Why the Long Face? Biological Natural Kinds without (Many) Tears
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DRAFT — Do Not Cite

1. Introduction

Have you heard the one about the vegetarian carnivore? He walks into a bar and orders a Guinness and a salad. When he’s finished, he pulls out a gun, fires a few shots into the air, and ambles out. As he passes, the bartender shouts: “Hey, what'dya do that for?!” To which the animal replies: “It’s cause I’m a panda: look it up!” The bartender reaches for his copy of *Mammalian Systematics* and reads with a groan: “Giant Panda . . . *Ailuropoda melanoleuca* . . . eats shoots and leaves.”

Why is this joke funny? (It’s not very funny, I admit.) But if someone didn’t get it at all, how might you explain it? You’d probably cite the homonymy of ‘shoots’ and ‘leaves’ and the verbal ambiguity of punctuation: The scene describes an animal who eats (a salad) — comma — shoots (i.e., fires a gun) — comma — and leaves (i.e., exits). The bartender’s textbook describes something quite different. It describes some general features not of a particular animal in a particular bar, but of an animal species. This joke pattern appears elsewhere. For example, there’s the one about the horse that walks into the bar: The bartender asks sympathetically: “Hey, why the long face?” These jokes are funny only because we recognize a sense in which claims of the form ‘x is a horse’ or ‘x is a panda’ are legitimate explanations. (Change the bartender’s words to ‘Why the velour hat?’ and the joke is, for the most part, spoiled.) I mention them to illustrate the commonplace thought that biological taxa figure intimately in biological explanations and inductions — and not just at the species level. For example, when my neighbors ask why my parents’ dog Lucy is obsessed with herding things (cows, cats, tennis balls, &c.), they are typically satisfied with the explanation that she is a border collie. They seek no more deep or specific psychological account. That’s just what border collies do; so it should come as no surprise that this dog (being a border collie) exhibits the standard repertoire of herding instincts. Of course, a more detailed explanation referring to Lucy’s particular neurology, upbringing, and such might be welcome. But such information is simply unavailable. In its absence, we commonly employ category names in inference and explanation of behavior, morphology, and habitat of particular organisms. There seems a clear sense too in which an individualistic explanation is superfluous — the best, most informative explanation of Lucy’s cat-herding-obsession probably refers to the selective practices of Scottish shepherds over the last few centuries.

Consider different sort of example: in late 2005, WWF biologists discovered a new species of mammal deep in the Borneo rain forest — an apparent carnivore, the first discovered there in more than a hundred years. Their discovery was made on the strength of two photographs (see below) obtained from a “photographic trap”. Referred to as the ‘red Bornean carnivore’, the new critter caused a minor stir among biologists — and interesting hypotheses are already being made both regarding its novelty and its phylogeny. The BBC News quotes Nick Isaac (Institute of Zoology in London) as suggesting that the creature is likely a viverrid (“the family which includes mongoose and civets”): “One of the photos clearly shows the length of the tail and how muscly it is; civets use their tails to balance in trees, so this new animal may spend chunks of its time up trees too” (Black 2005). Isaac clearly intends his remarks to bear not only on this particular organism, but upon any organism of this
species. Biologists are not trying to find this very animal, but one of its conspecifics. And if Isaac's reasoning is correct, they have an obvious place to concentrate their efforts: in the trees.

Biologists apparently discover new species and new facts about known species and their interrelations all the time. We appeal to species membership in explanation; we employ facts about species (their morphology, behavior, and so forth) in controlling our environment, for good or ill. What allows these inferential feats? It was formerly widely thought that species' explanatory and inferential role owed to their being natural kinds. The idea is relatively simple: the essence of a natural kind provides the nomological ground required for explanation.

Whether or not we buy the deductive-nomological model of explanation in general, that certain properties are as a matter of law (conceptual or natural) associated with a particular taxon would clearly provide precisely the sort of rock bottom for inductive or explanatory claims. At this point one can either attend to or ignore the sorts of questions that arise regarding this foundation. Can we properly be said to 'discover' the essence of natural kinds (even of paradigm examples)? If so, how does this story go? If not, how thoroughly does mere convention infect scientific knowledge and progress? Are there alternative senses of 'discover' to be pulled apart here? No matter (here). The presumption of the 'natural kindness' of species has come into wide disrepute.¹

This popular conclusion seems wrong to me. It depends on too strict a conception of what it is to be a natural kind. I want to outline an alternative which I think makes better sense of biological reality and practice. Rather than asking if species are natural kinds, let us simply ask: are there natural kinds in biology? Before getting to that question, it'll be worth spelling out some of the motivations for the orthodox account of natural kinds and the reasons for pessimism that species should be counted among them.

2. The Function of Taxonomies

Before we turn in earnest to biological taxonomy, let us think about the scientific role of taxonomies in general. A good taxonomy does not merely describe the general features of the world. As Hempel puts it:

... [t]he vocabulary of science has two basic functions: first, to permit and adequate description of the things and events that are the objects of scientific investigation; second, to permit the establishment of general laws or theories by means of which particular vents may be explained and predicted and thus scientifically understood; for to understand a phenomenon scientifically is to show that it occurs in accordance with general laws or theoretical principles. (Hempel 1965, 139)

The subsumption of certain descriptions under "general laws" is what sets scientific taxonomy (at least on Hempel's account) apart from superficially similar "popular" taxonomies (of players into basketball teams, shampoo into brands, &c.). This thought is most compelling in chemistry and physics where nuclear and electro-chemical laws govern the behavior of certain molecular and atomic structures. It's for this reason that, as Ernst Nagel remarked:

The statement that something is water implicitly asserts that a number of properties (a certain state of aggregation, a certain color, a certain freezing and boiling point, certain affinities for entering into chemical reactions with other kinds of substances, etc.) are uniformly associated with each other. . . . (Nagel 1961, 31, fn 2)

But in order for natural laws to dictate the uniform association of these various properties and affinities with the statement that something is water, that statement must have something to do with those laws. One possibility is that having the cluster of properties is just what it is for something to be water — the general laws could be definitional. Water could not conceivably have had properties and affinities other than it does — for nothing without those affinities would be water. This is what Putnam calls the "traditional view" (1975, 140).

The troubles with this traditional view are well known. For one, it makes little sense of scientific discovery and progress. For two, strict conjunctions of properties are somewhat few and far between — even in chemistry, but especially in biology. More lenient "cluster accounts" of proper names looked to do better at encompassing the full range of scientific vocabulary (and then some). Rather than possessing a set of necessary and jointly sufficient conditions, natural kinds were to be associated with a cluster of descriptions none of which were necessary: so long as a thing featured "enough" of the properties so clustered.

But extensional agreement with recognized kinds (such as it is) is not enough. Kripke and Putnam drew out compelling intuitions to the effect that the whole cluster of properties might go missing — say, on Twin Earth. Of course, this intuition depends on essentialist claims about kind membership that are very unlikely to accord with our understanding of biological homogeneity. But let us recall too the motivation behind positing a discoverable essence of natural kinds. According to Putnam, natural kind terms designate 'classes of things that we regard as of explanatory importance: classes whose normal distinguishing characteristics are held together' or even

¹ There are signs of a resurgence of the species-as-kinds view, however: see Griffiths (1999), Devitt (ms).
² associated with, among others, Wittgenstein (1953) and Searle (1963).
explained by deep-lying mechanisms” (1975, 139). Real essences are thus distinguished by two features: their stability (insensitivity to various sorts of perturbations) and their role in maintaining the coherence of a cluster of associated properties. ‘Water’ names a natural kind of substance whose real essence is its molecular structure. The cluster of properties associated with water (its ‘nominal essence’) are lawful effects of a common cause: water’s underlying structure. This much I take as orthodoxy. It’s by cutting the world at its microstructural joints that taxonomies—at least in physics and chemistry—play the role they play in induction and explanation.

3. Genetic Essences?

Ditto—the thought went—for species and other taxa. Species membership is determined by an organism’s underlying (genetic or chromosomal) structure. This claim is now regarded by most biologists as false. But it is not implausible. It’s worth reflecting on why genetic structure seemed a reasonable candidate for the ‘real essence’ of species. On the surface, it is no better a candidate for ‘taxonomic distinction’ than any other feature. As Dupré remarks: “Of course, there are other microstructural features that could be supposed to be especially favored [with respect to intraspecific homogeneity], such as the presence of particular proteins, lipids, or whatever. But there is no reason to expect that any such properties enjoy a privileged status with respect to variability” (Dupré 1981, 85). Somewhat more can be said for genetic homogeneity than Dupré makes out—particularly in genes’ causal role in maintaining whatever homogeneity a species enjoys.

Consider the Twin Earth thought experiment. Putnam purportedly shows that the real essence of water is not some superficial quality we associate with actual samples of water (its nominal essence), but rather the structure that is causally responsible for those superficial qualities. A similar thought goes for species. We cannot “define” tigers as, say, fierce striped feline quadrupeds (with whiskers and a tail) because some tigers lack these qualifications and. Just as water might behave differently in different conditions, tigers might get maimed or adopt different behavioral patterns in different environments. Tigers are not easily genetically maimed, though. Their genetic structure is causally upstream from their stripes and fierceness. Insofar as genetic structure remains stable—serving as an explanation for our habitual association of a certain nominal essence with tigers—it seems an admirable candidate for the of “real essence of tiger”. As Robert Wilson characterizes this view: “Species essence is not constituted by [observable] morphological properties themselves, but by the genetic properties—such as having particular sequences of DNA in the genome—that are causally responsible for the morphological properties” (Wilson 1999, 190).

The ‘genetic essences’ view succumbs to two problems. First, even if we are impressed by the structural account of physical-chemical kinds, we should bear in mind that “genetic structure” and “molecular structure” are not especially analogous in their role in chemical and biological function and individuation. What precisely is meant by ‘genetic structure’, anyway? Watson and Crick’s famous models spring to mind—but of course they merely depict gross features of the now famous double helical structure. Nor does ‘genetic structure’ likely refer to the secondary or tertiary structure (or chromosomal structure) of DNA molecules—despite the fact that these structural features are often implicated in gene expression. One very likely means genetic sequence—DNA’s primary structure—where genetic instructions are ultimately “encoded”. But in contrast to (the usual conception of) chemical structure, an organism’s “genetic structure” (its genetic sequence) does not alone determine—or even strictly suffice to explain—facts about its outward character or behavior (even probabilistically) in the absence of information about its actual environment. The effects of one’s DNA depends, in the first instance, on how the genetic sequence is parsed and interpreted as a series of genes and (more to the point!) how those genes are regulated and translated into proteins. And that may depend on a host of epigenetic factors.

The second problem with regarding “genetic structure” as species’ real essences devolves from the heterogeneity of species. We do not find much homology in the genetic structure of conspecifics—it does not seem that there are genetic markers that all and only members of a certain (recognized) species share. Nor should we...
much expect to. As both Wilson (1999) and Gould (1991) emphasize, genotypic and phenotypic variety is a cornerstone to the very idea of evolution by natural selection: “[i]t is the raw material of evolutionary change. It represents the fundamental reality of nature, not an accident about a created norm…” (Gould 1991, 161). Most species appear to be heterogeneous right down to their microstructure.

One might resisting anti-essentialism by attenuating the concept of “genetic essence” to include other epigenetic factors or adopting a “historical essence” approach. I’m not overly hopeful about the prospects of either avenue. Suppose my pessimism is warranted; what follows? The vast majority of philosophers of biology seem aligned regarding one rather surprising claim: that since species are not natural kinds, they must be individuals — composite spatiotemporally extended objects; literally, no kidding (Ghiselin 1974; Hull 1978). This surprisingly popular thesis faces a number of problems: it has not been convincingly argued for, it has unfortunate metaphysical consequences and constraints, and it fails to provide the sought after ontological ground for biological explanation and induction.

Its popularity no doubt stems from the near consensus that species are not natural kinds. I want to argue that this consensus depends on the faulty presumption that natural kinds must be characterized by real essences. Anti-essentialist conceptions of natural kinds have not been sufficiently explored. It seems to me that the cluster account of old has more going for it than most grant. I’ll focus on Richard Boyd’s (1991; 1999) homeostatic property cluster (or ‘HPC’) account of natural kinds and its application to the biological world. I’ll suggest that Boyd has it almost right.

4. HPC Kinds

Let’s return momentarily to the pull of essences (microstructural or otherwise). They function, as Putnam says, to “hold together or explain” the normal distinguishing characteristics of a natural kind. This “holding together” helps explain several features of natural kind terms. First, that members of a natural kind have associated properties more or less in common. We should never have been tempted by a conjunctive or cluster approach if it had not seemed that natural kinds had more than one property in common. Second, that they might find a role in inductions and explanations: my observation that several objects possess a series of properties in common might suggest to me that I may really be observing effects of a common cause — perhaps a subset of those effects might reliably indicate the presence of the cause and thus of a larger set of effects. The posit of a common essence to a kind, it seems, explains the existence and scientific importance of the cluster. Unlike the old cluster account, it explains why the cluster is more than a mere figment of my worldview, why it is more than an accident that these properties are found “bound together” in nature.

The insight of Boyd’s HPC account is precisely that a cluster of properties can hold itself together. He writes:

I argue that there are a number of scientifically important kinds (properties, relations, etc.) whose natural definitions are very much like the property-cluster definitions postulated by ordinary-language philosophers except that the unity of the properties in the defining cluster is mainly causal rather than conceptual. (Boyd 1991, 141)

The coherence of these clusters are maintained by what Boyd calls “homeostatic mechanisms” — sometimes a subset of the clustered properties about which it is an a posteriori theoretical question [as to] which of these properties and which of the homeostatic mechanisms count, and to what extent they count, in determining membership in the kind” (ibid.). It’s in virtue of the coherence of these clusters that they contribute to scientific investigation. So in elucidating the world’s natural kinds, we elucidate which properties are clustered together homeostatically.

By dropping the requirement that there always be a common essence and that the cluster of properties be uniformly held together, Boyd can extend his conception of HPC kinds to a wide variety of natural, social, and ethical categories. While the homeostatic mechanisms maintaining the coherence of a biological species’ properties may be imperfect, it is

uncontroversial that [they] are phenomena that exhibit something like the sort of property homeostasis that defines homeostatic property cluster natural kinds. A variety of homeostatic mechanisms — gene exchange between certain populations and reproductive isolation from others, eff ect of common selective factors, coadapted gene complexes and other limitations on heritable variation, . . . and so on — act to es-

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*As LaPorte points out, Kripke and Putnam’s erroneous focus on chromosomal or “internal” structure need not spoil their whole program. The error can be corrected: “Given that biological kinds are delimited historically, the essences of kinds simply become historical…. Kripke and Putnam would have committed to such evolutionary essences has they known more about biological systematics, presumably: As it is, Kripke (1980) recognizes historical essences for individual organisms”. Historical ‘essences’, in one form or another, are in fact widely accepted by biologists (wrongly, I think). Darwin too thought that unity of type owed to unity of descent. In this way, historical essences apparently serve a role analogous to what Kripke and Putnam thought genetic essences would serve.*
Recognizing such patterns — discovering which property clusters are causally sustained, uncovering what Boyd often refers to as the causal structure of the world — is part and parcel to our inductive and explanatory practices. We support these practices precisely by accommodating them to the natural kinds.

Boyd thus allies himself with Quine on the role of natural kinds in science, writing “the theory of natural kinds is about how schemes of classification contribute to the formulation and identification of projectible hypotheses” (Boyd 1999, 147). This “accommodation thesis” is at the heart of Boyd’s HPC account:

What is at issue in establishing the reliability of inductive and explanatory practices, and what the representation of phenomena in terms of natural kinds makes possible, is the accommodation of inferential practices to relevant causal structures. (ibid.)

But Boyd is not altogether clear about how the existence of certain relevant “causal structures” contributes to our inferential practices. What, precisely, is meant by “the causal structure of the world”? Do the relevant causal structures contribute to the justification of these practices or merely explain their success?

Perhaps both! Boyd and Kornblith (1993) apparently see the existence of HPC kinds as explaining the past success of our inductive practices, joining a venerable tradition of understanding enumerative induction as a kind of inference to the best explanation (see Harman 1965). If the fact that all observed emeralds are green or that pandas have enlarged sesamoid bones is best explained by the hypothesis that all emeralds and all pandas (seen and unseen) have those properties, then we are entitled to infer that the hypothesis is true. In general, if the best explanation that these observed Fs are G is the fact that all Fs are G, then we are entitled to the conclusion that all Fs are in fact G — and this is exactly what we wanted out of inductive inference. Hume’s problem solved!

That’s too fast, of course. Construing even a narrow range of enumerative induction as cases of IBE is probably a bit heroic. Then there are many difficult questions about the justification (and indeed the correct characterization) of IBE — but let us assume for the sake of argument that we know well enough both when a hypothesis is the best explanation of a thesis and that in such cases we are entitled to that thesis. Which explanations are relevant? Roger White notes an important difference between (E1) “explaining, concerning the Fs we have observed, why they are G” and (E2) “explaining why all observed Fs are G” (2005, 7).

Take E1: suppose we have only observed one F and found that it is G. The generalization that all Fs are Gs does not appear to explain why this one F (say, a blade of grass) is G(reen) — that is explained by the presence of chlorophyll (Peacocke 2004, 139). On the other hand, says White, E2 “is a claim about us, namely that we haven’t set eyes on a non-G F. In (E1) we are explaining instances of the generalization that all Fs are G; in (E2) we are explaining the absence of observations of counter-instances” (2005, 8).

Peacocke calls (E1) the “narrow-scope” and (E2) the “wide-scope” reading of these explanations: for in (E1), the explanation does not involve our observations — we merely describe certain Fs on that basis, seeking explanations for their qualities (2004, 140). Pretty clearly, if either reading is going to be relevant to induction, it will be the latter:

Under the wide-scope reading it seems to me correct to say that when the enumerative induction is sound the evidence — namely, that all observed Fs are Gs — is really explained by the fact that all Fs are G. Here we are explaining a partially psychological condition — that the Fs that are observed are really G — by the holding of a condition that (in at least this respect) is not psychological, that all Fs are G. (ibid.)

But as many, including Peacocke, point out, it seems that the mere fact that all Fs are G isn’t quite enough to explain our observation to this effect — not, at least, in all circumstances. Its being a mere accident, for example, that all Fs are G robs that generalization of much explanatory potential. Consider his example: I’ve gotten very lucky at the roulette table and have won on red a hundred times in a row. Your observation of fifty of these spinnings, though perhaps explained by the true generalization regarding all my spinnings, does not entitle you to infer that my next spinning will land on red. What is required, argues Peacocke, is a commitment to the existence of some condition C that explains why all the Fs are G” (141). Such a commitment is obviously missing in the roulette case; indeed a contradictory commitment had better be in place.

Ruth Millikan writes along similar lines that “Clearly a concept having [rich inductive potential] does not emerge by ontological accident. If a term is to have genuine [inductive potential], it had better attach not just to an accidental pattern of correlated properties, but to properties correlated for a good reason” (2000, 17). Kornblith (1993) and Sankey (1997) are a bit more explicit in their advocacy of causal mechanisms as providing this “good reason”. Causal, homeostatic mechanisms make it no accident that the correlated properties enjoy a broad stability and hence HPC kinds provide the “natural ground” of induction.

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9 See Lipton (2004) for an excellent discussion of these issues.
Unfortunately, for this general approach to address the inductive skeptic, it seems that we must construe the relevant homeostatic mechanisms as *essential*. For suppose we did *not*; but we nevertheless believe that a particular cluster is in fact held together by a particular homeostatic mechanism. Its existence, however, goes only so far in filling the Humean abyss between correlations today and correlations tomorrow (or observed correlations and universal ones). The skeptic might argue as follows: a particular causal mechanism may indeed explain observed regularities, but we resume the circularity worries in inferring the continued action of that mechanism into the future. Unless, that is, we can find a causal mechanism responsible for maintaining that (first-order) mechanism. But this, of course, is to turn a circle into a regress (perhaps later back again). We might go externalist about inductive knowledge in general, but this looks like changing the subject — anyway it renders the appeal to natural kinds otiose. If the homeostatic mechanisms maintaining the stability of the property cluster are the *essence* of that kind, however, we get a bye against the skeptic. Projection of observed properties across unobserved instances of that kind are ensured (reasonably well) by the homeostatic mechanism — for if something lacks that causal mechanism, it would cease to be a thing of that kind. And since we are entitled to infer the existence of the causal mechanism in projected cases, we are entitled to the projection.

5. The Challenge of Polymorphism

There are several problems with this line of thought. For one, there is the skepticism that members of a species share any essential properties. If, in order to pay inferential dividends, HPCers need to invoke some sort of essence, will they not run up against that same empirical reality? Waiving the requirement that the properties characterizing a homeostatic property-cluster kind are in some sense “deep-lying” (or accounted for by a deep-lying causal mechanism), raises a worry about the “surface” alternatives and the consistency of the HPC approach with modern biological practice — to say nothing of failing to provide that “natural ground” for induction. Even the most serious and uncompromising cladists recognize that species are practically identified on the basis of phenetic characteristics (Ridley 2003, 348). Indeed, these traits, along with genetic analyses are often the *only* evidence we have for the phylogenetic relationships later taken by the cladists as fundamental. But cladists maintain that phylogeny, not phenetic characters are of primary importance to taxonomy. It would thus appear that Boyd’s account rules out a phylogenetic treatment of species: for surely there are no homeostatic mechanisms maintaining a species place on the phylogenetic tree. As though incurring the ire of the now dominant school of taxonomy wasn’t enough, adducing a central role for phenetic character in HPC kinds becomes problematic in *polytypic* species — species, like *Canis familiaris* (the domesticated dog), which have many phenetically distinct morphs.

Ereshefsky and Matthen specifically claim that HPCers like Boyd (1991, 1999) and Millikan (1999) “misidentify the phenomenon” which the HPC theory attempts to solve.

They think that natural kinds, including biological taxa, are united by *similarity* within the species-population, and this is what needs to be explained by homeostatic mechanisms. And it is undeniably true that there is similarity within biological taxa. Nevertheless, it is equally true that many taxa are characterized by stable and persistent differences. (2005, 7)

Familiar examples — of sexual dimorphism or differences in developmental stage — show that if species are natural kinds, natural kinds are not united exclusively by similarity relations; at least not by *outward* ones. As soon

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10 Thanks to Heather Varco for pointing this cartoon out to me.
as one starts to seriously consider the prospect of characterizing species — even roughly, imperfectly — by reference to a cluster of shared properties, one encounters a biological accounting nightmare. Far from homeostatic mechanisms maintaining the similarity of members of a species, there may be mechanisms preserving a population's polymorphism: what Ereshefsky and Matthen refer to as “heterostatic mechanisms” (14).

Two knee-jerk responses to this problem should be set aside (one more gently than the other): on the one hand, as we've noted, polytypism drives many of “deep” structural properties (like genetic essences) that even members of different sexes or morphs necessarily share. This wrongly assumes that there are (and will always be) suitably deep properties to do the job. Failing this, biologists may go historical: phenetic character (either at the macro or micro level) doesn't matter nearly as much as descent. While this latter option has more going for it, it seems not the only solution. Do not HPCers have a way out?

Boyd recognizes the challenge posed by polytypic species, but sees it as requiring only elaboration of the HPC account: we may have to “characterize the homeostatic property cluster associated with a biological species as containing lots of conditionally specified disposition properties” (1999, 165). Matthen and Ereshefsky worry that this sort of maneuver will “become a universal solvent that makes all variation disappear and collapses the entire biological domain into a single morpho-clump”, thus trivializing natural kinds. “One could then regard the vast network of ecological relationships that constitute the biosphere as a single homeostatic property cluster maintaining polymorphism in ‘Gaia’” (2005, 9). This seems an overreaction. Clearly, that we can identify terrestrial life as a kind does not imply that we ought to, ignoring nested kinds. Ignoring kinds does not mean “dissolving” them in our dangerously corrosive HPC solvent — indeed, we don’t even have to choose which natural kind to ignore.11

So plausibly, the HPC account has resources to account for “conditional similarities” among species. Without trying to defend how this story might go in any detail, it’s worth pointing out two of its likely features. First, there will be a certain liberty in attending to properties. It is a mistake to think that the HPC account is or ought to be concerned only with “shallow” phenetic characteristics. As we’ve seen, both Boyd and Millikan place considerable weight on “mechanisms” (think Paterson’s mate-recognition mechanisms, for example) that facilitate conspecific interbreeding. While some of these mechanisms may be extrinsic to the members of a kind, they certainly have no intention of ruling out attention to the sort of “deep” causal mechanisms that Putnam and Kripke had in mind. Second, such liberality naturally breeds taxonomic promiscuity: it may be a wide-open question as to which properties we deem worthy of including in a particular cluster kind. It will likewise be an open question about how we attempt to cash out the sort of “conditionally-specified dispositional properties” in question.

The openness of these questions suggests a way out of this whole mess. The problems with “correctly” specifying the dispositional properties which characterize a polytypic species dissolve if we drop the insistence that there is a correct specification — if we drop the insistence, in other words, on the existence and identification of a uniquely appropriate basal rank. Consider a fairly straightforward case of polytypism, apparently easily handled by the mechanisms Boyd recommends. As is well known, many plant species assume dramatically different morphologies (sometimes referred to as “ecotypes”) in different environments (see a helpful discussion in Lewontin 2000, especially at p. 21). Ereshefsky and Matthen mention two varieties of hemlock, one a short shrub-like tree found in the mountains, the other a tall tree found at lower altitudes. A classification by surface characteristics alone might assign these to different taxa. However, when it becomes apparent that the environment influences the height and direction of growth, and that offshoots of a plant situated in one environment take on the other form in the other environment, it is recognized that they belong to the same species. Height, then, is not an independent classifier, but one that emerges from something deeper in conjunction with environment. Thus, one could say that hemlocks share the following characteristic: shrub-if-in-mountains-and-tall-tree-if-at-low-altitude, and further that this shared characteristic flows from some “homeostatic mechanism.” Thus, this contrived characteristic is important to membership in the species while the shrub characteristic taken by itself is not. The causal diagnosis helps us find an underlying similarity to replace the superficial variety. (2005, 5)

The existence of ecotypes might be an embarrassment to the HPC account if it was an account exclusively of species — but it’s not; it’s an account of natural kinds. (I should not want to attribute to Ereshefsky and Matthen the mistake of concluding that biological natural kinds are species from the claim that species are natural kinds.) Pretty clearly, HPCers might wish to regard the different ecotypes of hemlock as different natural kinds in virtue of their different morphology (stable in their respective environments). Such differences, after all, can play a significant role in our inductions and explanations. Equally, the discovery that these two apparently different species can in fact interbreed (and assume the characteristics of the other when swapping environments), may take

11 Nor is it absurd that we should regard the whole of terrestrial life as a kind: Despite appearances, but so far as we know, every member of every species currently living in this solar system is descended from one ancestral cell. In virtue of this common heritage we find imnumerable similarities across the widest species gaps — from startling similarities in our genetic code, the chirality of our amino acids, to the jury-rigged biochemical contrivances that keep us going.
precedence when we construct our taxonomies. And if we recognize dispositional properties into the cluster, what should stop us from including such ‘contrived’ conditional characteristics alongside them?

It should thus be clear that HPC account will likewise have no difficulty accounting for differences which are not readily apparent from an inspection of a species’ surface properties. HPCers can easily accommodate the three-stage procedure Ereshefsky and Matthen describe for identifying biological taxa. HPCers are not ‘dupered’ by ‘sibling species’ like those in the Anopheles complex or the pair Drosophila persimilis and Drosophila pseudoobscura. Dispositional features need not be left out: if flies from a persimilis line are put with flies from a pseudoobscura line, they do not interbreed [though] they are phenetically almost indistinguishable (Ridley 2003, 368).

Precisely the discovery of a causal pattern ... that belies the initial morphocluster — two distinct homeostatic mechanisms ... prompts us to revise our initial phenetic grouping (Ereshefsky and Matthen 2005, 6). Such revisions are not only common, but often instrumental in discoveries of considerable practical importance.

6. Mechanism and its Absence

But before we place undue weight on such “mechanisms,” we ought to note that there is often much more to the “discovery” of sibling species. Discovering the reproductive isolation of D. persimilis and D. pseudoobscura was only the tip of the iceberg. As Mayr reports, though initially thought to be physically identical,

on closer study one morphological difference after another is discovered, and these are fortified by ecological differences. Drosophila persimilis, for instance, has a more northerly distribution than does D. pseudoobscura, and is more frequently found at higher altitudes, showing a preference for lower temperatures. The two species also differ in their diurnal activity rhythm, in their reaction to light, and in other ecological-physiological characteristics. (1963, 35)

That such discoveries should be made is no surprise: for if members of the two species really were morphologically and behaviorally indistinguishable (simpliciter), it should be a miracle that they would segregate into reproductively isolated populations in the same context. We can, of course, see these differences as part and parcel to the homeostatic mechanisms holding the sibling species apart. But must we?

Mayr’s presumption, of course, (as the most outspoken defender of the biological species concept) is that the discovery of the existence of homeostatic mechanisms preserving the reproductive isolation of the Drosophila or Anopheles species (and thus the stability of their species properties), compels their basal separation. It does not compel their separation as natural kinds, however: we might very plausibly regard the genus as a natural kind. Suppose we do: what is the homeostatic mechanism “holding together” the cluster of properties we initially identify in the first-stage morphocluster? Not a propensity to interbreed. Ereshefsky and Matthen suggest that the two separate “interbreeding structures share a common historical origin and are subject to very similar environmental pressures: this is why members of the two species are similar to each other” (2005, 6). The homeostatic mechanisms Boyd and Millikan envisage are thus not only possibly extrinsic, but they may be hierarchically nested.

But this move stretches the meaning of “homeostatic” and “mechanism” considerably. Surely we cannot say that certain selective “pressures” (there’s a paradigm example of biological metaphor) and common environmental context “hold together” the cluster of properties typical of members of the Drosophila complex in the face of environmental change. What, then, is a homeostatic mechanism and why is it so important? Are gene-flow, dispositions (or mechanical ability) to breed only with similar organisms, common selective factors and the like properly regarded as mechanisms? Different species are indeed (often very subtly) differently disposed in, e.g., their breeding habits, but regarding such differences as, e.g., mechanisms for protecting a species gene pool smacks of anthropomorphism. Granted: the different dispositions do sometimes result in populations’ phenotypic stability over considerable stretches of time — and perhaps in this sense are part of the causal structure of the world — but that does not at all imply that that is what they are for.

Rather than struggling to cleanse ‘mechanism’ of such normative contamination, I suggest we let go of the dogma that causal mechanism must underpin the observation of stability. Many traits are stably clustered to over considerable stretches of time — and perhaps in this sense are part of the homeostatic mechanism holding the sibling species apart. But...
and dispositions may indeed call out for explanation, but it’s not clear that a particular mechanism is always the best explanation. When I call my bank about a spurious service fee and am repeatedly directed to the wrong department, I may well suspect that my misdirection was no accident: “there must be a mechanism in place to encourage me to give up!” But an obvious alternative explanation is available: that as a matter of purely contingent fact, people who happen to work for this bank are incompetent and the phone system confusing.

In the case of species, it is often easier to identify mechanisms pulling a species apart. Selection is often disruptive; individuals on the extreme ends of a trait parameter may have an evolutionary edge over their more “moderate” kin (see below). Such selective regimes are quite implausible as “homeostatic mechanisms”; nor need they be “heterostatic”. Nevertheless, I suggest that we can see even populations subject to “heterodynamic” regimes as natural kinds.

Disruptive selection in the seedcracking finch *Pyrenestes ostrinus*. (a) Beak size distribution; (b) General body size distribution; (c) Fitness shows “twin peaks” corresponding to the peaks and valleys in the frequency distribution in (a). (Figure and caption paraphrased from Ridley 2003, 80)

One might be tempted to object here that as soon as we grant the workings of these “heterodynamic” forces pulling apart clustered properties, we see right away that there must be opposing homeostatic mechanisms (intrinsic or extrinsic) at work as well. For we all agree that the biological world (indeed the natural world at large) displays lots of “property clumping”! The existence of homeostatic mechanisms is an even better explanation of this clumping when we acknowledge the existence of heterodynamic forces. But this doesn’t follow. Clearly much turns on the timescale with which we are concerned. The fact of short-timescale stability in the face of disruptive selection needn’t imply the imperfect workings of homeostatic mechanisms ultimately fighting a losing battle. Material science furnishes a useful analogy: diamond, that most valuable allotrope of carbon, is not thermodynamically stable. Diamonds (like paparazzi-worn celebrities) would “rather” slide into the anonymity of amorphous carbon. But this transition is so slow (ordinarily) as to “mimic” genuine stability: diamonds are in this sense “metastable”. Even in cases where populations are gradually diverging, the relevant property clusters may exhibit what we might term “biological metastability”. This may in fact be the norm for species and may — depending on our interests, intentions, and such — suffice to accommodate our inductive and explanatory practices. Mimicked stability may be, for the purposes of accommodation, stability simpliciter.

15 Though in some cases this inertia resembles that of Aristotelian physics — with traits atrophying “when no selective forces work to maintain them” —, a more Newtonian model seems widely appropriate.
16 Material scientists have been attempting to coax hydrogen into a metastable metallic solid that superconducts at room temperature.
17 I suspect that Gould’s punctuated equilibrium model of speciation supports this claim.
7. Accidents Happen (and Can Ground Inductions)

One might worry that "biological metastability" fails to offer the sought-after foundation for inductive inference. Without homeostatic mechanisms holding the relevant properties together, we have nothing better than your dumb luck at the roulette table to ground our inductions. Surely this is "accommodation on the cheap" — and as we well know, cheap is often not worth the price.

But there is a disanalogy to exploit between the roulette wheel generalization and (relatively) stable persistence of certain property clusters. Not every accident is like the accident of the lucky roulette wheel. Much has been made of the difference between laws and merely accidental generalizations and the difficulty to tell it. It was said that only laws can "support counterfactuals", ground inductions, or afford explanations. But this won't do as the distinction. Accidents can be explanatory: my accidentally washing my new red t-shirt with my whites might very well explain why all my formerly white shirts are now pink. When I take the first pink shirt out of the wash, I quickly infer with a sinking heart that they are all pink for precisely this reason. As Lange (2000; 2005) points out, accidents also exhibit stability — it is not even true that accidents have a narrower range of stability than laws.¹⁸

The stable clustering of properties in biological cases may similarly be (in a certain sense) a matter of historical accident. Lange mentions the belief among anthropologists that "any person of entirely Native-American heritage is blood type O or blood type A" (2000, 13). Though a historical accident — "research has suggested that all Native Americans are descended from a very small band that crossed the Siberia-Alaska land bridge, and as it happened, allele B was not represented in that company" (ibid.) —, that accident features a broad range of counterfactual stability. To modify another of Lange's examples (2004, 106), doctors might report that a certain Native-American patient would still have gone into anaphylactic shock if the transfusion of type B blood had been administered sooner, or administered along with a different concentration of saline, or what have you.

Recall our Bornean carnivore: we observe an organism with certain morphological qualities: a long muscular tail and hind legs, a red-ish coat, a certain gross structure, &c. Say we later catch it and discover that it has a number of other distinctive properties and behaviors (some of which explain others). On the strength of these discoveries (and perhaps our background knowledge of the viverrids) we predict where we might find more, how best they can be protected from encroaching civilization, what changes to the ecological web would be most damaging, and so on. What entitles us to these bold projections? After all, perhaps this creature is an oddity — a mutant morph of a known species. Say the HPCers: the warranted inference to the existence of some homeostatic mechanism unifying those properties together.

But this seems too strong. It may very well be that the best explanation of the stable cluster is a mere accident of evolutionary history: a small peripheral group of proto-red-Bornean-carnivores became isolated and evolved a certain cluster of features/behaviors, which in the absence of interfering environmental pressures have been preserved through history. While the co-instantiation of these features is in a very clear sense an accident, it is an accident unlike the red-spinning roulette wheel: the Bornean accident, having occurred in the remote past, is no longer sensitive to counterfactual perturbations. Nor are the counterfactual perturbations to history to which that accident are sensitive (e.g., differences in the environment of its evolutionary parents) relevant to mammalian systematics in the here and now. To modify the Vegas slogan: What happens in the past stays in the past.

8. Conclusions

What emerges from this, I think, is a notion of degrees of stability for various purposes, for even (what may be from a certain vantage point) accidents. Lange uses roughly this point to defend the thesis that there are natural laws concerning particular species (1995; 2000, §8.2–4):

Biological laws of the form 'The S is T' are reliable for the purposes of certain biological disciplines in virtue of accidents of evolution. They are not laws of evolutionary biology. Instead, a claim reflecting the frequency with which various traits are currently presenting the members of a given species functions as an initial or boundary condition in evolutionary accounts of population change. On the other hand, while the inference rules associated with species-specific laws sometimes lead us astray, they represent the products of the best inductive strategies for us to carry out in fields like physiology. They can justly be used to make predictions in those fields (when we have nothing better than a given individual's species affiliation to guide us in predicting its biological properties. (2000, 252–3)

¹⁸ For example, Lange asks us to "Suppose we have laid out on the table a large number of electric wires, all of which are made of copper. Had copper been electrically insulating, then of course, the wires on the table would not have been much good for conducting electricity. Now look what just happened. We had the law that all copper is electrically conductive, and this law obviously would not still have held had copper been electrically insulating. But that counterfactual supposition fails to undermine the accident that all of the wires on the table are made of copper" (2005, 416).
And though I am quite sympathetic to his proposal about laws, I do not think that one needs to follow accept Lange's whole account of natural laws (and autonomous laws concerning particular species) to appreciate the insight that it's the stability of properties that matters most for induction and explanation. Moreover, properties which are clustered together by sheer chance can feature a degree of stability owing to their insensitivity to counterfactuals relevant to our purposes.19

When it comes to the accommodation of our “best inductive strategies”, we needn't do any better than zero in on clusters of properties and dispositions which are as stable as they need to be to render those inductive strategies tolerably reliable. Of course, if we can do better than this and accommodate our inductions and explanations to causally-sustained or nomically regularities, then great. More power to us — so long as we are careful to check our positivist sympathies at the door. Boyd himself seems open to convincing, when he writes of these regularities, that they

need not be eternal, exceptionless, or spatiotemporally universal in order for our epistemic success with them to require the sort of explanation provided by the theory of natural kinds. Whatever philosophical importance (if any) there may be to the distinction between, on the one hand, causally sustained regularities and the statements that describe them, and, on the other, LAWS (Ta! Ta!), it is not reflected in the proper theory of natural kinds. (Boyd 1999a, 152)

I agree with Boyd that the distinction between causally sustained and nomically sustained regularities is unimportant for an account of natural kinds. I think we can do Boyd one better: it is not even important that the regularities be causally sustained. Lipton expresses a similar sentiment when he comments on Kornblith’s application of the HPC account to the problem of inductive knowledge: “Essences are supposed to hold together observable properties in stable clusters, but it is not made clear why this should make for a more inductively knowable world than one where that stability is a brute fact” (1996, 493). The mere fact of cluster stability, it seems to me, would be sufficient explanation for our inductive/explanatory successes.

Invoking notions of homeostasis and causal structure is more trouble and less necessary than it seems. Insisting on these vague underpinnings of stability of a property cluster both risks attenuating whatever there is to these notions and forecloses on applying the HPC account to cases where there is obviously no causal mechanism underlying the stability of a property cluster.20 I believe that a (deflationary) conception of natural kinds as certain sorts of stably co-instantiated clusters of properties sufficiently accommodates our inductive and explanatory practices. Such a view obviously requires a good deal of expansion and defense — but it seems to me to deserve it. How is stability to be characterized? Which properties are relevant? Is it really capable of making sense of what many regard as “historical kinds” (species and perhaps higher taxa) without forcing what Mayr derides as “typological thinking”? Does it in fact provide our inductive practices with any justification?

High hopes aside, I should admit that I am not overly sanguine on the latter score (though that depends in part on my dissatisfaction with externalist theories of justification). On the most natural characterizations of stability (as, e.g., some kind of insensitivity to “counterfactual perturbations”), there will always be a question of how we come to justifiably believe some properties are stably co-instantiated — perhaps also question a of meta-stability (in the sense of a stable cluster’s stability). For as Russell’s chicken discovers at the chopping block, stability for certain purposes, for arbitrary swaths of time can become irrelevant in a gruesome instant.

19 Now obviously, this point raises deep issues regarding the objectivity accorded to natural kinds on this model. For both the choice of properties to which we attend and the degree of stability we recognize as accommodating our inductive practices quite clearly will be a matter of our intentions. This makes room for pluralism; does it leave room for any kind of realism?

20 This last point is nicely illustrated by enantiomers. See my (2005) and (2006).
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