the explanation – or at least not the proximal explanation – of why it has the morphological traits that it does. So interbreeding/phylogeny only play the semantic role that Kripke and Putnam attribute to "hidden structure", not the causal/explanatory role.

But to repeat, there is no reason at all why the same thing should play both roles. Kripke and Putnam fail to notice this, simply because they assume that intrinsic microstructure is always the ultimate determinant of kind membership (except where purely nominal kinds are concerned). Where that assumption is warranted, as in chemistry, the semantic and causal/explanatory roles will automatically dovetail, given that an object's "superficial characteristics" supervene on its microstructural properties. However, where the assumption is not warranted, and the determinants of kind-membership are relational properties as in biology, the semantic and causal/explanatory roles will necessarily be detached from each other. So the Kripke/Putnam story, suitably amended to apply to biological species, is still only half-right.

Nonetheless, Kripke's and Putnam's ideas *are* of value in thinking about biological classification, for they provide a philosophical foundation for the rejection of phenetic concepts. As I noted, most biologists reject pheneticism (about both species and higher taxa) because of the arbitrariness involved in choosing one similarity measure over another, and the fact that different similarity measures give different taxonomies. That is no doubt good reason to view pheneticism with suspicion, but it does not answer the pheneticists' argument that only their way of classifying is properly "operational", i.e., that all other approaches try to sort organisms into species and higher taxa using criteria to which we have no direct epistemic access, such as interbreeding, phylogeny etc. Kripke's and Putnam's account of natural kinds is in part an attack on the idea that fundamental scientific categories are or should be definable operationally, and thus provides a deeper philosophical rebuttal of pheneticism than do attacks on the notion of "overall similarity".

7. INDIVIDUAL ESSENCE AND SPECIES MEMBERSHIP

I turn now to the issue of individual essence – specifically to the thesis defended by Wiggins, Aristotle and others that particular organisms belong essentially to their species, i.e., could not have belonged to a different species. How do the foregoing considerations bear on this thesis? The thesis is not *immediately* refuted by the fact that organisms belong to their species in virtue of relational not intrinsic properties. For some of the properties that advocates of *de re* essentialism allege to be essential are relational. Kripke, for example, argues that being born of one's actual biological parents is an essential property of every human being, and that property is clearly relational. So what philosophers of biology call the failure of essentialism, i.e., that fact that species have relational not intrinsic essential properties, does not immediately imply the falsity of Wiggins' thesis. Nonetheless, Wiggins' thesis *does* turn out to be false, whichever of the modern species concepts we favour. This point has recently been argued in detail by Laporte (1997), whose treatment I follow here.

Laporte's basic argument is straightforward. He shows that whether we adopt an interbreeding, ecological or phylogenetic[7] account of species, it turns out that the property in virtue of which any given organism belongs to its species is a property the organism could have lacked – in other possible worlds the organism exists but lacks the property in question, and so is a member of a different species. Laporte's argument turns on certain modal intuitions, but they are relatively innocuous.

For the interbreeding and ecological species concepts, it is easy to see that Laporte is right. Imagine a single large population from which a small splinter group becomes physically isolated (e.g., by a river changing course). Over time the splinter group adapts to new environmental conditions, and diverges from the larger population to such an extent that interbreeding between the two populations breaks down.[8] On the interbreeding concept, we then have two species not one. As Laporte points out, it is obviously possible that any organism in the splinter group might have remained in the larger population. Equally, it is possible that the splinter group might not have become reproductively isolated at all – not all peripheral isolates do. So it follows that any organism in the new species formed by the splinter group might not have been a member of that species – it might have remained a member of the original species. The same applies on an ecological concept, where speciation is judged to have occurred when the splinter population enters a new ecological niche. It seems obviously possible that a population which adopts a new ecological niche might not have done so – had ecological resources not permitted it, for example. If so, members of the splinter population (and their descendants) would have remained members of the original species. Again, species membership turns on properties that, at least in some cases, seem clearly accidental rather than essential to the organisms which possess them.

The same applies on a phylogenetic species concept, Laporte argues, where species are identified with lineages bounded by speciation events and extinction events. If we accept the quite widely held convention that one species goes extinct as soon as it is buds off a daughter species, then the falsity of Wiggins' thesis follows immediately. For it seems obviously possible that any particular unbroken lineage *might* have given rise to a daughter species. For example, consider the lineage which originated in Africa 300,000 years ago and has led to contemporary human beings. It is surely possible that the lineage could have split 200,000 years ago, say if a small local population had become stranded on an island and followed a separate evolutionary trajectory, while the rest of the lineage evolved unchanged. If so, then us contemporary humans would not have been members of *Homo Sapiens*, given the convention in question. *Homo Sapiens* would have gone extinct 200,000 years before our birth! Laporte notes, correctly, that the convention that a species goes extinct when it buds off a daughter species is not universally accepted by advocates of phylogenetic species concepts, so he concludes only that Wiggins' thesis will *probably* fail, on a phylogenetic view.

In my view Laporte underestimates the force of his own line of argument here. Wiggins' thesis will certainly fail on a phylogenetic species concept, for a simple reason. As explained earlier, phylogenetic concepts are dependent on non-temporal concepts to provide an account of the speciation events which bound lineages. Since the non-temporal concepts, whether interbreeding or ecological, identify species by properties that are accidental not essential to the organisms possessing them, as we have seen, it follows that the same is true of phylogenetic concepts. If species are lineages bounded by speciation events, and if a speciation event occurs when one lineage breaks into two reproductively isolated segments (for example), and if an organism from one segment might have been in the other, then it follows that organisms do not belong to their species essentially. So Wiggins' thesis fails.

The fact that contemporary species concepts all imply the falsity of Wiggins' thesis may seem surprising. For as I noted above, many philosophers regard claims about individual essence, unlike ones about kind essence, as turning on the modal intuitions of metaphysicians, rather than on empirical scientific facts. Now it is true that modal intuitions are needed to assess claims about individual essence, but they are not the whole story –

empirical facts are relevant too. To decide whether an organism essentially belongs to its species, we need firstly to know which is the property in virtue of which the organism does belong to its species, and secondly, to decide whether the organism might have existed but lacked that property. Modal intuitions settle the second issue, but the first requires an examination of the species concepts actually at work in biology, which Wiggins and supporters of his view fail to provide.

It is not hard, in fact, to diagnose where Wiggins' erroneous thesis derives from. For Wiggins, like Kripke and Putnam, assumes without argument that organisms belong to their biological species in virtue of their "hidden structure" – their internal, presumably genetic, properties. I believe that Wiggins combines this assumption with the general metaphysical view that an individual entity cannot undergo too drastic changes in its internal properties without ceasing to be one and the same entity, i.e., that at least some internal properties of a thing are *de re* essential to it. This metaphysical view is quite plausible – it underwrites such intuitions such as the impossibility of my pet hamster having been made of silicon etc. But the view is wholly irrelevant to determining whether organisms essentially belong to their species, for the species to which an organism belongs is not determined by its "hidden" internal properties, as we have seen. Whether or not it could have lacked such properties is therefore beside the point.

8. ON A POSSIBLE SOURCE OF OPPOSITION TO RELATIONAL TAXONOMY

Finally and somewhat speculatively, I want to consider whether there are any reasons, other than ignorance of biology, which lead Kripke, Putnam, Wiggins and others to think that the species to which an organism belongs must depend on the internal structure of that organism, rather than its relational properties. I believe that there may very well be such reasons.

One possible source of philosophical opposition to a relational taxonomy stems from a general view about the purpose of classifying in science. Clearly any set of objects, living or non-living, can in principle be classified in a variety of different ways, i.e., placed in many different kinds. That is a philosophical commonplace. But we naturally regard some kinds as more fundamental than others – chemists classify substances by their atomic number, not their colour. According to a widely held view, what makes one classificatory scheme more fundamental than another is that it permits more predictively useful generalisations to be formulated. Knowing a substance's atomic number allows you to predict much about its likely behaviour, whereas knowing its colour does not. From this perspective, sorting objects into kinds based on their relational properties will almost certainly not yield a scientifically fundamental scheme of classification. For if the members of a kind K share only a relational property, that means they may differ widely with respect to intrinsic properties. But since an object's causal propensities are usually thought to supervene on its intrinsic properties alone, K will include objects whose causal propensities are very different, which means that predictively useful generalisations about the behaviour of K's members are unlikely to exist. In short, if the scientific value of a classification is determined by the scope of the generalisations to which it gives rise, and if causal propensities supervene on intrinsic properties, then classifying by relational properties is likely to be of little scientific value. Jerry Fodor (1987) uses precisely this argument to argue against 'externalism' in the philosophy of psychology – the view that psychological states should be individuated by their relations to the environment. Relationally individuated psychological kinds can play no role in scientific psychology, Fodor claims (pp. 27–54).[9]

Suppose we endorse the above line of argument, and we also believe (for whatever reason) that the species taxa into which biologists classify organisms are scientifically useful categories. We will then be naturally led to the view that the members of any biological species *must* have some intrinsic property in common. I suggest that a similar line of reasoning may underpin Kripke's, Putnam's and Wiggins' assumption that organisms fall into biological species in virtue of their "hidden structure". In other words, these authors assume from the outset that sorting organisms into species provides a scientifically valuable way of classifying the living world, and they accept the reasoning sketched above, according to which classification must bring together things which share intrinsic properties. They are thus led to suppose that con-specific organisms must share an intrinsic essence.[10]

But as we have seen, they do not – species have relational essences. It follows, therefore, that either the species category is not a scientifically valuable one, or the line of argument outlined above, which concludes that relational taxonomies are not scientifically valuable, is flawed. I see no reason to adopt the former option – despite the myriad of problems associated with the species concept, it is hard to see how biologists could get by without it, nor is it likely that they will try.[11] So we should reject the argument against the utility of relationally defined kinds. That argument depends on two premises: (a) that a good classificatory concept must give rise to predictively useful generalisations, and (b) that a thing's causal propensities supervene on its intrinsic properties alone. I see no reason to reject (b), a standard metaphysical view. (a) is thus the culprit – we must reject the idea that the worth of a classificatory concept is solely a function of the generalisations to which it gives rise. In some areas of science this idea is no doubt correct – the value of the periodic table *does* stem from the fact that when you know what atomic number something has, you can predict a lot about how it will behave. But in other areas of science, such as biology, this account of what makes a classification a good one is not necessarily appropriate.

What then *does* make the species category a scientifically valuable one? What reason is there to sort organisms into species the way we do, rather than in some totally different way, e.g., according to their body mass, or their shape? The answer is straightforward. Whether we favour an interbreeding, ecological or a phylogenetic species concept, the species taxa we recognise represent units which we believe to play an important role in the evolutionary process. Interbreeding concepts pick out groups of organisms bound together by gene flow, ecological concepts pick out groups of organisms adapted to similar environmental conditions, and phylogenetic concepts pick out groups related by common ancestry. Each of these units is biologically significant, according to the theory of evolution. For restricted gene flow and the existence of ecological niches help explain the origin and maintenance of the biological diversity we see all around us (though which plays a more fundamental role is a matter for debate); and the branching tree-of-life, which the phylogenetic species concept attempts to reflect, is obviously the product of evolution by natural selection. The value of the species category, therefore, stems from the fact that the taxa it identifies are ones which, according to our best biological theories, play theoretically significant roles in nature. Classification in biology, unlike in chemistry, is not concerned with causal generalisation, but rather with identifying those units that play a fundamental role in the evolutionary process.

This is not to deny, of course, that knowing which species a particular organism belongs to does often allow us to predict its traits, both morphological and behavioural. Con-specific organisms, after all, do usually share many traits, even if none is essential to species membership. Nor is it to deny that the overriding purpose of taxonomy, in the pre-Darwinian era, was to allow efficient "information retrieval", a purpose it still serves today. What I do deny, though, is that the ultimate explanation of why biologists sort organisms into the kinds they do, rather than into wholly different kinds, has anything to do with the scope of the generalizations that those kinds allow organisms to be subsumed under. Philosophers of biology have often noted that there seem to be no laws which apply to all and only the members of a species taxon (Hull 1978; Rosenberg 1985);

this is no embarrassment to biologists, whereas if the same were true of the kind “oxygen”, for example, this would be an embarrassment to chemists. This reflects the fact that facilitating causal generalizations is not the primary purpose of biological classification, which in turn explains how it is that biological species can be relationally individuated and still retain their scientific importance. If Kripke, Putnam and Wiggins assume that biological kinds must be non-relational, as I hypothesise, this is because they adopt an account of the point of scientific classification which is not necessarily appropriate to all the sciences.

9. CONCLUSION

My conclusions are the following:

1. The anti-essentialist arguments of philosophers of biology show only that species cannot be defined by essential *intrinsic* properties.
2. Kripke and Putnam do make assumptions that contradict accepted biological wisdom. Nonetheless, their model of natural kinds can be applied to biological species, by replacing their “hidden structure” with whatever relational property we take to determine species membership. This preserves the semantic role played by Kripke and Putnam’s “hidden structure“, but not the causal-explanatory role. Appropriating Kripke’s and Putnam’s model in this way provides a philosophical foundation for the rejection of phenetic concepts.
3. Wiggins’ thesis that organisms belong essentially to their species fails, whichever species concept we adopt. Wiggins may be right to regard a thing’s microstructure as *de re* essential to it, but this is irrelevant to the essentiality or otherwise of species membership.
4. Kripke’s, Putnam’s and Wiggins’ error stems, I suggest, from accepting an account of the purpose of classifying which implies that relationally defined kinds cannot have fundamental scientific importance. This account is inapplicable to biology, where the centrality of the theory of evolution by natural selection dictates the need for classifications to yield taxa which correspond to units deemed important by that theory.

NOTES

[1] This independence is not always appreciated, probably because some authors, e.g., Kripke (1980), defend essentialist claims of both types.

[2] The unintelligibility of the notion of *de re* essence was famously argued for by Quine (1966), and famously denied by Plantinga (1974) and Kripke (1980).

[3] The basis for this claim is simply that for any composite individual, it is possible to define a unique class whose members are the parts of that individual.

[4] See the papers in Ereshfesky (1992b) for discussions of the various species concepts and their interrelations.

[5] See the papers in Ereshefsky (1992b) and Wilson (1999).

[6] From this quotation it may *appear* as if Locke is talking about individual essence not kind essence, but that is not so. The quoted passage continues: "but essence, even in this sense, relates to a sort, and supposes a Species. For being that real Constitution, on which the Properties depend, it necessarily supposes a sort of Thing, Properties belonging only to Species, and not to individuals" (1689, 442). Locke adhered consistently to the empiricist view that talk of *de re* essence does not make sense. See Michael Ayers (1991) 73–74 for discussion.

[7] Laporte actually uses the label 'cladistic species concept' instead of 'phylogenetic species concept'. But the latter is preferable, as its use is much more widespread.

[8] This is a standard model of how speciation occurs, known as the allopatric speciation model.

[9] Fodor writes: "what you need in order to do science is a taxonomic apparatus that distinguishes between things insofar as they have *different* causal properties, and groups things together insofar as they have the *same* causal properties" (1987, 34), emphasis in original). Fodor concedes, in a footnote, that this argument may not apply to scientific enterprises such as "natural history" which are "not primarily in the business of causal explanation" (ibid. p. 157n). He should actually exclude significant portions of evolutionary biology, systematic biology, paleontology, biogeography and ecology, not just "natural history".

[10] I should emphasise that Kripke, Putnam and Wiggins do not explicitly reason in this way. I am sketching a plausible-looking line of argument which I think may explain their views. I do not mean to imply that they have arrived at these views by consciously employing the line of argument I sketch.

[11] Though abandoning the traditional species category altogether has been advocated. See Ereshefsky (1999) and Mishler (1999).

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