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Drakes, seadevils, and similarity fetishism*

P.D. Magnus

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Abstract

Homeostatic property clusters (HPCs) are offered as a way of understanding natural kinds, especially biological species. I review the HPC approach and then discuss an objection by Ereshefsky and Matthen, to the effect that an HPC *qua* cluster seems ill-fitted as a description of a polymorphic species. The standard response by champions of the HPC approach is to say that all members of a polymorphic species have things in common, namely dispositions or conditional properties. I argue that this response fails. Instances of an HPC kind need not all be similar in their exhibited properties. Instead, HPCs should instead be understood as unified by the underlying causal mechanism that maintains them. The causal mechanism can both produce and explain some systematic differences between a kind's members. An HPC kind is best understood not as a single cluster of properties maintained in stasis by causal forces, but as a complex of related property clusters kept in relation by an underlying causal process. This approach requires recognizing that taxonomic systems serve both explanatory and inductive purposes.

1 HPCs in a nutshell

Samuels and Ferreira write that “philosophers of science have, in recent years, reached a consensus — or as close to consensus as philosophers ever get — according to which natural kinds are *Homeostatic Property Clusters*” [18, p. 222].

*This paper is part of a larger work on natural kinds, written while I was a visiting fellow at the Center for Philosophy of Science at the University of Pittsburgh. My productivity while there was due in no small part to vigorous interaction with many clever and helpful people; among these were Pierluigi Barrotta, Jonathan Birch (who convinced me to think about anglerfish), Kareem Khalifa, Bert Leuridan, Joseph McCaffrey (who pointed me toward bone worms), Sandy Mitchell, John Norton, Richard Samuels, Samuel Schindler, Peter Vickers, Ioannis Votsis, and James Woodward. An earlier version of this paper was presented at *Metaphysics & the Philosophy of Science* (Toronto, Ontario; May 2011) and improved in light of feedback at the conference, especially from Juha Saatsi, Matthew Slater, Joel Velasco, and Rob Wilson. Thanks also to Marc Ereshefsky, the editor of this journal, and referees for comments on earlier drafts.

The idea of homeostatic property clusters (HPCs) is due to Richard Boyd, who originally introduced it to make sense of philosophical kinds like rationality, reference, and moral goodness. Despite those philosophical motivations, Boyd's clearest example of an HPC was biological species, and the HPC idea has been picked up as a solution to the so-called species problem.

As an example, consider mallards — birds of the species *Anas platyrhynchos*. Members of the species typically look like ducks, walk like ducks, and quack like ducks. It is an aphorism that a thing which possesses these properties is probably a duck. We can extend the list to include more precise structural or biochemical properties, but for the sake of illustration I will continue to talk in terms of gross morphology. Regardless of how thorough we make our list, we will never arrive at a list of occurrent properties such that possession of them is both necessary and sufficient for being a duck. A one-legged mallard will not walk like a duck, and a mute mallard will not quack — but both are nonetheless still mallards. So the properties are not essential to the species. Rather, the properties form a cluster.

It is not merely a convenience to think of the properties which are typical of ducks as a cluster. An individual duck is a locus of properties from the cluster not by chance, but rather because of facts about her physiology. The properties typical of ducks occur here because of a causal process. The properties obtain — and continue to obtain into her dotage — literally because of her homeostatic properties. Her present physiological condition was brought about by the related causal process of her growth and development, from egg to duckling to full-grown duck. Here the 'homeostasis' is less literal, because development involves change rather than mere maintenance.¹

Yet our concern is not just with an individual duck. There are many individual ducks in the world. These separate loci of properties from the cluster are each held together by their own (similar but numerically distinct) physiologies. The numerous ducks are also the result of a causal process. The point is not simply that there is a similar but distinct process going on in each duck, but rather that ducks altogether are the result of a single, shared natural history. To put it simply, the population of mommy ducks and daddy ducks spawns clusters of duck properties in the form of ducklings. Those mommy and daddy ducks themselves were spawned by a previous generation, and so on, back through the whole evolutionary lineage.

One might pedantically object that there is no such thing as a *daddy duck*. Males of the species are *drakes* or simply *mallards*. Indeed, the word 'mallard' originally meant a wild drake, and some people I have spoken with still use the word that way. My sense of contemporary English is that both 'mallard' and 'duck' can be used in sex-neutral ways. This shift would be important if the question of natural kinds was primarily a matter of understanding natural kind terms — if, in the Kripke-Putnam tradition, one hoped to employ reference to natural kinds in order to solve semantic or metaphysical problems. Even Boyd sometimes presents the question as one of language, writing "that the subject

¹Griffiths [10] emphasizes the importance of development within species conceived as HPCs.

matter of the theory of natural kinds is... the use of natural kind terms and concepts...” [1, p. 148]. Yet he admits that the use of a particular term to mark a particular category is often a matter of convenience. If we might adopt one terminology or another, what matters is that “either choice would result in the establishment of a vocabulary... in which the same class of causally and explanatorily relevant distinctions could be drawn” [1, p. 158]. The taxonomic system is what matters, not the mapping of linguistic labels onto pigeonholes in the taxonomy. In what follows, I will use ‘mallards’ as the common name of for the species, ‘duck’ as the word for a female of the species, and ‘drake’ as the word for a male. I could just as easily use ‘member of *Anas platyrhynchos*’, ‘*A. platyrhynchos* female’, and ‘*A. platyrhynchos* male’.

Although insistence on the difference between the words ‘ducks’ and ‘drakes’ is a pedantic point, insistence on the difference between the organisms ducks and drakes is not. Mallards importantly come in two varieties, differing according to sex. An HPC identifies the causal process that explains the *similarity* among members of the species, but what does it have to say about *differences*? This question motivates an objection to the view that species are HPCs. I turn to that in the next section.

2 E&M’s objection

Ereshefsky and Matthen [8, henceforth E&M] raise an objection to the HPC approach. Although I do not think that the objection ultimately succeeds, the usual responses to it are inadequate.

It is uncontroversial that even judgements of surface similarity require a prior structuring on qualities. The structure might be common sense perception or a sophisticated theory, but it determines the the relevant properties and their respective importance. Things count as similar or different relative to that determination. As E&M put the point: Given a metric on the space of qualities, similarity can be defined as proximity according to that metric. They call this space of qualities with its accompanying metric a *morphospace*.

As they understand the HPC approach, biological taxa are identified in three steps. (Although I paraphrase each step, they enumerate the steps in this way [8, p. 5].)

1. We look around and find creatures that are clumped together in morphospace. The properties of these creatures form clusters.
2. We look for the mechanisms responsible for these clusters.
3. We rejigger the morphospace in light of these mechanisms and start again.

The final step is necessary because we ultimately want to group together specimens that do not initially look similar. For example, imagine we start by making brief observations of caterpillars and moths. They occupy different regions of the initial morphospace. Yet we want to count *Biston betularia* caterpillars and peppered moths as members of the same species, because the caterpillars grow

up to be peppered moths who lay eggs for such caterpillars. Trying to understand this in terms of property clusters and similarity is awkward. The twig-like, crawling caterpillar and the speckled, flying moth are different in their surface features. If we want to say that they form a natural kind — the argument goes — then there must be many properties that they hold in common. That is, we need to unify the list of properties which are clustered together in the HPC.

There are several possible options, none of them especially appealing

First, one might refuse to rejigger the morphospace at all. This would require saying that the caterpillar and the moth are members of distinct HPCs. E&M call this the *splitting maneuver* [8, p. 12]. For the account that species are HPCs, it would require saying that the caterpillar and moth are not members of the same species — a biological absurdity.

Second, one might rejigger the weighting of properties in the morphospace. Suppose we do this with moths and say that flying is the more significant property, that the natural condition for a peppered moth is to be a flying creature. Any individual of the species which can not fly would count as a monster, in the sense of being deformed. We might call this the *standardize maneuver*. This would be wrong headed, because a caterpillar's not flying is different than a one-legged duck's not walking. The crawling caterpillar is doing just what a member of its species naturally does *at that point in its life cycle*. Moreover, even if it were acceptable to say that a caterpillar is a deficient moth, such a maneuver is clearly wrong headed for handling sexual dimorphism. Neither ducks nor drakes are deformed counterparts of the other. Rather, both instantiate perfectly legitimate ways to be mallards. (Obviously, parallel points could be made about human sexuality, but I will not pursue them here.)

Third, one might rejigger the morphospace by changing the properties that are included in it. The caterpillars and moths that are members of the species *B. betularia* neither all crawl nor do they all fly, so one might instead say that they have the disjunctive property crawling-or-flying. E&M call this the *mereological maneuver*. As they note, it threatens to make similarity vacuous. They write:

One needs . . . to be careful how one uses such powerful logical tools: they can become a universal solvent that makes all variation disappear and collapses the entire biological domain into a single morphoclump. For one could account for the differences between bees and apes by a function on some suitably selected variables such as genetic constitution and environmental inputs . . . And one could then regard the vast network of ecological relationships that constitute the biosphere as a single homeostatic property cluster maintaining polymorphism in "Gaia." [8, p. 9]

Moreover, this approach loses track of the fact that there are two separate clusters. Members of *B. betularia* are not just crawling-or-flying and cocoon-weaving-or-egg-laying. The crawling goes together with the cocoon weaving. An HPC *qua* cluster of properties must somehow encode the fact that members of the kind come in distinct, coherent varieties.

Despite the difficulties with this approach, advocates of the HPC view of species have typically opted for the mereological maneuver. Writing before E&M, Boyd says:

The fact that there is substantial sexual dimorphism in many species and the fact that there are often profound differences between the phenotypic properties of members of the same species at different stages of their life histories (for example, in insect species), together require that we characterize the [HPC] associated with a biological species as containing lots of conditionally specified dispositional properties for which canonical descriptions might be something like, “if male and in the first molt, *P*,” or “if female and in the aquatic stage, *Q*.” [1, p. 165]

Note that Boyd does not rejigger the morphospace to introduce disjunctive properties (like crawling-or-flying) but rather to introduce conditional properties (like crawling-if-larval). The maneuver is not so much *mereological* as *dispositional*. Wilson et al. give the same reply in response to E&M, concluding that there is “nothing in what [E&M] say about this that makes this a problematic, implausible, or ad hoc view to adopt. Indeed, biologists’ knowledge about species reflects the empirical presence of complex and conditional traits” [20, p. 211].

There may be some sense to saying, of an individual caterpillar, that it has the dispositional property *if adult, then flying*. That individual caterpillar will grow up to be a moth, after all. Regardless, the dispositional maneuver is hopeless in cases of sexual dimorphism. Consider again a duck. She will never be a male, so the property *if male, then green-headed* could only be an unrealized disposition.² It depends on the counterfactual that *if* she were a male, *then* she would have a green-head. How might this counterfactual be understood such that it would be true of her? I can think of three ways.

First: *Male mallards have green heads. So, if she were a male mallard, then she would have a green head.* This counterfactual is true, but will be of no help to the HPC view. It is a syllogism. It is true of the telephone on my desk as much as of the duck that if *it* were a drake then it would have a green head.

Second: *She is a mallard. Male mallards have green heads. So, if she were a male, then she would have a green head.* The idea is to hold her species fixed when evaluating the counterfactual. The counterfactual is true, I guess, but only because we have an independent grip on what counts as a mallard. So it would be circular to say that properties like this are constitutive of the HPC that *constitutes* the species. The species boundaries must already be in place before this counterfactual has a determinate truth value. Furthermore, I do not see how the counterfactual corresponds to any trait of the duck. What makes it true is not any trait of the duck herself but facts about other mallards — the male ones.

²Perhaps ‘disposition’ here is too metaphysical a term. Regardless, the conditional property requires the truth of the corresponding counterfactual.

Third: *If we intervened on her sex so as to make her a male mallard, then she would have a green head.* This might be true, but I am unconvinced. There are many ways we might perform a sex change operation on a duck, some of which would influence her plumage and some of which her not. Perhaps the regimen of hormone treatments would in fact turn her head feathers green, but I do not know enough about ducks to say. It is possible that no ornithologist does. This ignorance does not make us question whether ducks and drakes are all mallards, but it does undo any hope of cashing out the conditional property in terms of an intervention counterfactual.

Admittedly, it is possible to make sense of counterfactuals like these for some organisms. The bluehead wrasse (*Thalassoma bifasciatum*), for example, is sequentially hermaphroditic. A female bluehead may later change into a male. Just as we can say of an individual caterpillar that it will develop into a moth, we can say of a female bluehead that it could become a male. And just as we can describe the properties the eventual moth will have, we can describe properties that the secondary male fish would have: The female-turned-male bluehead wrasse would have a blue head. So it makes sense to say, of the female, that it has the *disposition* to be blueheaded on the condition of being male. This works because there is a typical way in which females of the species become males, so the question of what this individual would be like if male has a well-defined answer.

The case of the duck is less clear because there is no typical way for a duck — once it is a duck — to become a drake. Even if there were a way of resolving the counterfactual, however, it is not clear what justifies the metaphysical backflips. The properties are only introduced to fulfill an apparent bookkeeping requirement of the HPC approach, namely that there be a unitary cluster of typical properties. So I think E&M's objection is successful against the characterizations of species as HPCs that have been offered.

Moreover, taking a step back from the specific objection, it is unclear why conditional properties like green-head-if-male should have been part of the HPC approach at all. Consider the cluster of properties that are typical of all mallards: two legs, two wings, a beak, quacking behaviour, and so on. The initial motivation for treating this as a property cluster was that these features, although typical, were not necessary. A one-legged duck which loses its leg when attacked by a fox is still unproblematically a duck, even though it lacks the property of having two legs. The dispositional maneuver would allow us to say that even the one-legged duck has the property *two-legged-unless-it-loses-a-leg*. It is true of the duck that it would still have two legs if it had not been attacked, so the corresponding counterfactual is true. Yet the *disposition* to have two legs becomes a *necessary* feature of mallards, a disposition that it just unrealized or defeated in one-legged ducks. Although the HPC approach began with the idea that species could be distinguished (in part) in terms of properties without having the properties be necessary conditions for species membership, the dispositional maneuver cooks up conditional properties which are necessary for membership.

In the next section, I suggest how HPCs can be saved.

3 Getting over similarity fetishism

Natural kinds have traditionally been conceived as collections of things that share many features. Because all the members of the kind are similar, one can make successful inductions from one or a few members of the kind to all members of the kind. This basic idea, that a natural kind is primarily a set of similar things, is what I'll call *similarity fetishism*.

Boyd begins with worries about projectability. Projectable predicates are those that allow for successful inductive inferences; they correspond to properties that can be observed in one individual which are typical of all individuals. So the task of charting a natural kind becomes a matter of writing down the list of projectable predicates. There is one list, and it becomes the property cluster of the corresponding HPC kind. As a result, Boyd writes, "Species are defined, according to the HPC conception, by those shared properties and by the mechanisms (including both 'external' mechanisms and genetic transmission) which sustain their homeostasis" [2, p. 81]. He says also, "I do not, for better or worse, hold that HPC kinds are defined by reference to historical relations among the members, rather than by reference to their shared properties" [2, p. 80]. Ereshefsky calls him to task for this, quoting this same passage and concluding, "For Boyd, similarity trumps historical connectedness" [7, p. 296].

Crucially, similarity fetishism and the impulse behind the HPC view are separable. If we deny that the identification of HPCs is a matter of drawing up *one* list of projectable predicates for the kind, then there need not be the recursive rejiggering of the morphospace that E&M describe. Of course, E&M are right to say that there is no utterly objective, non-trivial sense of *similarity*. We begin with some characterization of the space of qualities, and we identify property clusters. We then explain those clusters in terms of causal mechanisms. Perhaps we will be able to characterize the kind that is sustained by that mechanism in terms of a single list of canonical properties, but perhaps not. When we look at how the properties of an adult duck come to be manifest in a particular individual, we recognize that she had to be a duckling first. There is a related but not entirely coincident cluster of properties that characterize ducklings.

Similarly, when we consider how there come to be ducks in the world, we recognize that the process both *requires* ducks and drakes as input and *produces* ducks and drakes as output. Again, drakes are characterized by a cluster of properties that is related to but not entirely coincident with the one that characterizes ducks. The story of mallards is one that involves ducks, drakes, ducklings, and baby drakes (drakelings?).

The difference between the process I described and E&M's description of how HPCs are identified is at the last step. My version might be summarized in this way:

1. We look around and find clusters of properties.
2. We look for the mechanisms responsible for these clusters.
- 3'. We identify natural kinds by the scope of those mechanisms.

Note that 3' is not the end of enquiry. We can still study the kind that we have identified. We may even have to modify our understanding of the kind in light of further discovery. The point is simply that we are not required to immediately reconstrue the properties involved so that all the members of the kind are clustered together in morphospace; i.e., so that there is a single list of properties which are all typical for members of the kind.

I have acknowledged that ducks and drakes are distinct in important ways, so one may object: Since stronger inductive conclusions can be drawn about *ducks* by examining a duck then can be drawn about *mallards*, what I have said makes the ducks and drakes separate natural kinds. The objection is that I have performed what E&M call the splitting maneuver while propping the two kinds close together in an effort to hide the seam. Although the objection would make sense if we were trying to determine whether ducks and drakes are the same kind of thing *tout court*, there is no reason to suppose that the duck must only be counted as a member of one natural kind.

Moreover, the separate kinds *duck* and *drake* both rely on the general causal mechanism of the species, reproducing generation after generation. The homeostatic mechanism which keeps the world full of duck properties clustered together in the form of ducks is the same one that propagates drakes. So the two separate kinds, considered as HPCs, both require the same mechanism. The mechanism is not just the physiological process of an individual — although it includes that — but the whole reproduction and selection history of the species. If we begin by noticing one or the other cluster of properties, we are led to acknowledge a causal story that explains both.

E&M resist an attempt to appeal to a single mechanism, on the grounds that there will not be a single developmental mechanism present both in males and females, such that it makes males exhibit male features and females exhibit female features. They write that “the HPC theorist would need to find an underlying similarity between males and females that expresses itself differently in different circumstances. But there is no such similarity in mammalian [or, we might add, avian] species” [8, p. 9].³ This works as an objection to the dispositional maneuver, which attempts to explain dimorphism by inventing properties that are typically present in both the ducks and the drakes. Even though there is no single causal process present in a duck which is responsible for its duckness and also present in the drake which is responsible for its drakeness, the duck and drake do participate together in a *larger* process. The genetics and development of ducks and drakes differ, but they are both part of the mallard reproductive process by which a new generation of mallards (both ducks and drakes) is produced. And all mallards are products of the causal selection history which produced the species.

So I am suggesting that we adopt a view which Boyd mentions but dismisses, namely understanding species as an HPC kind “by reference to historical rela-

³They add, “Sexual dimorphism within a mammalian species is due to males and females having different chromosomes and different developmental processes. There is no theoretically meaningful similarity under which the variation between the males and females of such species can be subsumed” [8, p. 9].

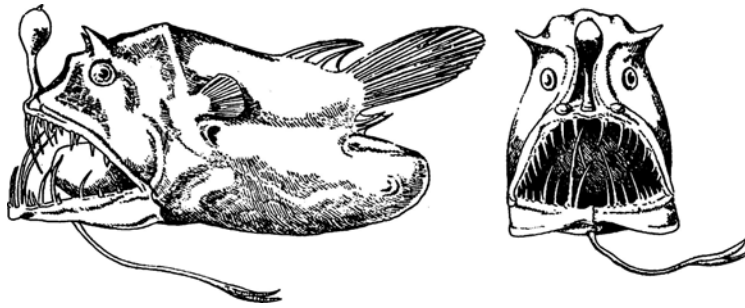


Figure 1: A female anglerfish of the species *Linophryne lucifer*. [9, plate 121]

tions among the members” [2, p. 80, quoted above]. The suggestion that species boundaries are produced by causal processes that might be called ‘homeostatic’ is not unique to Boyd, after all. Eldredge and Gould ask what marks the limits of species — that is, why some separated populations become separate species but others do not — and they give their answer in terms of ‘homeostasis’. They write:

The answer probably lies in a view of species and individuals as homeostatic systems — as amazingly well-buffered to resist change and maintain stability in the face of disturbing influences. . . . The coherence of species, therefore, is not maintained by interaction among its members (gene flow). It emerges, rather, as an historical consequence of the species’ origin as a peripherally isolated population that acquired its own powerful homeostatic system. [5, p. 114]

The causal process that unifies the species operates on the population. Mechanisms of reproduction — involving both males and females — produce another generation that includes both males and females. Mechanisms of reproductive isolation keep this kind — the mallards — separate and distinct. The homeostasis is not the production of approximately identical individuals but the maintenance of a stable configuration among individuals. The continuation of mallards, as a species, requires comparable numbers of ducks and drakes.

One might agree with this construal of HPC species but insist that it just severs the connection between HPCs and natural kinds. In the next section, I will articulate and then try to answer this objection.

4 HPCs and natural kinds

By considering mallards and peppered moths, I have perhaps been helping myself to the easy cases. Consider instead the extreme dimorphism of some species of deep-sea anglerfish. The females considered separately are rather exotic, with bioluminescent filaments growing out of their heads. Looking at an image like fig. 1, it is easy to understand how they came to be called *seadevils*. However,

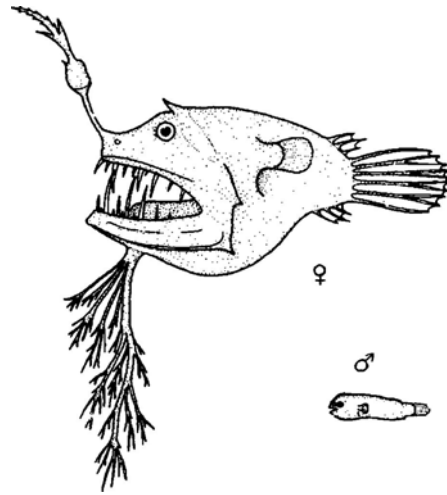


Figure 2: Female and male anglerfish of the species *Linophryne arborifera*. (©1982 Tony Ayling, used under the Creative Commons Attribution Share-Alike 1.0 license.)

the males are much smaller and less fearsome. They are incapable of feeding and attach to the females for reproduction. In some species, this attachment is permanent. As Pietsch and Kenaley explain, “the male’s attachment to the female is followed by fusion of epidermal and dermal tissues and, eventually, by a connection of the circulatory systems so that the male becomes permanently dependent on the female for blood-transported nutrients, while the host female becomes a kind of self-fertilizing hermaphrodite” [14]; see also [12] and [13].

The difference is much more radical than that between ducks and drakes. Take a specific seadevil species, such as *Linophryne arborifera*; see fig. 2. Females and males are so dissimilar that there are few inductions one can make about the species in general from a single sample. If one were simply looking for projectable predicates, then the species would not be a relevant kind at all. Rather, one might instead construct the kind *female anglerfish*. Although a diverse lot, one can draw more inductive conclusions about them from a single sample than one can about members of a specific anglerfish species.

If we are in the grip of similarity fetishism, this situation should be quite disturbing. *L. arborifera* females share a distinct, recognizable cluster of properties. They can be distinguished from the females of other anglerfish species. There is also a cluster of properties associated with anglerfish of both sexes. Pietsch says, “Anglerfishes differ radically from all other fishes” [13, p. 23], and proceeds for several pages to describe “characters shared by both sexes” [13, p. 24–30]. However, the properties of males are insufficient to diagnose species. Of anglerfish generally, Pietsch writes, “With some few exceptions, and despite major efforts, it has not been possible to establish characters that allow intra-generic [species level] identification of males” [13, p. 24]. If we merely caught free

swimming male and female specimens of *L. arborifera*, we might never include them in a common category.

As a further blow to the similarity fetishist, the dispositional maneuver does worse even here than it did with ducks. A duck is a separate persistent individual. Even though I despaired of answering the question, it at least seemed *prima facie* coherent to ask what colour her head would have been if she had been male. The male anglerfish, who has become a parasitic set of testicles, is not so obviously distinct. To describe the host female as “a kind of self-fertilizing hermaphrodite” [14, cited above] is already to treat them together as one organism. So it is less clear what object is supposed to have the unrealized disposition *bioluminescent-if-female*.

If we focus attention on the underlying causal process, however, matters are less worrisome. There are females — which is to say that the cluster of properties appears repeatedly in separate individuals — because of ongoing sexual interactions between females and males.

One might take this just to show that *L. arborifera*, although it is an HPC, is not a natural kind. This would be right if natural kinds were simply those categories which strongly support inductive inference. Indeed, many philosophers take natural kinds to be those that appear in laws of nature or at least strong, law-like regularities.

However, this conception of natural kinds is straining under the weight of similarity fetishism. As we have seen, Boyd thinks that an HPC is defined both by the causal mechanism and by a singular cluster of properties. This leads him to attribute uniform conditional properties to all members of a species, so as to make them all similar. Yet he does not think that taxonomy is simply a matter of finding kinds that will support induction. He writes, “It is a truism that the philosophical theory of natural kinds is about how classificatory schemes come to contribute to the epistemic reliability of inductive *and explanatory* practices” [1, p. 146, my emphasis]. Systems of classification are implicated in both induction and explanation, and there is no reason to think that induction is always doing the heavy lifting.

Explanatory considerations identify *L. arborifera* as a legitimate taxon, even if it is not an *inductively* robust category. The fact that populations of *L. arborifera* continue from generation to generation relies on the presence of both males and females. It is not merely that each female right now has the same *type* of causal process maintaining her characteristic features (the physiology characteristic of the species) or even that each female was the result of the the same *type* of casual reproductive process (parasitism and so on), although both are true. More than that, each and every member of the species is part of the same *token* causal process — namely, the evolutionary history of the species. This will be true for any monophyletic or even paraphyletic group, because the members of the species participate in a common causal history over evolutionary time; species are such groups.

One might object to these explanatory concerns in this way: Considering *L. arborifera* and its reproductive parasitism, the females are the autonomous organisms. The explanatory framework most appropriate for thinking about

them would treat males as a mere intermediate step in the reproductive process rather than as distinct individuals, analogous to the way we tend to think about sperm in a less dimorphic species. So the females genuinely constitute the important kind.

To press the objection, consider also recently discovered *Osedax* bone worms. Females root to the carcass of a marine vertebrate, such as a whale, and digest the bone. Males, which are often just one hundredth the size of the female, accumulate in the female's tube. Males do not share observable characteristics with females, for unaided observation at least, because only the females are large enough to see with the naked eye. Surely, the objection has it, *Osedax* females are the natural kind — they share many properties with one another but few with *Osedax* males. (This genus of worm was only discovered in 2002, but has already been the subject of much attention. See [15] [16] [17] and [21].)

Admittedly, if all or most species were like anglerfish and *Osedax*, then we might well think of males as a link in the reproductive process rather than as individuals on par with females. However, biologists recognize these species as oddities. Pietsch enthusiastically portrays anglerfish as “among the most intriguing of all animals, possessing a host of spectacular morphological, behavioral, and physiological innovations found nowhere else” [13, p. 8]. Worsaae and Rouse describe *Osedax* as “*strange, bone-eating marine annelids* with ‘roots’ that devour the sunken bones of whale and other vertebrates” [21, p. 127, my emphasis]. So the biology that would be appropriate for a world full of anglerfish and *Osedax* cannot be a guide to biology for our world. Just as hard cases make bad law, exotica make bad taxonomy.

The crucial point is that explanatory categories are not one-offs. Particular explanations figure in a discipline's overall explanatory strategy. Our attention is drawn to the interaction of *L. arborifera* females and males because of our prior encounters with less wildly polymorphic species. Species taxa are explanatorily important in biology as practiced, and biologists in the business of identifying species identify the HPC.

The approach I have outlined here requires some minimal conditions for how species are identified: For example, a taxon of sexually reproducing organisms must include both the males and the females. Another constraint, which I have simply assumed so far, is that all the members of the species must be organisms; other products of the species' causal history (such as duck droppings) will not count as part of the species. These are not arbitrary stipulations, but reflect the explanatory and empirical demands of actual biology.

Moreover, the requirement that species have a single causal history is not toothless. Craver [3] argues that the HPC approach is insufficient to pick out determinate kinds. Craver's notion of a *mechanism* is more local than something like the natural history of a species, but his worries about mechanisms can motivate parallel worries about causal histories. However, many of his arguments are directed at HPCs that are sustained by distinct but relevantly similar causal processes. If we were to think of the kind *economic recession* as an HPC, for example, then we would have to say what makes the causes of different recessions the same kind of cause. No such problem arises for a specific species, because

all of its members are the result of a single historical process. This is often expressed by saying that species are *individuals*, but what is crucial to ‘individual’ talk is the requirement that a species be historically continuous. (Hull [11] is especially clear on this point. Regarding the relation between individualism about species and the HPC approach, see Boyd [2].)

Craver also worries that explanatory considerations will be inadequate to distinguish individual mechanisms [3, §7]. Even eliminativists about the species category⁴ acknowledge the existence of specific species taxa, so there is some reason to be optimistic. All the species I have discussed in this paper reproduce sexually, so interbreeding and reproductive isolation criteria can serve to distinguish boundaries. The usual biological considerations are in play. Of course, this is not a matter to be resolved by *a priori* philosophy. If the explanatory considerations of biology are insufficient to chart the boundaries of species, then metaphysics is not up to the task either.

The considerations on which I have relied do not require any specific account of *explanation*. As Douglas [4] argues, good explanations should yield predictions. The upshot of the argument I have given is that systematic prediction, rather than straight induction, requires more than just kinds held together by similarity.

If we see an HPC as an explanatory instrument, rather than as a narrowly inductive one, we shed the vestiges of similarity fetishism. One might worry that this explodes the HPC approach and that these are no longer properly called ‘HPCs’. I am not an essentialist about philosophical doctrines, so I would not struggle too much over labels. Nevertheless, I see the view I have defended as an elaboration of Boyd’s original proposal. At the risk of being pedantic, let’s look at each term in the ‘HPC’ moniker and ask if it applies:

Are the kinds in my view ‘homeostatic’? One might worry that ‘homeostasis’ is now be a misleading metaphor, because it is not just a force to produce the *same state*. Yet homeostasis has often been used metaphorically in this broader way. Recall the quote from Eldredge and Gould (§3, above). So I answer yes.

Are the kinds in my view characterized by ‘properties’? Yes. The approach begins with properties of kind members and charts how these properties are created, sustained, and connected.

Are the properties arranged in ‘clusters’? On my view the ‘property cluster’ of an HPC need not be a single list of characteristics which all members of the kind are even likely to possess. It will be whatever complex array of property regularities are sustained by the underlying causal process. These might be a single cluster (for an asexual species), distinct but overlapping clusters (as with ducks and drakes), or almost entirely distinct clusters (as with anglerfish and Osedax). What matters is the complex of related property clusters maintained by an underlying causal process. I see this as a natural development of the

⁴Ereshefsky [6] advocates eliminating the species category, but his eliminativism just involves rejecting the idea that there is one correct set of criteria that distinguishes the boundaries of all species. Since he thinks that specific species taxa do exist, it is compatible with his view to say that any specific species is an HPC.

original cluster idea.⁵ If this is a bridge too far, then my phrase ‘HPC’ can be read instead as ‘homeostatic property complex’.

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⁵The notion of a property cluster is supposed to be in contrast to a set of necessary and sufficient conditions for kind membership. Yet, as we saw at the end of §2, proponents of the HPC view endorse the dispositional maneuver which replaces non-necessary properties with necessary conditional properties. So one might worry about whether *their* views are strictly-speaking ‘HPC’ views.

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