# Millikan’s Historical Kinds

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Ruth Millikan introduced history into the theory of functions, and so changed the game of naturalism. The central insight of her approach to functions lay in her original use of the notion of copying or reproduction. It seemed to her that if functions are based on reproduction, they look backward in time to the earlier items from which they were copied, and to the circumstances under which the copies of some things were preserved while copies of other things were destroyed. Thus, she reasons, biological functions are historical. This was not clearly evident in other analyses of function.

The historicity of Millikan’s account is contested – it is not universally accepted that functions should be understood non-historically - as is the *kind* of historicity she postulates. However that may be, it is now (thanks in the main to her) thought to be a live option to hold that historicity has a place in the theory of universals. Straightforwardly, Millikan’s account historicizes apparently functional categories such as types of bodily organs, for example, the heart. For if, as many think, the category *heart* is defined by a certain functional role in the circulatory system, then Millikan’s analysis would make the history and emergence of this role important. (This is an area in which a different kind of history has sometimes been urged: see, for example, Matthen 2000.) More surprisingly, Millikan argued that her account historicizes non-linguistic meaning – she defines the representational content of perceptual and other mental states by their indicator-function. Finally, the notion of reproduction can be recruited to help with the definition of biological kinds such as species. For many think that these biological kinds are reproductive families defined by a unique originator. All of these universals turn out to have historically determined content in Millikan’s system.

Like many pioneers, Ruth does not always cleanse herself of the past from which she seeks to liberate the rest of us. She bursts through the walls that enclose earlier paradigms, but fragments of brick and bits of mortar cling to her. It remains for minor figures to dust her off. In an earlier article (Matthen 2006), I tried to play such a minor figure: I suggested how her treatment of non-linguistic meaning could benefit from greater consumer orientation.[[1]](#footnote-1) Here, I shall attempt to play that role again. I will suggest how her “historical kinds” can be made more historical. I shall also suggest that in one way, she goes too far with the historicity of biological kinds.

### Introduction: Russell’s Natural Kinds

The “tradition of natural kinds” (as Ian Hacking 1991 calls it) was initiated in modern philosophy by William Whewell, who defined a kind as “a class denoted by a common name about which there is the possibility of general, intelligible and consistent, and probably true assertions” (Hacking 2007, 216). In this tradition, kinds are thought of not as definable properties, but as bases for generalization and induction – “any one can make true assertions about dogs, but who can define a dog?” said Whewell (quoted by Hacking).

A paragraph from Bertrand Russell (1948) illustrates this:

The existence of natural kinds underlies most pre-scientific generalizations, such as “dogs bark” or “wood floats”. The essence of a “natural kind” is that it is a class of objects all of which possess a number of properties that are not known to be logically interconnected. [As Ian Hacking (1991, 112) notices, Russell misplaced the ‘all’ in the preceding sentence: he meant the “class of *all* objectswhich . . .”] Dogs bark and growl and wag their tails, while cats mew and purr and lick themselves. We do not know why all members of an animal species should share so many common qualities, but we observe that they do, and base our expectations on what we observe. We should be amazed if a cat began to bark. (*ibid* 335)

Russell here follows both Whewell and his (Russell’s) godfather John Stuart Mill’s way of capturing the significance of “classes”, or predicables, such as *gold* and *tiger*.[[2]](#footnote-2) In Frege’s theory of Concept and Object and Russell’s Theory of Types, these predicables are like *white*: that is, they subsume multiple individuals under a single class. (Equivalently, they are functions from individual objects to truth values.) But there is a significant difference between these natural kinds and *white*. Mill wrote:

White things are not distinguished by any common properties, except whiteness. But a hundred generations have not exhausted the common properties of animals or plants, of sulpher (*sic*) or phosphorus; nor do we suppose them to be exhaustible, but proceed to new observations and experiments, in the full confidence of discovering new properties which were by no means implied in those we previously knew. (quoted by Hacking 1991, 117)

As defined by Russell, natural kinds are not classifications like *white* that are based on a bounded commonality. They are rather associated with “a number of properties”, some of which may not be known. Let us say, anticipating a bit, that they are unbounded “property clusters”.

Russell writes:

The bearing of all this on induction is of considerable importance. If you are dealing with a property which is likely to be characteristic of a natural kind, you can generalize fairly safely after very few instances. Do seals bark? After hearing half a dozen do so, you confidently answer “yes”, because you are persuaded in advance that either all seals bark or none do. (1948, 336)

Why do these properties come in clusters? We do not know, says Russell, and for this reason, “the doctrine of natural kinds, though useful in establishing such pre-scientific inductions as ‘dogs bark’ and ‘cats mew’, is only an approximate and transitional assumption on the road towards fundamental laws of a different kind” (461-462).

Why do natural kinds support induction? This is one traditional puzzle. Another puzzle concerns natural kind terms. Corresponding to the difference between *tiger* and *white*, there are differences between the terms that denote them:

1. *User Ignorance* If I do not know the basis for distinguishing white from non-white things, I don’t possess the concept *white* and do not understand the word ‘white’. Much the same is true of concepts such as *Italian*, *dollar*, and (perhaps) *democracy*. The same is *not* true of *gold* or *tiger*. I can possess, use, and perhaps even understand the concept *tiger* even though I do not know how to distinguish tigers from other things. I may, for example, confuse tigers with leopards, but nevertheless understand the meaning of “Tigers are striped” – this is the meaning of Whewell’s aphorism about dogs, quoted at the start of this section. Even experts can be wrong about the identifying marks of tigers (or of gold). They may think that tigers are always orange and black, though in fact some are white. Experts may even once have said *a priori* that nothing white is a tiger, and they would have been wrong. Nevertheless, the concept *tiger* was, even in such a time, in the possession of humans.
2. *Variety* Finally, members of kinds such as *tiger* may not even share a distinguishing set of properties, bounded or unbounded. With biological kinds in mind, Russell (1948) writes:

A natural kind is like what in topology is called a neighbourhood. . . Cats, for example, are like a star cluster: they are not all in one intensional place, but most of them are crowded together close to an intensional centre. Assuming evolution, there must have been outlying members so aberrant that we should hardly know whether to regard them as part of the cluster or not (461).

The current consensus with regard to natural kinds derives from a proposal by Richard Boyd (1991). Boyd’s claim is that natural kinds are “homeostatic property clusters” or HPCs. According to Boyd, these classes satisfy Russell’s characterization because “homeostatic” causal influences push their members toward conformity with each other.

With *gold*, atomic structure plays this homeostatic role. That is, pieces of gold are like each other because they all have the same atomic structure. The case of *tiger* is interestingly different. A long process of selection has adapted tigers to the environmental niche that they uniquely occupy. This process has resulted in their sharing the properties by virtue of which they are so adapted. For example, they are typically brownish-orange with black stripes, because this provides good camouflage in a forest. As well, new tigers are born by a process of reproduction. This means that they will resemble already existing well-adapted tigers. Hence, they too will share the properties in question. Selection and reproduction are the homeostatic mechanisms here: tigers are like each other because they are shaped by these mechanisms. However, neither adaptation nor reproduction ensures perfect conformity. For though selection has pushed tigers toward their orange-and-black colouration, it nevertheless permits considerable variation in the actual pattern that individual tigers display. Moreover, reproduction is not perfect. Some tigers are albino because of a mutation. The net result is that tigers share a lot of properties, and in accordance with Mill’s description above, we may not know all the properties they share. But they also display a certain amount of variety, in accordance with condition ii above.[[3]](#footnote-3)

In ordinary language, a term like ‘tiger’ is used as a predicable: ‘Benji is a tiger’, we say, thus predicating *tiger* of Benji.[[4]](#footnote-4) Assuming that we are using the term ‘tiger’ here as a natural kind term, HPC theory holds that we are in effect relying on a hypothesis, namely that tigers are subject to homeostatic mechanisms that cause them to be associated with a property cluster. We rely, in other words, on an implicit theory that there are “homeostatic influences” such as shared structure, or selection and reproduction which induce an open-ended set of similarities that we can discover. (This implicit theory constitutes what Boyd calls the “accommodation demands” of our use of natural kind terms.)

We should note, in passing, that HPC theory is not, as yet, sufficiently well defined. HPC theorists such as Wilson, Barker, and Brigandt (2007) say that natural kinds are defined by “causally basic” properties, and assert that these properties must be identified empirically, rather than *a priori*. But they do not identify the empirical criterion by which the following cases should be judged:

1. Sub-classes of natural kinds may or may not be natural kinds on the HPC account. Hacking noted (1991, 112) that the class of white cats is a natural kind under Russell’s characterization (as well as Whewell’s and Mill’s). Is it so under the HPC account? There may be a homeostatic influence that has kept them white – reproduction. This uncertainty is not happy for HPC theory. Hacking thinks, rightly, that white cats should definitely not be a natural kind. But HPC theory leaves the question open, and offers no guidance on how to decide.
2. Again, it is far from clear what counts as an admissible homeostatic influence. To adapt an example from Elliott Sober (1984), let’s say that Snooty Hall (a famous school) accepts only the children of parents with a household income of $ 1 million per year or more. The admission policy is homeostatic. It ensures that all the children are from well-off families, and thus supports an indefinite number of generalizations, some still unknown, about their apparel, mode of transportation, health, and so on. Is attendance at Snooty Hall a natural kind? If it is not, is it not *only* because Snooty Hall’s admission policy is not a part of “nature”? (Be careful how you answer: is assortative mating natural or unnatural? Among peacocks? Among humans?)

Let us set these doubts aside, and continue with the exposition of the theory. With terms like ‘white’, we know the associated property – if we didn’t, we would not understand the term. But in the case of natural kind terms such as ‘tiger’, we do not necessarily know how to identify the things that belong to the class. In this case, understanding the term consists of (a) knowing (by acquaintance or by report) some tigers, and (b) knowing that these tigers belong to an HPC class (satisfying some further conditions of specificity and type). We use the term to denote the HPC class that we thus posit. This open-ended understanding accounts for why the term is epistemically unbounded. We do not know what properties are homogenized by the homeostatic influences underlying the class (thus the User Ignorance above).

The hypothesis of homeostasis could be wrong: racists make the mistake – not their only one, of course – of assuming that certain human sub-groups are natural kinds. When they use a term like ‘Muslim’, they use it as if it denotes a natural kind. But, as Mill pointed out, there is no property that Christians have and Muslims lack except as follows from their being members of their respective religions (Hacking 1991, 118). *Muslim* is therefore not a natural kind in the intended sense. One does *not* understand the term correctly if one does not know that the distinguishing mark of a Muslim is that s/he is a member of a particular religion, and that one is *not* entitled to assume that Muslims have unknown characteristics that follow upon their membership in this kind.

So goes the consensus, setting aside the sectarian skirmishes sketched in notes 2-4. Millikan accepts a version of it.

### Is Biological Homeostasis Historical?

According to Millikan (1999), biological taxa are *historical* HPCs.

The members of these kinds are like one another because of certain historical relations they bear to one another . . . (1) . . . something akin to reproduction or copying has produced all the various kind members from one another or from the same models . . . (2) . . . the various kind members have been produced in or in response to the very same historical environment (54-55)

In the case of natural kinds such as *gold*, the properties of instances come about because of “an underlying structure common to members of the kind” (49). The homeostatic influence that ensures conformity between one bit of gold and another is that each of these bits has the same underlying atomic structure. But in biological kinds, homeostasis operates through relationships among members of the kind, and relationships that those members bear to their environment. It is these relationships that ensure that “the kind does not do as Achilles’ horse did and ‘run off in all directions’; but remains relatively stable in its properties, maintaining its integrity as a kind” (55).

Put in this way, however, it is not entirely clear why Millikan says that biological kinds are *historical*. (Boyd 1999 denies, by the way, that biological HPCs are historical, though he acknowledges that selection and reproduction are homeostatic.) The difference between *gold* and *tiger* seems to lie, as Millikan describes it, in the relationality of the homeostatic influences operating in the latter. Pieces of gold are like each other because gold always has the same atomic structure. Thus, the similarity of one piece of gold to another is a matter merely of each having a certain intrinsic structure. The case is different for tigers. They are like each other because of how they stand with respect to other things, including each other. Or so Millikan argues. But where does history enter the picture?

Millikan answers this question by invoking the spatiotemporal locatedness of homeostatic relationships in biological kinds – selection and reproduction happen in time. She contrasts the “historical relations” that biological organisms bear to one another with the “eternal essence” that pieces of gold share in common. This is unconvincing.

First of all, the fact that this piece of gold has atomic number 79 is a temporal, not an eternal, fact – it has atomic number 79 *now* (and at every other moment of time), which is why its solid form is yellow, heavy, malleable, and non-reactive *now* (and always). Gold does not have atomic number 79 outside of time. (What is more, it could be argued that gold came into being, with atomic number 79, some time after the Big Bang.) Conversely, in the sense that gold *is* eternal – i.e., in the sense that the kind *gold* existed, uninstantiated, before the Big Bang – so too is the kind, *tiger*. Any organism *thus* adapted to *this* kind of environment would participate in the *tiger* property cluster whenever and wherever it existed. (Again, I am speaking on behalf of HPC theorists: I don’t agree with this way of understanding *tiger*.)

Secondly, Aristotle’s species were relational in pretty much the same way as Millikan’s. Aristotle too knew that biological organisms reproduce, and believed that every organism belongs to the same species as its parents. He could perhaps have been persuaded to adopt the Biological Species Concept. Moreover, Aristotle held that every animal’s essence is to function well in its environment. So his way of characterizing species has something to do with reproduction, function, and adaptation. But any relationality that this entails should not be taken as making Aristotle a proponent of historical kinds. In fact, when philosophers of biology say that biological essences are historical, they are precisely repudiating Aristotle’s view of these essences. To say that taxa, or biological kinds, are historical is, at a minimum, to say that they are not fixed in time (as Aristotle thought), but that they change or evolve, and display variation besides. But Millikan conspicuously avoids all talk of evolution or species-change in her argument for this particular claim of historicity. (It is actually something of a gloss on her account to say that she speaks of selection.)

Millikan is out on a limb talking about historicity in this context. Is she even right about relationality? There is another issue that intrudes here. What exactly does it mean to say that relations among organisms and environment bring it about that tigers are the same as one another? Let’s consider selection first. Elliott Sober (1984) makes an important distinction along the following lines. Selection comes about by individuals of certain types perishing. Let’s suppose tigers once came in two types: stripy and non-stripy. Prospective prey spot non-stripy tigers easily; thus, they manage to elude these tigers, which, as a consequence, starve and die. The tigers that remain are almost all stripy.

Now, one can ask two questions.

1. Did selection bring it about that most tigers are stripy?
2. Regarding any individual tiger, did selection bring it about that *it* is stripy?

The answer to question (A) is clearly “yes”. But the answer to (B) is arguably “no”.[[5]](#footnote-5) Any stripy tiger descends from an ancestor who was stripy: the elimination of non-stripy tigers has nothing to do with why *it* is stripy (as opposed to why most extant tigers are stripy).

In light of Sober’s distinction, certain of Millikan’s turns of phrase become quite significant. She says that selection maintains the integrity of a *kind*; that it prevents the *kind* from running off in all directions. Her view is *not* that selection ensures that any particular tiger is stripy, but rather that selection ensures that the kind *tiger* is more or less uniform with regard to stripiness.[[6]](#footnote-6) Thus, she implicitly acknowledges that the reason why any individual tiger is stripy is that it is descended from (or that it originates by mutation) a line of stripy tigers, and not because of selection. Let us say that selection explains trait-distributions *non-distributively*: it explains why the distribution has a certain shape, but it does not explain why any particular individual has the traits it has. This distinction will become significant later.

Now, Millikan says that reproduction too is a historical relation: so it is open to her to say that the reproduction is the locus of historicity in distributive explanations of generalizations over biological kinds. However, this too is dubious. It’s certainly true that Benji the tiger is stripy because he is copied from his stripy tiger parents – and that the same is true of every other tiger (unless they originated stripiness through a mutation). However, in any such case, there is a more proximate cause of Benji’s stripiness – the genes that Benji inherited from his parents. The parents cause him to have his genes, and the genes cause him to be stripy. So the most proximate, most direct, most relevant cause of Benji’s stripiness is something *intrinsic* – his genes. If the same genes could have been implanted in him, it wouldn’t have mattered whether his parents were stripy or not. (Technically, the genes screen the parents off as causes.)

At this point, one half of the difference between *gold* and *tiger* has just about disappeared. The kind *gold* consists of those things that have atomic number 79. The kind *gold* is associated with a property cluster because each of the things that it comprises has this structure, which is a cause of the property cluster. Similarly the kind *tiger* consists of those things that have a certain genotype (or disjunction of genotypes). This kind is associated with a property cluster because the genotype is a cause of the property cluster. Selection only shows us why some other genotypes and phenotypes were eliminated. It shows us why there are no non-stripy tigers. It doesn’t tell us why *these* tigers (all of them) are stripy, or why *those* tigers – the ones who were eliminated without issue – were non-stripy.

I said that one half of the difference between *gold* and *tiger* has disappeared. But one half remains. For in the story we have told, the environment eliminated non-stripy tigers; this implies that there were once non-stripy tigers, and would be now, except that these unfortunate animals perished. There is no analogue of this in the *gold* case. To be gold is to have atomic number 79. There were never any non-79-atomic number bits of gold. No such thing was ever gold, nor could it have been. This is an intriguing difference, and it gives relationality (and historicity) a residual role in biological explanation. But as we shall see in the following section, it is not enough. A wily Australian spoiler waits his turn in the wings.

Let me summarize the results of this section. Millikan speaks of two sources of homeostasis within biological kinds, selection and reproduction (or at least she seems to endorse Boyd in this matter). She claims that these mechanisms are relational. We have found that selection does explain certain uniformities across kinds, but it does so non-distributively. Reproduction, on the other hand, explains distributively, but it is trumped by a non-relational explanation.

### Intrinsic Properties Redux

The argument of the preceding section constitutes a major disappointment for relational natural kinds *a la* Millikan. In the terms that Russell sets, HPC theory is a success. It schematically explains why most tigers are stripy – the homeostatic mechanisms make it so – and thus it makes successful induction over HPC kinds not just a matter of chance. On the other hand, it was always a hope that HPC kinds would be explanatory. However, the Sober argument rehearsed in the previous section indicates a difficulty. Selection shows us why all tigers are stripy, but it does not explain why Benji is stripy. Even with the Theory of Natural Selection firmly in place, the reason why Benji is stripy has something to do with causal interactions between his genes or other intrinsic properties and his environment.

This is where Michael Devitt’s (2008) brilliant and pugnacious attempt to revive biological “essentialism” enters the picture. Devitt takes on board the traditional commitment of natural kind theory to explanation. But he distinguishes between *historical* explanation, which involves selection and other occurrences in the past, and *structural* explanation, which involves explanation in terms of concurrent structures intrinsic to an organism.

Some intrinsic underlying property of each Indian rhino causes it, in its environment, to grow just one horn. A different such property of each African rhino causes it, in its environment, to grow two horns. The intrinsic difference explains the physiological difference. If we put together each intrinsic underlying property that similarly explains a similar generalization about a species, then we have the intrinsic part of its essence. (*ibid*, 352)

Devitt correctly insists that structural explanation is different from historical explanation. “Regardless of the history of its coming to be true, in virtue of what is it now true?” he asks – history cannot answer *this* question, he rightly says.[[7]](#footnote-7) The arguments of the preceding section bolster his conclusion. Selection is historical, but it does not explain the properties of any individual. Parentage and reproduction are historical antecedents of an individual’s properties, but structurally explaining these properties is more fundamental.

Devitt assumes that any individual tiger’s stripes are explained not by just any property, but specifically by it being a tiger.[[8]](#footnote-8) (We are assuming here that stripiness is a characteristic property of tigers; as we shall see in a moment, Devitt needs to maintain that only characteristic properties of a kind are explained by the kind.) Now, presumably being-a-tiger is the tiger essence. But we have just shown that the relational properties both of individual tigers and of the tiger kind are not fundamental with respect to explanation. Thus, Devitt concludes that species kinds have intrinsic essences, and that these structurally explain characteristic species properties. To me, this further argument seems specious. Is it not possible that tigers were once non-stripy? If so, whatever explains their stripiness is not the essence of *tiger*. So if being-a-tiger explains stripiness, it must explain it in some other way than structurally. I’ll put this point aside for the moment. (See, however, section IV below.)

The question now arises. What happens when there is variation across a species? Consider:

1. Mohan is 5’ 11” tall and Shaq is 7’ 1”.

Though 1 is evidently a concomitant of within-species variability, it should give Devitt no difficulty at all. For though Mohan and Shaq are both humans, their widely divergent heights are not characteristic species-properties and are not explained in terms of both Mohan and Shaq being human. Devitt is committed to characteristic generalizations being distributively explained in terms of intrinsic species properties. He would presumably deny that divergences could be so explained. From his point of view, then, Mohan’s height will be partially explained structurally by some intrinsic property that he does not share with Shaq. Since Mohan and Shaq are both humans, this will not be a species property. However, their both falling within a certain height-range characteristic of humans *would* be so explained.

It should be noted *en passant* that relational HPC theory is also quite comfortable with 1. After all, HPC theory knows all about imperfect homeostasis. HPC theory will explain the divergence of height in 1 by (a) the failure of selection to homogenize height. Presumably, this failure of homeostasis is connected to (b) the viability of the two lineages that led respectively to Mohan and to Shaq. But (b) is not a species property. So here, once again, the explanation will involve factors special to these individuals, and not shared by them as members of the species.

### Population Structure

Mill and Russell thought that natural kinds support induction. Following in that tradition, Millikan, Boyd, and now Devitt put a lot of weight on uniformities across species. All think that the explanatory task of systematic biology is to explain characteristics across biological taxa. What I want to argue now is that there are certain *differences* among organisms that are also maintained by species structure. (See Ereshefsky and Matthen 2005 and Matthen 2009 for a fuller argument than I shall present here. The main novelty of the present argument is the discussion of the contrast between 1 above and 2 below.)

Here are two closely related phenomena of the sort that I want to consider.

*The Sexes* Most sexually reproducing species have two types of organism, males and females. (Some sexual species are self-fertilizing.) Though these types of organism might be very similar to one another in many respects, there is a crucial difference with regard to their reproductive role that is maintained by natural selection and species structure.

*Sexual Dimorphism* Females have an interest in mating with fit males. In some species, males develop a trait that has no function other than to indicate their fitness, or lack of deficiencies. In some cases, the indicat*ed* deficiency is ecological. The indicat*or* trait is not a deficiency of this type. However, it is a sign of such a deficiency. For example, in many birds, males do not display their characteristic bright colour in the presence of worms or blood-infections (Hamilton and Zuk 1982). Thus, a female will be interested in choosing brightly coloured mates, and males have an interest in developing traits that advertise their fitness. In other cases, the proper expression of the trait – for example, a complex courting behaviour or a suite of decorative features – depends on a host of other fitness determining genes. In these cases, an easily perceptible trait (or traits) indicates fitness, and evolves to draw more and more on fitness (Rowe and Houle 1996). In both these sorts of situation, females scrutinize prospective mates for the presence of these traits. Thus, males (but not females) end up with exaggerated traits that distinguish them sharply from females. Females, on the other hand, evolve exaggerated preferences for indicator traits.

In the sort of case that Millikan has in mind, all or most members of a species have a characteristic. This is explained by some underlying property that each has. We have argued that relations between organisms do not best explain these characteristics. By contrast, relations between organisms are essential to explaining the above phenomena. In these cases, we appeal to relations among the members of a species to explain a *difference* among them. Thus:

1. Males of species *S* have characteristic *C* and females of *S* lack *C*.
2. 2 is explained by mate choice behaviour in *S*.

Cases like this constitute the heart of Population Structure Theory (Ereshefsky and Matthen 2005). PST is meant to be an alternative to HPC theory. In PST as in HPC theory, there are underlying influences that explain the properties of individuals. However, in PST, some of these influences are difference-making or *heterotic*, not similarity-maintaining or homeostatic. In such cases, the species *S* historically explains why a certain organism has characteristic *C*, despite the fact that not every member of *S* has *C*.

We saw that Devitt’s structural explanation theory and Boyd’s HPC theory have no difficulty dealing with 1. What about 2, which mentions a property that males have and females lack? Here, Devitt and HPC theory would presumably take a similar line. Devitt would insist, rightly, that there must be a structural explanation of why any male or any female became coloured in the way they are. But, he will say, this explanation will fracture: it will not appeal to species-characteristics, much less to species essences, but to characteristics peculiar to each sex. In fact, Devitt would be hard pressed to distinguish case 1 from case 2: both involve properties not shared by every member of the species in question, and so they involve explanatory intrinsic properties that are not species properties, but rather sub-group properties.

The same goes for HPC theory. Wilson, Barker, and Brigandt (2007) are explicitly insist that cases like 2 are the same in kind as cases like 1, and hence pose no problem for HPC theory:

In light of the embrace of the intrinsic heterogeneity of biological kinds, a feature highlighted by our earlier appeal to the natural flexibility of the HPC view, it is difficult to see how pointing to a particular form that such heterogeneity takes – its clustering into polymorphisms – could undermine that view.

*Pace* these authors, dimorphisms due to sexual reproduction and sexual selection have to be treated differently from mere variation. The reason is *not* that sexual dimorphism is a “clustering”. It is rather that sexual dimorphisms arise irreducibly from relations between the males and females of a species. The case of sexual conflict, which has been intensely studied only during the last couple of decades, is particularly revealing. (See Arnqvist and Rowe 2005, chapters 1-2, for a review.) Sexual conflict arises because males and females invest unequally in offspring, for instance because of the unequal size of their gametes. Thus, the optimal mating frequency for one sex is different from that of the other – typically females invest more in a gamete, and therefore mate more selectively. In such asymmetric situations, the male’s fitness is best served by having offspring with as many females as possible and not caring too much about the risk of unfit progeny. On the other hand, the female, who might produce only one offspring per mating cycle, cannot afford to lose her offspring.[[9]](#footnote-9) Consequently, it in the male’s interest to induce a female to mate with them, but at the same time, it is in the interest of the female to choose the fittest male, and hence to postpone mating until she is ready to choose. Both strategies are costly; that is, they risk or incur reductions of fitness.

Here is an example adapted from Holland and Rice (1997). (Note how this example is different from that of fitness-indication described earlier.) Suppose that a fish feeds on red berries and has hence evolved visual sensitivity to red spots.

[The] preexisting sensory bias of females selects males to evolve an initial, rudimentary display trait [a red spot] that enhances their attractiveness to females. . . These overly attractive males induce females to mate in a suboptimal manner (e.g., too often, less-than-ideal time or place). This counter-selects females to evolve resistance to (i.e., decreased attraction), rather than preference for, the male display trait, for example, a higher requisite stimulatory threshold to induce her mating response. Males are now selected to evolve a more extreme display trait to overcome the increased receiver threshold (by receiver we mean the signal receptor(s) and all associated neurological processing of the display signal), and cyclic antagonistic coevolution ensues. (*ibid*, 1)

In this case, there is antagonistic selection for a larger vivid red spot in males, and for lower visual sensitivity for red spots in females. This selection is balanced by interactions with the environment – the red spot may make the male more subject to predation; the reduced visual sensitivity may make the female less good at finding food. These diverse pressures eventually find an equilibrium that is quite different from what one would expect on the basis of survivorship benefits alone.

This kind of case illustrates what sort of entity a species population is. (In my view, a species is a collection of populations such that there is a non-zero probability that an organism from one can participate in gene-flow to the other – see Matthen 2009 for details.) Organisms do not just interact with the environment and evolve so as most efficiently to survive and reproduce in it. They also interact with each other. Sexual reproduction, which initially evolved because of environmental advantages, creates sub-groups in a population. Each sub-group now constitutes a part of the environmental problem that that the other sub-group must solve. Since each sub-group is responding to the other one, each sub-group has a different puzzle to solve. Yet, because they use each other to reproduce, they are not independently evolving groups. The two different problems faced by the sub-groups have to be understood against the background of the integration of the group and the environment they face in common.

In a case like this, I would argue, we have:

1. Males of species *S* have a red spot and high visual sensitivity to red spots, and females of *S* lackred spots and have low visual sensitivity to red spots.

And in accordance with Devitt’s argument, we have

1. Males of species *S* share an intrinsic character *IC* that explains the male-characteristics mentioned in 6, and females of *S* share an intrinsic character *IC’* that explains the female-characteristics there mentioned.

The explanations alluded to in 7 will not be species-based, but in light of my earlier arguments, we will also have:

1. Males and females of species *S* jointly participate in mating behaviour common to their species that historically explains why males and females have the divergent characteristics mentioned in 6.

So though there are structural explanations in terms of divergent intrinsic characteristics, there are historical explanations in terms of shared relational characteristics.

Two small points:

1. Are these relational explanations non-distributive? Sober’s argument would indicate that they are. I am dubious. In my view, populations are concrete collections or pluralities (i.e., not just sets) of interbreeding organisms. The conditions that prevail within such concrete collections influence which mating relations will take place, and thus which individuals will be born (see Matthen 1999). However, this is not a particularly important point here. The important point for me here is that there is a species based explanation of difference.
2. Does my evocation of populations entail the existence of irreducibly population-level causes? I do not think so. The relations that exist between sub-groups of a population change the adaptive landscape for individual members of populations, but they do not force the view that selection, for instance, acts on population. Selection is simply a bias in births, matings, and deaths of individuals induced by the adaptive landscape.

Millikan rightly spoke of “historical relations” that members of a population bear to one another. But the relations she had in mind – reproduction and adaptation to the “external” environment – are uniform throughout the species population. Given Devitt’s arguments recounted above, her historical relations seemed to be pre-empted, as far as explanation goes, by intrinsic species properties. The fractured problems that I have introduced cannot be pre-empted in the same manner. But once one has seen how population relations influence evolution, it seems to me that one will resist the pre-emption of historical explanation even in cases where the outcome is uniform. The point that I want to make is that population structure explains trait distribution, whether that distribution is more or less uniform, or sharply divergent.

Populations are collections of individuals within which there are, as I said earlier, both similarity-making or homeostatic influences and difference-making or heterotic ones. The problem for HPC theory does not arise from a failure of uniformity at the shallow level – as Wilson, Barker, and Brigandt (2007) say, this theory is designed to cope with such failures of uniformity. The problem arises from the fracturing of homeostasis at the explanatory level. Because homeostasis is broken up in this way, the small-sample generalizations noted by Russell are more complicated than he thought. Do seals bark? You would be rash to conclude that they do after observing half a dozen males.

### Conclusion: Are Species Duplicable?

In the preceding two sections, I have attempted first to take relationality away from Millikan and then to give it back. I argued that reproduction and environmental adaptedness are not reasons enough to make species relational entities, but that the divergence of secondary sexual characteristics is. My conclusion does not, however, touch on another point – are species *historical* entities in the sense of being individuated by their history? For this is what Karen Neander (1996) asserts (see below), with Millikan’s (1996) assent.

In her 1999 paper, Millikan drew a parallel between biological taxa and kinds like the 1969 Plymouth Valiant. Something is a 69PV because it has a certain historical origin; similarly, something is a tiger because it has certain historical origin. A car just like a 69PV but not copied from the Chrysler Corporation plan is not a 69PV. Similarly, if a stroke of lightning were spontaneously to create an animal just like a tiger, it would not be a tiger, Millikan says, since as she puts it, its ontogeny and phylogeny are wrong. This strong conclusion does not follow from anything we have said so far, or from anything else that Millikan (1996, 1999) says.

First of all, it is unclear to me that the analogy with the 69PV throws much light on the biological case. Clearly, the 69PV is a historical kind. It is so not just because its members are reproduced from a template created at the Chrysler Corporation at a certain time, but also because they were branded as such. Thus:

1. Imagine that some Brazilian engineers designed a car, which turned out to be an exact replica of the 69PV. This car would not be a 69PV. This is a bit like the Swamptiger example, and one might be tempted to export the negative result. But now consider:
2. Imagine that Chrysler had sold a slightly modified version in Australia under the marquee, Dodge Dingo. The Australian car would not be a 69PV.
3. Imagine an Australian enthusiast painstakingly modified his Dodge Dingo in such a way that it exactly matched the 69PV, down to the PV plates and branding. His product would not be a 69PV.
4. Imagine that Chrysler then copied the Australian modification back to the PV late in the 1969 model year. This version would be a 69PV despite having been copied from the Australian Dodge Dingo, and not directly from the original 69PV plans. Note that it is an exact copy of something that is *not* a 69PV.
5. Finally, imagine that some Brazilian knock-off artists had copied the 69PV exactly and sold it under that name. The result would not be a 69PV.

In b-e, the copying relation works differently from that in biological taxa. So I do not think that we should use the 69PV case as a source of intuitions about biological taxa.

What historical properties mark tigers off from other things? The process of speciation suggests that tigers are those animals that belong to populations that descend from an originally speciated tiger population, which has not itself speciated. Here, reproductive isolation can be taken as a criterion for speciation: so, the mark of speciation in the original (tiger) population could be that its members ceased naturally to mate with prototigers – i.e. with animals of the type with which the tigers’ ancestors did naturally mate. This gives the original tiger population a unique historical role in the origin of the species. But does this historical role *define* the species? What would happen if an ancestor population gave rise to two descendant populations each of which speciates, i.e., each becomes reproductively isolated from the common ancestor? Suppose that the two descendants happen naturally to interbreed with each other. Would the two descendant populations then be properly regarded as conspecific? If one were a group of tigers would the other one also be?

No interbreeding

Tigers

Proto-tigers

Interbreeding

No interbreeding

Tigers??

Karen Neander (1996) writes as if this question were completely settled:

How best to define ‘species’ is still highly controversial, but all the major contemporary schools in evolutionary biology agree that conspecifics must be united by descent. Species are, on all accounts, unified segments of the phylogenetic tree; the debate between the schools concerns how that segment is to be demarcated. (119)

And she adds in a note:

According to Cladistics, for example, a species starts at one speciation event and ends at the next, whereas, according to Phenetics, parent species can survive the budding of a new species. (*ibid*, note 2)

It seems to me that this is far too strong a statement. It does not follow from the broad historical approach that Millikan favours – namely that biological kinds have trait-distributions that are ecologically shaped, even when one adds the notion that the ecology in question includes relationships within a population. For Millikan’s idea tells us nothing about the identity of kinds in cases of parallel evolution.

A moment ago I imagined an example of two speciated descendant populations that turn out to be inter-fertile. The example is not wholly invented. Schluter and Nagel (1995) give two examples of this sort of process in fish among which there is assortative mating by size. One of these is the Pacific sockeye salmon, which gives rise to several freshwater populations, each isolated from the others. In each such freshwater population, the smaller sized varieties rapidly become isolated from larger varieties and thus speciate. The other example is that of the three-spined stickleback which have independent populations in various places, including Japan and British Columbia. In each of these habitats, assortative mating by size and by habitat (shallow vs deep water; fresh-water vs oceanic) has resulted independently in speciation. The question is whether to regard the newly evolved populations in the separate locations as conspecific. Clearly, they are not “united by descent”, since there are two separate speciation events in two separate locations, and the fish from separate locations do not have the same ancestors of the same species.

Schluter and Nagel suggest that this is a case of “parallel speciation”. In a later study, Rundle et al (2000) (in which Schluter and Nagel are co-authors), parallel speciation is characterized as “a special form of parallel evolution in which traits that determine reproductive isolation evolve repeatedly in independent, closely related populations as a by-product of adaptation to different environments.” “The outcome,” Rundle et al say, is “reproductive compatibility between populations that inhabit similar environments and reproductive isolation between populations that inhabit different environments.” And yet another study (McKinnon et al 2004) shows that the Japanese and British Columbia sticklebacks interbreed assortatively by size rather than by place of origin. In these cases, it does not seem unreasonable to say that the interbreeding populations belong to the same species. In any case, conspecificity does not seem to go so clearly with unity of descent as Neander and Millikan suppose. Nor do these scientists treat the matter as if it were settled by definition. Caution is desirable.

To summarize then: species are collections of individuals among which there are relations that produce both similarities across the entire collection and also sharp dissimilarities across sub-groups. The between-sub-group dissimilarities trace to and are maintained by interactions among members across the sub-groups; hence they are historically explained by heterotic selection within a population. HPC theory and Millikan’s account are myopic in their paying attention only to homeostasis – this is a mistake that traces back to the idea that natural kinds, including biological kinds, support induction. Finally, Millikan is right to think that there is a sense in which biological kinds are historical. However, this sense of historicity does not exclude the possibility that species can originate independently. They may not be historical with regard to tracing back to a unique origination event.

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1. My own work has often paralleled, sometimes diverged from Ruth’s, and so I mention some of my writings in this context, though I am not fond of self-citation. I apologize for the volume of advertising in this paper. [↑](#footnote-ref-1)
2. Two notes concerning terminology. First, I shall italicize terms when I intend them to refer to predicables or classes; when they are in ordinary script, I am using them predicatively. Thus, *tiger* is a kind, but Benji is a tiger. Second, I’ll say that kinds are “classes”. While it is not entirely clear from the above quotation, Russell thought that natural kinds are properties, not sets. However, I want here to distinguish them from *properties*, in view of the Mill-Russell thesis that kinds are associated with *many* properties. [↑](#footnote-ref-2)
3. HPC theory recognizes both the superficial property clusters that members of kinds share (sometimes imperfectly) and the underlying homeostatic mechanisms that explain these clusters. Should kinds be defined/individuated in terms of the former or the latter? According to Boyd (1999), the answer is both: “Species are defined, according to the HPC conception, by those shared [phenotypic] properties *and* by the mechanisms . . . which sustain their homeostasis” (1999, 81). But according to Rob Wilson, Matt Barker, and Ingo Brigandt (2007), the burden falls on the homeostatic mechanisms. These authors, however, make the interesting claim that the species *category* i.e., the class of species, is an HPC kind. In arguing for this claim, they follow Marc Ereshefsky (1992) in saying that species instantiate an imperfectly shared property cluster, but do not show how an underlying homeostasis keeps these together. So perhaps there are three views (two of which are held by these authors in the same paper): namely that superficial property clusters constitute HPCs – this last attitude seems to reduce to Wittgensteinian family resemblance theory. [↑](#footnote-ref-3)
4. Michael Ghiselin (1974) and David Hull (1978) argue that species are individuals. My small amendment (Matthen 1998) is that species *populations* are individuals – species themselves are collections of populations – while *belonging to a population* is a predicable. In ‘Benji is a tiger’, the term is being used in the second way, i.e., to assert that Benji belongs to a tiger population. In this sentence ‘tiger’ does *not* denote a whole of which Benji is said to be a part. (Ghiselin and Hull take ‘Benji is a tiger’ to mean that Benji is *part of* the individual denoted by ‘tiger’. This implies that the copula ‘is a’ denotes the relationship of parts to wholes. This would be unprecedented: “My hand is a me” would be a parallel, and it is evidently inadmissible.) [↑](#footnote-ref-4)
5. Sober gives a “no” answer. I argued for a “yes” answer in Matthen (1999). But my reasons for contesting Sober are not relevant as yet. I’ll return to them later. [↑](#footnote-ref-5)
6. Wilson, Barker, and Brigandt (2007) seem to miss this point. They write that: “biological individuals often are as they are and behave as they do because of the relations in which they stand . . . Finches may tend to have beaks of a certain size and shape because of selective regimes their ancestors faced in the deep past.” [↑](#footnote-ref-6)
7. Some take Devitt to be asserting that structural explanation is more fundamental than historical. In a personal communication, he denies this. He says that he is not dealing with historical explanation – so in the end, he might well agree with everything I say below. [↑](#footnote-ref-7)
8. Devitt writes: “Matthen points out that “many biologists seem committed to the idea that something is striped *because* it is a tiger (1998, 115)”. This is a bit cheeky, since the ‘because’ I had in mind was historical, not structural. Nevertheless, I take the point. Any tiger’s stripiness must be explained in terms of an intrinsic property. So if you want to show what makes each and every tiger stripy, you need to find a common intrinsic property. [↑](#footnote-ref-8)
9. Though anisogamy, gametes of unequal size, is always a factor, the males of many species, including humans, devote considerable resources to parental care. This evens up parental investment. Mate choice is presumably more mutual in such species, though the characteristics used by males for sexual display might be different from those used by females. [↑](#footnote-ref-9)