

# Natural Kindness

Forthcoming in *The British Journal for the Philosophy of Science*

Matthew H. Slater  
Bucknell University

matthew.slater@gmail.com  
<http://www.facstaff.bucknell.edu/mhs016/>

Philosophers have long been interested in a series of interrelated questions about natural kinds. What are they? What role do they play in science and metaphysics? How do they contribute to our epistemic projects? What categories count as natural kinds? And so on. Owing, perhaps, to different starting points and emphases, we now have at hand a variety of conceptions of natural kinds—some apparently better suited than others to accommodate a particular sort of inquiry. Even if coherent, this situation isn't ideal. My goal in this paper is to begin to articulate a more general account of “natural kind phenomena”. While I do not claim that this account should satisfy everyone—it is built around a certain conception of the epistemic role of kinds and has a certain obvious pragmatic flavor—I believe that it has the resources to go further than extant alternatives, in particular the Homeostatic Property Cluster (HPC) view of kinds.

- 1 Introduction
- 2 The Fall and Rise of Cluster Kind Concepts
- 3 The Role of Natural Kinds in our Epistemic Practices
- 4 Concern over Causal Mechanism
  - 4.1 Sufficiency Worries
  - 4.2 Necessity Worries
- 5 Stable Property Cluster Kinds
  - 5.1 The Basic Idea
  - 5.2 Two Conceptions of Stability
- 6 Interests and Realism about SPC Kinds

## 1 Introduction

Many metaphysicians and philosophers of science have puzzled over whether our best theories can reasonably be taken to “carve nature at its joints”. A common way of approaching this metaphorical question has been to ask first what Ian Hacking has called a “gentle metaphysical question”: “are there natural kinds—real or true kinds found in or made by nature?” (Hacking [1990], p. 135). Though one could fret about the exact formulation of this question, it seems a reasonable first stab:

A realist should have to lay down a general metaphysical conception of natural kinds before going on to assess our theories' success at attuning our categories to them. Call this the *Realist Presumption*.

One obvious difficulty with the Realist Presumption is that it often seems to put the cart before the horse. Certain metaphysical accounts of natural kinds might entail that categories widely used in a particular science cannot, in fact, count as natural kinds. Whether we interpret this as a problem or as a result depends on our prior commitments. Categories within the biological sciences, for example, have been particularly contentious. What we might call “traditional” conceptions of natural kinds,<sup>1</sup> developed initially with pristine examples from physics and chemistry in mind, accommodate unruly biological categories rather poorly. Some of these problems are empirical: biological species, for example, appear not to have any intrinsic properties which are fit candidates for being essences (cf. Devitt [2008]; Wilkerson [1995]). Other problems are philosophical: even if there *were* properties that all and only members of a particular taxon possessed, it's unclear that we should rightly count them as that taxon's *essence*. Yet the thesis that many such biological categories do, in some sense, “carve nature at the joints” cannot be abandoned lightly. For these categories play important roles in our epistemic practices (in and out of science); we often treat them as objects of discovery rather than merely pragmatic, contingent reflections of how it suits us to portion some homogenous metaphysical pudding.

Realism about *species*, of course, can be defended via alternative means. And indeed, Ghiselin and Hull's Species-as-Individuals thesis (Ghiselin [1974]; Hull [1978]) currently enjoys broad support from both philosophers and biologists. But even if this is the right metaphysic for species,<sup>2</sup> it offers little consolation for those seeking an account of realism about biological categories generally. It wasn't meant to. Nor do I see any plausible, non-trivial way of extending the individualist metaphysic to such biological categories as genes, proteins, cells, tissues, organ systems, races, ecosystems, and so on (cf. Wilson [2005], p. 99; Wilson, Barker, & Brigandt [2007], p. 194); ditto traditional accounts of natural kinds.

It's for these reasons that Richard Boyd's ([1988], [1991], [1999]) Homeostatic Property Cluster (HPC) account of natural kinds has been enthusiastically received—particularly among philosophers of biology who feel the sting of essentialism's failure the most poignantly. Though many have worked to fill in the details of the account (Chakravartty [2007]; Griffiths [1997], [1999]; Kornblith [1993]; Wilson [1999], [2005]; Wilson, et al. [2007]), several worries and open questions about the account remain (see, e.g., Ereshefsky [2010]; Ereshefsky & Matthen [2005]). Not all of these purported problems are actual, in my view. I won't be able to argue for this in the present paper, however. Instead, I focus on concerns involving the HPC account's reliance on the idea of causal homeostatic mechanisms. In brief: while it may often make sense for us to describe our classificatory activities in terms of our seeking out causal mechanisms underlying natural kind phenomena,

---

<sup>1</sup> By which I have in mind accounts which construe natural kinds as individuated by intrinsic essences.

<sup>2</sup> I am skeptical; see my ([2014])

making causal mechanism the focus of our *account* of natural kinds unduly restricts the application of HPC theory. I will argue that such mechanisms are neither necessary nor sufficient to underpin these activities.

Happily, considering these problems leads the way to a more general and flexible account of natural kinds. I propose that we drop the emphasis on mechanism and instead focus on what mechanisms were supposed to *offer* to a cluster of properties: a certain kind of cohesiveness or stability. In virtue of this change of emphasis, my account does not advertise itself as an account of a *kind* of natural kind—it is, I think, an attractive candidate for a *general* natural kind concept, able to accommodate the diversity of natural kinds we find in the world.

Is this a major or minor revision I am proposing? Insofar as homeostatic properties were supposed to play a major role in the HPC account, dropping them can seem a major change and leaves a rather gaping theoretical hole. To plug this hole, at least as a starting point, I'll offer a characterization of the kind of stability that I argue unites the clusters of properties which are natural kinds. Yet in other ways, I am proposing a rather minor revision to HPC—one which I hope will be sympathetically received by advocates of HPC. For one, there is a sense in which the stability I am proposing to make central to the account was already implicitly in view and that a shift of focus and further articulation is all that is required. For two, the account I offer clearly works within Boyd's general cluster framework for understanding how natural kind phenomena integrate into our epistemic practices. At the end of the day, however, I have no significant interest in settling the issue of whether the account I sketch in this paper amounts to a novel theory of natural kinds or a major or minor revision to HPC. For convenience's sake, let us give it a name nevertheless: the *Stable Property Cluster* (SPC) account of natural kinds.

Here is the plan of the paper: I will begin by briefly motivating the cluster approach to natural kinds, describing how it fell initially to the neo-essentialism of Kripke and Putnam but was later reinvigorated by Boyd (§2). Because previous discussion of neo-essentialism tended to focus on the exciting semantic and metaphysical theses Kripke and Putnam proposed—some thought it a resurrection of Aristotelian essentialism—the affinity between essentialist kinds and HPC kinds is sometimes under-appreciated. But the accounts share important common ground on the specific role of natural kinds in our inferential practices and how their metaphysics suits them to this role (§3). After describing my worries about the HPC account's invocation of causal mechanisms (§4), I outline a conception of stability for property cluster kinds (§5) and explore the ways in which the SPC account of natural kinds supports and undercuts the Realist Presumption (§6). My final suggestion will be that we need an account not of natural kinds *per se*—not a detailed answer to Hacking's Gentle Metaphysical Question—but an account of “natural kindness”: a kind of status things can have that partially underpins their role in our inferential practices.

## 2 The Fall and Rise of Cluster Kind Concepts

Recall that cluster accounts of reference were seen as an improvement on what Putnam called the “traditional view” according to which the meaning of kind terms is given by a conjunction of properties (Putnam [1975], p. 140). Whereas Descriptivism famously encountered trouble when it came to the modal and epistemological status of these properties, cluster accounts seemed to fare better on this count (Searle [1958], p. 160). Relaxing the requirement that all of the descriptive properties be possessed by the bearer of a particular proper name, the proponents of cluster accounts allowed that a name might be associated only with a *cluster* of descriptions none of which were necessary for successful reference. However, as Kripke ([1980]) argued rather convincingly, it seems that we *can* successfully refer to, say, Aristotle even if we are dramatically misinformed about his accomplishments. Even the loose, cluster variety of Descriptivism seems to conflict with compelling modal/semantic intuitions that Kripke saw as motivating the causal theory of reference.<sup>3</sup>

The popularity of Kripke and Putnam’s “neo-essentialism” about natural kinds stemmed partly from their success in translating these intuitions about the semantics of proper names to natural kind terms. Cluster accounts of kind-membership look like they’d face analogous difficulties. Might not we just be mistaken that a certain kind of stuff is correctly described by even a cluster of properties? Perhaps. But the tenability of this possibility turns on the intuition that we can, as it were, keep our finger on the misrepresented stuff in different circumstances (across possible worlds, and so on)—in other words: that there is some underlying *essence* of what it is to be stuff of this kind. When it becomes difficult to maintain this intuition, the temptation to abandon the cluster approach tends to diminish.

The essentialist intuition—whatever the status of its application to specific cases—has two main features. One is a claim about the semantic role of essences just mentioned: that when we refer to kinds, we do not refer to things possessing the superficial properties which might normally be taken to describe the kind, but to things possessing a “deeper” level of similarity. Another claim has a metaphysical-explanatory character and is arguably more central to essentialist kinds’ putative role in science. Essences explain why properties “clump” together. The properties commonly associated, but not definitive, of a kind—its “nominal essence”—are the effects of a common cause: the instantiation of its “real essence”.

The existence of natural kind essences would thus explain not only why cluster accounts of kinds would have been tempting in the first place, but why such clusters were not ephemeral, accidental features of the world. This, in turn, helps explain why kinds often seem to play a role in explanation and inference. Were the clustering of a bunch of properties associated with a kind a mere accident, inferring that a certain object had some of those properties from the fact that it was of a particular kind would be unreliable. Likewise, such accidents would be devoid of explanatory force.

The essentialist explanation of clustering is so good—and apparently so prevalent (in certain domains, anyway)—that it is tempting to suppose that it is the *only* possible explanation. Devitt

---

<sup>3</sup> Not that those intuitions *necessitated* such a theory, of course.

seems to express this attitude in his argument for intrinsic biological essentialism. The law-like truth of generalizations about biological taxa (such as ‘Indian rhinos have two horns’) demands explanation: “There has to be something about the very nature of the group . . . that, given its environment, determines the truth of the generalization. That something is an intrinsic underlying, probably largely genetic, property that is part of the essence of the group. Indeed, what else could it be?” ([2008], p. 352). But why suppose that there is any *one* explanation for property clustering, much less that it is the existence of essential properties attaching to the kind?<sup>4</sup>

What seems clear is the truth of essentialism’s explanandum: The world exhibits a lot of property clustering. As Anjan Chakravartty puts it:

Properties, or property instances, are not the sorts of things that come randomly distributed across space-time. They are systematically “sociable” in various ways. They “like” each other’s company. The highest degree of sociability is evidenced by essence kinds, where specific sets of properties are always found together. In other cases, lesser degrees of sociability are evidenced by the somewhat looser associations that make up cluster kinds. In either case, it is the fact that members of kinds share properties, to whatever degree, that underwrites the inductive generalizations and predictions to which these categories lend themselves. (Chakravartty [2007], p. 170)

Essences explain one end of the spectrum of property “sociability” phenomena. But what, if anything, accounts for the *looser* societies of properties?

Here’s where Boyd’s HPC account of natural kinds apparently shines. 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. The natural definition of one of these homeostatic property cluster kinds is determined by the members of a cluster of often co-occurring properties and by the (“homeostatic”) mechanisms that bring about their co-occurrence. (Boyd [1991], p. 141)

By emphasizing the role of causally-sustained sociability, the HPC account purports to solve a “Goldilocks” problem for natural kinds: explaining both the “non-accidentality” of some property clustering and the manner in which even such clustering can nevertheless be imperfect. This imperfection in turns accords just the sort of flexibility that allows the HPC account to make sense

---

<sup>4</sup> Devitt seems to hold open the possibility that homeostatic property clusters might count as essences ([2008], pp. 350–1).

of our classificatory practices in domains (such as biology) that have proved challenging to the essentialist.<sup>5</sup>

We will circle back to consider the role of homeostatic mechanisms for the HPC account shortly. First, it is worth asking after its intended breadth. Is Boyd offering us a *general* account of natural kinds or something more specialized? It's sometimes difficult to tell. Like Chakravartty, Hilary Kornblith—an early advocate of the HPC view—describes the view in general terms: “Natural kinds involve causally stable combinations of properties residing together in an intimate relationship” ([1993], p. 7). On the other hand, Boyd's above description suggests a more circumscribed intent.<sup>6</sup> Wilson, Barker, and Brigandt take the cue; in their discussion of stem cells, they list a number of characteristics typically possessed by stem cells, noting that “there are exceptions, so that the above describes a genuine HPC kind” ([2007], p. 218). This makes it sound as if rather than HPC kinds *tolerating* the non-instantiation of some of a cluster's properties in a member of the kind it characterizes, such lapses were in fact *required* for that kind to be one of the scruffy yet hip HPC underground.<sup>7</sup>

Whatever Boyd and company's intent, it seems doubtful that the HPC account can straightforwardly serve as a *general* account of natural kinds—for two (nested) reasons. First, it seems quite odd to think of essences as homeostatic mechanisms (in anything but a dramatically weakened sense) maintaining the stability of a cluster of properties. Suppose the essentialists are right that the essence of water is having the molecular structure commonly denoted ‘H<sub>2</sub>O’: it's in virtue of having this structure that something is a water molecule with all the superficial properties associated with that kind. In what sense, however, is that structure also a causal *mechanism*? Such essences clearly fall short of even very general accounts of mechanism on the market (such as those of Bechtel [2006]; Machamer, Darden, & Craver [2000]; Woodward [2002]). Second, some natural kinds are not plausibly thought of as being defined by causally-united properties at all. The elementary particles, for example, appear to be individuated by perfectly-maintained suites of properties, none of which are derivative from any others. Perhaps it is simply a fundamental law that an up quark has a spin of  $\frac{1}{2}$ , charge of  $\frac{2}{3}$ , baryon number of  $\frac{1}{3}$ , mass of  $360 \text{ MeV}/c^2$ , and so on (Lange [2011], p. 54). For these reasons, it seems wise to interpret HPC kinds as a subgroup of kinds.

Adopting this sort of compartmentalized stance about HPC kinds has its strategic advantages in any case. For one, it allows essentialists and defenders of other accounts to reign more or less

---

<sup>5</sup> The HPC account actually exhibits a couple of dimensions of flexibility. Wilson *et al.* note its “natural flexibility” repeatedly ([2007], pp. 190, 197, 207), referring both to the ability of a cluster to include extrinsic as well as intrinsic properties and to allow some of them to go missing. In the next section, I shall offer a few reasons for thinking that we can and should increase this flexibility further.

<sup>6</sup> Some have suggested to me that Boyd saw what he was doing as more expansive than this—that he was offering a maximally general account of natural kinds. Whether or not this is Boyd's view, this stance is unreasonable for reasons I will summarize shortly.

unchallenged in their separate fiefdoms. If a particular example of a purported natural kind fits poorly in the HPC mold, the HPCer needn't press the matter. She can shrug and admit that the purported kind may not be an HPC kind while maintaining that *some* kinds are. While there is nothing inappropriate about this maneuver, I think that we can do better. I am not alone. Alexander Bird also suggested that the HPC account

can be extended to all natural kinds. The laws will explain why there are certain clusters; they will also explain the natures of those clusters—the loose and vague clusters in biology, the partially precise clusters of chemistry and the perfectly precise clusters of particle physics. Boyd introduces his idea in order to provide an alternative to the essentialist view of natural kinds. However, if I am right, the homeostatic property cluster approach can be expanded to include the essentialist view in respect of the kinds to which it applies. ([2007], pp. 210–211)

Unlike Bird, however, I believe that we will need to modify the foundations of the HPC account in order to generalize it to all natural kinds. My proposed modification drops the requirement of homeostatic causal mechanisms in favor of a flexible notion of stability for property clusters.

If this modification was motivated only by the desire only to expand the HPC account's scope, it would be of limited interest. However, I will argue that it also addresses concerns facing even the narrow interpretation of the HPC account. To contextualize my proposal, it's worth stepping back for a moment to consider what makes HPC kinds a particular *variety* of natural kind. Why are they not a different phenomenon altogether? The most straightforward answer—and an area of consensus between HPCers and the neo-essentialists—involves the epistemic role that natural kinds are supposed to play. To this we now turn.

### 3 The Role of Natural Kinds in our Epistemic Practices

A centerpiece of Boyd's account of natural kinds is his Accommodation Thesis: "the theory of natural kinds is about how schemes of classification contribute to the formulation and identification of projectible hypotheses" (Boyd [1999], p. 147). This stance seems at least implicit in recent history of thinking about natural kinds (Hacking [1991]), extending into the neo-essentialist accounts of the 1970s and forward. We see in Putnam, for instance, an explicit tying together of the metaphysics and epistemic role of natural kinds: they are "classes of things that we regard as of explanatory importance: classes whose normal distinguishing characteristics are 'held together' or even explained by deep-lying mechanisms" ([1975], p. 139). This is very much of a piece with Boyd's view: this "holding together" of properties associated with kinds what affords their explanatory and inferential importance.

---

<sup>7</sup> Even their title, "When Traditional Essentialism Fails", suggests that HPC is a sort of liberal fallback account of natural kinds—reserved for when things get too rowdy for the tidy, conservative essentialist account to manage.

Two important questions arise at this juncture. One question concerns the interpretation of the phenomena gestured at using such metaphors as “clustering”, “holding together”, “sociability/intimacy” and how exactly such phenomena contribute to our epistemic projects. A second question asks after the metaphysical explanation for these phenomena. Though the questions blur together to some extent, we might think of the first question as directed at articulating a particular explanandum (the existence of a clustering phenomenon—or phenomena—and its connection to some of our epistemic practices) and the second as its explanans: what *accounts* for the clustering/sociability of properties in virtue of which we enjoy a measure of inductive success.

Clearly, the second question has received the lion’s share of philosophers’ attention. This is in one sense unsurprising. After all, the above metaphors are evocative (if imprecise) and it might be regarded as reasonably apparent that the “holding together” (somehow understood) of a bunch of characteristics would contribute to our epistemic lives. More exciting is the prospect of connecting these epistemic projects with some underlying metaphysics and semantic theory. Forging such a link would not only fit into the broader naturalistic project in epistemology<sup>8</sup> but would also shore up scientific realism—so some suppose, anyway.

Both the HPC and the essentialist accounts can be interpreted as emphasizing the second question, focusing on the *something*—be it an essence, a homeostatic mechanism, or some other feature of “the causal structure of the world”—they believe must underlie or explain the sociability of a set of properties. As Boyd put it in an early paper: “Kinds useful for induction or explanation must always ‘cut the world at its joints’ in this sense: successful induction and explanation always require that we accommodate our categories to the causal structure of the world” (Boyd [1991], p. 139).

The emphasis on the second question over the first is in another way surprising, however. For in judging whether some account of natural kinds can satisfy the accommodation demands of a given discipline, don’t we need a precise understanding of the phenomena that the account is supposed to save?<sup>9</sup> Perhaps those pursuing the second question presume that offering an account of the metaphysics of kinds would automatically shed light on their epistemic roles.

The asymmetric emphasis on the metaphysics of “clustering” also reflects a popular strand of thought in general discussions of the problem of induction—especially in those of the “explanationist” tradition pioneered by Harman ([1965]).<sup>10</sup> Suppose that every F we’ve ever observed has been G. Given an amenable background, we may be inclined to infer that *every* F is a

---

<sup>8</sup> Kornblith begins his ([1993]) book, *Inductive Inference and its Natural Ground*—an extension and discussion of Boyd’s HPC account—with an epigraph from Quine’s paper “Natural Kinds”: “For me then the problem of induction is a problem about the world: a problem of how we, as we now are (by our present scientific lights), in a world we never made, should stand better than random or coin-tossing chances of coming out right when we predict by inductions which are based on our innate, scientifically unjustified similarity standard” ([1969], p. 127). Accordingly, Part I of Kornblith’s book is entitled “What Is the World That We May Know It?”.

<sup>9</sup> Lange ([2000], p. 4) pressed a similar question in response to popular reductive approaches to natural laws; see also (Lange [2005c]).

<sup>10</sup> See (White [2005]) for a clear and detailed discussion of the strategy of using explanation to guide and justify.



G. But what justifies this inference? The explanationist purports to offer a way of avoiding (or delaying) the inductive skeptic. If the best explanation of our observations of only G Fs is that *all* Fs are G, then we are (defeasibly) entitled to infer that this explanation is correct.

Explanationists recognize that philosophical care is required here. For one, it must be clear that we are explaining a fact about our *observations* (that we haven't ever seen a non-G F), not a fact concerning the G Fs we've seen. That this particular frog is green is not explained by the fact that *all* frogs are green; ditto for all the observed frogs (Peacocke [2004], p. 139).<sup>11</sup> For two, it seems to many in this tradition that some kind of "non-accidentality" condition must be met. Peacocke offers the following illustration:

Suppose one hundred spinnings of a roulette wheel are spinnings in which the ball lands on red, and suppose we observed the first fifty spinnings. The fact that all of the hundred spinnings ended with the ball landing on red is sufficient to explain why all the fifty observations of spinnings are ones in which the ball landed on red. But an inductive inference to the fifty-first spinning that it will end with the ball landing on red is unsound. The generalization does give the explanation of our evidence, but we are not entitled to the inductive inference if we know the wheel to be unbiased.  
(*ibid.*)

What is required, argues Peacocke, is a commitment to the existence of "some condition C that explains why all the Fs are G" (p. 141).

Concerning natural kinds, 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], p. 17). Kornblith puts the point this way:

Inductive inferences can only work, short of divine intervention, if there is something in nature binding together the properties which we use to identify kinds. Our inductive inferences in science have worked remarkably well, and, moreover, we have succeeded in identifying the ways in which the observable properties which draw kinds to our attention are bound together in nature. In light of these successes, we can hardly go on to doubt the existence of the very kinds which serve to explain how such successes were even possible. ([1993], p. 42)

While I am sympathetic to Kornblith's line on the source of our confidence that there are such kinds, I disagree with the claim that the epistemic value of natural kinds is contingent on the

---

<sup>11</sup> As White ([2005]) points out, some philosophers deny this. Harman ([1973]), for instance, urges a distinction between explanation and causation: while the greenness of a particular frog is not *caused* by the fact that all frogs are green, the generalization does indeed *explain it*.

existence of some concrete ground—some essence, mechanism, or feature of the causal structure of the world—that Kornblith believes “binds together the properties which we use to identify kinds”. Call this *The Grounding Claim*; its broad acceptance seems a likely explanation for the emphasis on the metaphysics of natural kinds to the comparative neglect of the precise epistemic role they play.

Before building a case against the Grounding Claim and laying out my alternative approach to kinds, it’s worth distinguishing between two “epistemic roles” natural kinds might be taken to play in our inductive practices (leave aside our explanatory practices for the moment). Very optimistically, we might see the identification of natural kinds as providing us with rational justification of inductive inference. Evaluating the tenability of such an “ontological solution to an epistemological problem [the problem of induction]” (Sankey [1997], p. 239) is beyond the scope of this essay.<sup>12</sup> But I don’t think we have to see natural kinds as issuing inductive warrant in order to see them as playing a necessary role in certain sorts of inductive inferences. Modest realists, for instance, can interpret the “projectibility” of certain categories as amounting to a metaphysical fact about those categories and an epistemological matter concerning our recognition of this fact (presumably in the context of much background knowledge).<sup>13</sup> It is this latter role I would like to focus on in this paper. What features must kinds have in order to be “apt” for inductive inference in this latter sense? This is the question I will aim to answer in §5 in offering a new, broadly unifying account of natural kinds—or, as I prefer to put it, “natural kindness”. But first, let us begin to consider the plausibility of the Grounding Claim as put to work by the HPC approach to kinds.

#### 4 Concern over Causal Mechanism

Whereas Essentialists typically see microstructural essences as binding together the properties associated with a natural kind, HPC advocates see causal homeostatic mechanisms as playing that role. In each case, though, this metaphysical grounding is supposed to be responsible for kinds’ epistemic potential. The shift from essences to mechanisms purportedly allows for the greater flexibility familiar in HPC kinds while still accommodating our inductive practices (in accordance with the Grounding Claim).

However, I will argue that there is cause for skepticism on this latter count: the existence of the causal mechanisms proffered by HPCers are (alone) neither clearly necessary nor sufficient for the stable clustering thought to underpin cluster kinds’ projectibility. I address the sufficiency concern first (in §4.1), describing a circularity and regress problem for the HPC account. Then (in §4.2), I describe cases that suggest that that the relevant kind of stability can be had without the mechanisms. This will serve as my main argument against the Grounding Claim and will lead us

---

<sup>12</sup> I will say that I am not this optimistic; see (Beebe [2011]) for a nice discussion of how neo-essentialists such as Ellis ([2001]) fail in their attempts to solve the problem of induction.

<sup>13</sup> ‘Projectibility’ has of course been used ambiguously to refer just to the metaphysical fact (in which case a category might be projectible without our knowing it) and to the “composite” fact that adds our (justifiable) willingness to actually formulate inductive projections using the category.

into a positive proposal for what I believe is a more satisfying and general approach to natural kinds (§§5–6).

#### *4.1 Sufficiency Worries*

Ideally, in order to answer the question of whether causal homeostatic mechanisms can serve as the ontological ground for the epistemic fertility of (a subclass of) natural kinds, we would draw on a precise account of how to characterize the phenomenon (or, possibly, phenomena) metaphorically described as “clustering”, “holding together”, “sociability”, and so on. Unfortunately, little has been said on this subject. I will offer my own take in §5.2; but for the purposes of this section, I simply take as my target the intuitive idea (gestured at via the metaphors) that homeostatic mechanisms are always sufficient to underpin a sort of counterfactually stable (if imperfect) clustering of some properties in virtue of which HPC kinds are projectible.

A parallel difficulty is that the concepts of a “causal homeostatic mechanism” or “the causal structure of the world” have been little more clarified. But even with a specific and uncontroversial account of causal mechanism in hand, some worry whether such mechanisms will be able to play the grounding and individuating roles HPCers envision. Carl Craver ([2009]) has argued, for example, that contemporary accounts of causal mechanism (e.g., Bechtel [2006]; Bechtel & Abrahamsen [2005]; C. Craver [2007]; Machamer, et al. [2000]) lack the resources for generating objective divisions between kinds. For it is not always clear whether two phenomena are expressions of the same kind of mechanism or where one mechanism begins and another ends. This is particularly problematic for HPCers (such as Griffiths [1997], pp. 171–174) who emphasize the importance of dividing HPC kinds along lines of causal mechanism. Craver puts the worry in terms of “lumping” and “splitting” purported kinds on the basis of investigations into the mechanisms which maintain those kinds’ property stability: “one can be led to lump or split the same putative kind in different ways depending on which mechanism one consults in accommodating the taxonomy to the mechanistic structure of the world” (2009, 583).

So if Craver is correct that “human perspectives and conventions enter into judgments about how mechanisms should be typed and individuated” (591), then it would appear that what natural kinds on the HPC view will depend on those perspectives and conventions, leading to a denial of the Realist Presumption and what many will regard as an unacceptably conventionalist pluralism about what kinds there are. As will become clear shortly, I am less concerned about the sort of conventionalism Craver believes the HPCer is saddled with. Though it will have different causes on my account, I believe that at least for many categories, we are forced into a deep pluralism and domain-relativity about natural kinds.<sup>14</sup>

---

<sup>14</sup> Craver’s point, however, should be taken seriously by those unwilling to embrace this level of conventionalism or pluralism: Boyd’s Accommodation Thesis seems to place a normative demand on our practices of classification that is incompatible with the degree of conventionalism arguably involved in individuating mechanisms. To meet this

Conventionalism aside, however, Craver raises a regress worry about mechanism individuation that goes to the heart their theoretical role in the HPC account. In speaking of dividing mechanisms, HPCers presumably have in mind dividing them into *kinds*. Perhaps in some cases—e.g., with species—the self-same mechanism might be thought to maintain the coherence of a property cluster. In many others, however—e.g., cell types (Slater [2013])—the relevant mechanisms would presumably be distinct tokens of the same type. Hepatocytes (cells that make up most of your liver), for example, would be the kind of cell they are in virtue of possessing a distinctive cluster of properties whose stability is maintained by a certain kind of hepatocyte-making/maintaining mechanism. But what account should we offer for understanding these *mechanism kinds*? Here’s how Craver puts the concerns:

Property clusters are united in a kind because their clustering is explained by a single kind of mechanism. When are mechanisms mechanisms of the same kind? If one responds that mechanisms are mechanisms of the same kind when they are explained by a single kind of mechanism, the regress is transparent. If the answer is that mechanisms of the same kind are composed of the same kinds of entities, activities, and organizational features, then we need some way to unite entities and activities into natural kinds. Either way, we only stave off our ignorance of natural kinds a little longer. ([2009], p. 586)

One might attempt to evade the problem by disallowing mechanisms of the same type from doing the individuating work—insisting instead that HPC kinds be defined by individual *instances* of mechanisms (mechanism tokens).<sup>15</sup> Unfortunately, this insistence would lead to a rather revisionary position about many scientific categories. Insofar as distinct physiological mechanisms are responsible for the similarities between hepatocytes in my liver and those in the liver of my wife, these would have to count as distinct kinds of cells.

Another strategy would be to interpret mechanism types as a different sort of kind—either along traditional essentialist or other (possibly *sui generis*) lines. The first option does not appear particularly promising. It’s doubtful that an essentialist account would accommodate a reasonable division of mechanisms into types—not, at least, in the biological world, where bio-mechanism types appear to exhibit as much internal heterogeneity we see in the biological world generally. The second

---

objection, HPCer must either say something substantive in order to reclaim the objectivity of the “mechanistic structure of the world” or weaken the accommodation thesis.

<sup>15</sup> P.D. Magnus suggested this move to me as a way of handling the application of the HPC view to species. I do not take him as an advocate of the view in general, however.

option is impossible to evaluate without some specific candidate account.<sup>16</sup> All things being equal, the HPC account would seem the obvious choice. But this leads us into Craver’s vicious regress.

However, one might object at this point that even if there was *some* regress here, who’s to say that it won’t halt *somewhere*?<sup>17</sup> Might we not entertain the following scenario (see Figure 1)?

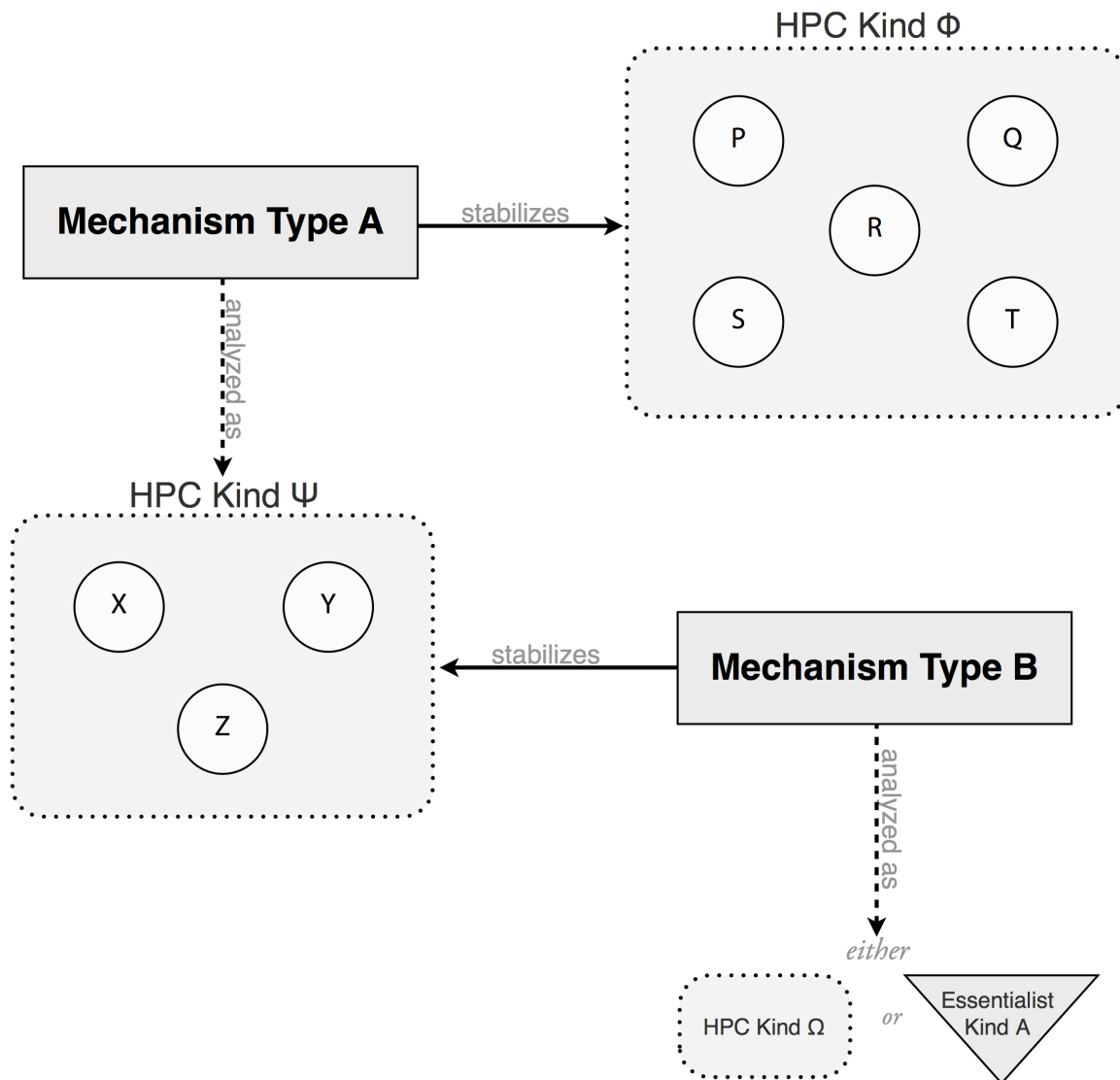


Figure 1: Mechanism Regress

<sup>16</sup> I should note that the problem is exacerbated by attempting to generalize the HPC account. Perhaps this is one reason why HPCers might wish to see their account as describing only a subtype of natural kinds—a subtype that is conceptually parasitic on other natural kind accounts.

<sup>17</sup> Thanks to Marc Lange for suggesting this response and pressing me to expand this discussion.

Suppose that the mechanism type (A) that maintains the stable clustering of the properties characterizing kind  $\Phi$  is best understood as another HPC kind ( $\Psi$ ). Kind  $\Psi$  must in turn be analyzed in the HPC mould as being characterized as a mechanistically-stabilized cluster of properties. The regress looms: another mechanism type (B) requires analysis. Perhaps it too is best characterized as an HPC kind, and so on. But if it can instead be characterized as an essentialist kind, the HPCer is out of the woods.

That this sort of analysis may be possible in some cases, I won't dispute (though I cannot think of a clear example). That it is *always* possible for any given HPC kind seems highly questionable. What, aside from allegiance to some sort of general reductionist stance about biology, would justify such a claim? When we reflect on the fact that the heterogeneity of high-level biological categories is generally recapitulated at lower levels of mechanistic organization, the prospect of halting our regress at something other than a primitive notion of mechanism type is dubious.<sup>18</sup>

The second regress has a more practical character. On the traditional account of natural kinds, essences play the role of a sort of “metaphysical cement” that enables certain characteristic epistemic efforts. Perhaps there are epistemic conditions under which noting that something has properties P, Q, and R—all standard effects of having property E, the essence of a particular kind  $\Phi$ —gives us prima facie warrant for inferring that this thing *is* of kind  $\Phi$  and thus possesses the other properties S, T to which E gives rise. E offers us this warrant insofar as these various causal entailments are themselves known (or justifiably believed) and are non-accidental in some appropriately robust sense.

Even when E's identity is unknown, treating some category as a kind (in this traditional sense) may be tantamount to accepting the existence of something like Peacocke's “condition C”: that there must be *something* that explains the stability of properties P, Q, R, S, T, &c. Possessing good reason for making this commitment—being justified that category  $\phi$  should be treated as a natural kind for certain epistemic purposes—is arguably often part of the background knowledge we rely on when inferring things about members of kinds (Godfrey-Smith [2011]).

On the HPC account, homeostatic mechanisms take over the essence E's role. However, even paradigmatically homeostatic mechanisms—for example, those involved in the maintenance of an organism's physiological states within certain tolerances—need not *themselves* be stable. There are conditions under which those mechanisms fail or simply shut down. When those conditions are well-understood (as they often are in physiological cases), we make allowances accordingly—relativizing the kind, perhaps, to “normal conditions” or implicitly seeing the given mechanism as necessarily embedded in a larger-scale mechanism.<sup>19</sup> The homeostatic mechanisms that maintain the

---

<sup>18</sup> I do not deny, of course, that bio-mechanisms are *constituted* by parts which are decomposable into pieces—molecules, atoms, and so forth—that do not demand a HPC treatment. The present regress concern pertains to *analysis*, however. And it is scarcely obvious that the *analysis* of a particular HPC kind should always terminate in kinds characterizable in non-HPC terms.

<sup>19</sup> Note that if one construes the causal mechanisms as playing an individuating role, we may face tricky questions in the spirit of Craver's original challenge about whether to lump or split different mechanistic ensembles.

stable co-instantiation of a hepatocyte's properties depend on the successful operation of a whole host of enabling mechanisms that keep it switched on. The point is that the mere existence of a homeostatic causal mechanism responsible for clustering some properties does not *by itself* ensure that that clustering will be sufficiently robust to ground our epistemic practices.<sup>20</sup>

To see this, consider an analogy. My guest room has a separate thermostat, a homeostatic mechanism *par excellence*. It is designed to maintain a consistent, comfortable temperature in the room by adjusting the heating and cooling in predictable ways given different circumstances. But it is not homeostatic for *all* circumstances. It needs to be properly hooked up, powered on, and so on. This arrangement has obvious advantages. When I don't have guests, I switch the heat off in the room and allow its temperature to vary according to the weather. The fact that there are conditions under which the mechanism doesn't operate hardly entails that the thermostat isn't a homeostatic mechanism when it is powered on. But clearly, the fact that it *is* a homeostatic mechanism (when powered on) won't explain the stability of the room's temperature unless we know that it is likely to *stay* powered on. If my wiring is temperamental, my guests are likely to complain about the inconstant temperatures during their stay. Even if the thermostat itself has a tendency to occasionally short out, we still might consider it a homeostatic mechanism. Suppose that in fact it *doesn't* short out during my guests' stay. Does the fact that the heating and cooling of the room is regulated by a homeostatic mechanism constitute a full explanation of the stability of the temperature in the room during that period? I don't think so. What is needed, in addition, is some explanation of the absence of whatever suffices to short out the thermostat (perhaps only bad luck).

Many homeostatic mechanisms have this character, operating only for a time or in some but not other conditions. They can be homeostatic in some respects but flighty or unreliable in others. Simply stipulating that there be some mechanistic explanation for the clustering of some properties does not clearly ensure that such clustering will be stable. Nor does it help to pile on further mechanisms—for this will just initiate a regress. Suppose that it's very important that my guest room remain a comfortable temperature for a time. Being lazy but technologically ingenious, I design a robot to watch the thermostat and switch it back on whenever it craps out. But now we must ask after the stability of the robot-watcher's mechanism. And what watches the watcher? Another robot, perhaps? The threatened regress can of course be stopped at any stage by offering a mechanism that *guarantees* the sort of stability that accommodation to our epistemic practices demands (whatever this turns out to be). But now we are elsewhere from a *mere* homeostatic causal mechanism. Rather than attempting to offer an account of the sort of homeostatic mechanisms we need, however, I will propose in §4 that we would do better to circumvent mechanisms altogether and simply focus on articulating a conception of the sort of stability a property cluster needs to possess in order to serve

---

<sup>20</sup> Again, I must ask for your patience in waiting until §5 for a fuller story about what such robustness comes to. I will briefly readdress the issues broached here at that point.

the epistemic functions we are used to natural kinds serving. Before defending this proposal, we consider one more class of concerns with the HPC approach.

#### *4.2 Necessity Worries*

My aim in the previous subsection was to cast some doubt on the sufficiency of causal homeostatic mechanisms to ground our epistemic practices. Now I want to press on their necessity. As I mentioned above, many scientifically important categories—such as elementary particles or chemical species—are associated with clusters of properties whose stability is not plausibly maintained by causal homeostatic mechanisms. HPCers can handle such cases easily by simply restricting the scope of the HPC account to cover only the categories where essentialism fails. But there remain some cases apparently within HPC’s purview for which it is difficult to make out the activity of causal mechanisms.

Consider species. Long regarded as paradigm cases of natural kinds, the neo-essentialists assumed that each species taxon could be defined by some shared real essence which bound together the nominal essences associated with the taxa. As before, the HPC account offers a parallel explanation for the “clumpyness” of such properties. Here is Boyd:

It is, I take it, uncontroversial that biological species . . . 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 isolations from others, effects of common selective factors, coadapted gene complexes and other limitations on heritable variation, developmental constraints, the effects of the organism-caused features of evolutionary niches, and so on—act to establish the patterns of evolutionary stasis that we recognize as manifestations of biological species. ([1999], pp. 164–165)

While it is true that evolutionary theory provides us with many candidate explanations for the degree of similarity we see between the organisms of a species, it is not obvious that these explanations (or ensembles of them) can be interpreted in mechanistic terms.<sup>21</sup> Talk of evolutionary “pressures”, “forces”, and “mechanisms” is natural, but the appropriateness of these interpretations are controversial. Take, for example, the reference to “common selective factors” which (I presume) are suppose to constitute a stabilizing mechanism (in virtue of its selecting out organisms whose traits are not within a certain adaptive range). In recent years, a controversy has raged about whether to interpret natural selection as a *cause* of evolutionary change (the “dynamical interpretation”) or whether it should instead be interpreted as a non-causal, statistical properties of populations (the

---

<sup>21</sup> And again, HPCers wishing to see mechanisms as individuating species kinds will face some tricky questions about how to divide the overlapping influences of such mechanisms.



“statistical interpretation”).<sup>22</sup> Denying a causal interpretation of natural selection requires (on any reasonable account of mechanism) denying that natural selection should be construed as a “mechanism”. The matter does not turn solely on the fortunes of Statisticalism, however. Even some of Statisticalism’s critics have argued that “neither of the two main conceptions of mechanism adequately captures natural selection as a mechanism” (see also Barros [2008]; Havstad [2011]; Skipper & Millstein [2005], p. 328).

The situation is more complicated at different taxonomic ranks. Consider sibling species: morphologically (nearly) indistinguishable species. Mayr reports in his classic discussion of the fruit flies *Drosophila persimilis* and *Drosophila pseudoobscura* that though initially thought to be physically identical, a number of differences were eventually discovered ([1963], p. 35). His presumption, of course (as a trenchant defender of the biological species concept,<sup>23</sup> is that the discovery of the existence of homeostatic mechanisms preserving the reproductive isolation of the two *Drosophila* species (and thus the stability of each species’ properties), compels their basal separation as distinct species. It does *not*, however, compel their separation as natural kinds: for we might very plausibly regard the whole *Drosophila* genus as a natural kind encompassing both species kinds (Boyd [1999], p. 176).

Suppose we do regard *Drosophila* as a natural kind: what is the homeostatic mechanism “holding together” the cluster of properties we initially identify as characteristic of that genus? Not a propensity to interbreed—for we have two reproductively-isolated species! 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], p. 6). Wilson *et al.* similarly propose that “biological individuals often are as they are and behave as they do because of the relations in which they stand” ([2007], p. 198). But in the case of common historical origins, *homeostasis* seems like the wrong metaphor. What we have is not the resistance to disrupting a cluster of properties by the workings of certain causal connections but the stability of such clusters due to their relative causal *isolation*—the *absence* of potentially-perturbing causal pathways from the here and now to the there and then. Such cases seem better characterized by what Griffiths calls “phylogenetic inertia” ([1999], p. 220)—they are, in Francis Crick’s memorable phrase, “frozen accidents” ([1968]).<sup>24</sup>

---

<sup>22</sup> For defense of Statisticalism, see (Matthen & Ariew [2002]), (Walsh, Lewens, & Ariew [2002]), and (Walsh [2007]); for critical discussion see (Millstein [2006]) and (Brandon & Ramsey [2007]).

<sup>23</sup> according to which species are “interbreeding natural populations that are reproductively isolated from other such groups” (Mayr [1963], p. 17).

<sup>24</sup> Though in some cases this “inertia” resembles that of Aristotelian physics—with traits atrophying “when no selective forces work to maintain them” (Griffiths [1999], p. 220) —, a more Newtonian model seems widely appropriate. Even the Aristotelian model of phylogenetic inertia is acceptable if the rate of “atrophy” (due to the effects of genetic drift, and so on) is long enough to suit the purposes of the particular sciences in which a kind is embedded.

One might object at this point that causal homeostatic mechanisms underpin phylogenetic inertia insofar as reproduction and development are (relatively) high-fidelity copying mechanisms. This looks ad hoc. For it is uncontroversial that they are low-fidelity enough to allow for the variation that fuels evolutionary change via natural selection. We cannot simply regard reproduction and development as maintaining the coherence of a cluster of properties just when selection and drift *do not* disrupt the homogenizing activity of reproduction and development. Otherwise, we trivialize their role: they become the relevant mechanisms unless they're not.<sup>25</sup>

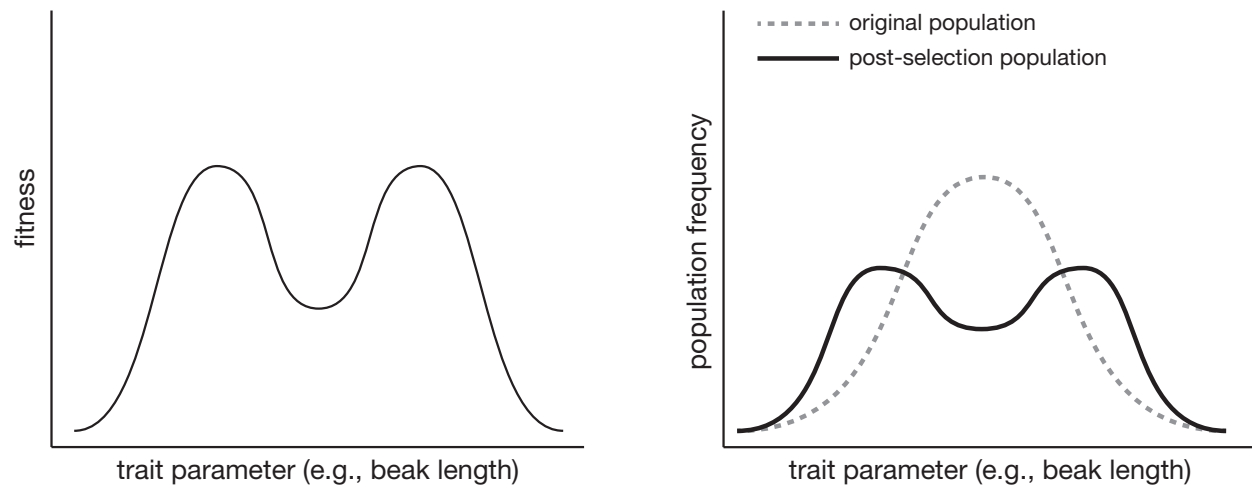
One might also argue that my reluctance to treat “common selective regimes” as homeostatic mechanisms merely betrays an overly particular (or literal) conception of homeostatic mechanisms. I might be persuaded to plead guilty to this charge (I *try* to banish images of clockwork!) if HPCers admit in return that they need to say more about what constitutes a homeostatic mechanism. Be this as it may, other examples seem to me non-negotiable. Just reflect on the fact that many of the sorts of processes Boyd and other HPCers focus on can actually be engaged in pulling the members of a kind apart. Only *stabilizing* selection is a decent candidate for mechanistic treatment.

Consider, for example, a species taxon which accommodates our epistemic practices in ways we'd expect to see from a natural kind. Clearly, we cannot count on this category *always* playing these roles—natural selection might disrupt the stability of the associated property cluster. We need to remember that in some cases the disrupting influences of selection are ever-present. Individuals on the extreme ends of a trait parameter may have an evolutionary edge over their more moderate kin; this will generally give rise to disruptive selection and possibly speciation (see Figure 2). Such selective regimes are quite implausible as “homeostatic mechanisms”. Nor need they be “heterostatic”, maintaining differences within a population. But discovering that disruptive selection is operating on a particular taxon need not besmirch that taxon's epistemic utility. Accordingly, it does not show us that we were wrong to treat that category as a natural kind. For in certain scientific contexts (such as conservation ecology, medicine, and functional biology) where stability across evolutionary timescales is of little concern, even species in disruptive (“heterodynamic”) selection

---

<sup>25</sup> Thanks to P.D. Magnus for pushing me on this issue. Once one sees this pattern, similar cases are relatively easy to spot. I have elsewhere suggested that different enantiomers of biochemical species might be considered to be distinct natural kinds (Slater [2005]). But enantiomers seem like paradigm cases of kinds whose characteristic properties are maintained neither by a common microstructural essence nor causal mechanism. They are merely “stuck” in three-dimensional space in ways that are causally isolated and given certain types of chiral environments lead to the manifestation of different characteristic dispositions. One might object to my treatment of this case in like fashion by contending that molecular structure of enantiomers, while evidently unable to serve a role as essences, *do* maintain the conjunction of intrinsic and extrinsic properties constitutive of being a particular enantiomer by resisting the sort of contortions that would be required to turn one enantiomer into the other. I cannot say more about this case in this context except to point out that even if the intrinsic structure of a chiral molecule causally explains the maintenance of its associated cluster of properties, it does not explain why *those* properties are stably maintained. As with the “high(ish)-fidelity copying mechanisms” suggestion, the explanandum is merely the stability of whatever properties a certain category possesses, not the fact that the particular properties in the cluster are stable. I'm not certain how much this should bother us.

regimes may possess a cluster of properties which are stable enough to afford inference and explanation in those contexts.



**Figure 2:** In this schematic depiction of a disruptive selection regime, we have a particular trait parameter—say, beak length in a species of bird—that has a bimodal fitness distribution (in a given environment, it’s good to have a small beak and a large beak). All things being equal, we can expect that in this will result in a trend in the population toward a prevalence of small- and large-beaked organisms.

It might be thought that as soon as we grant the workings of these heterodynamic forces working to pull apart clustered properties, we see right away that there must be opposing homeostatic mechanisms (intrinsic or extrinsic) at work as well maintaining the stability of the clusters. The mechanisms of reproduction and development will again seem tempting candidates. But 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.

\* \* \*

Thus far, I have interpreted the HPC account as according a literal, crucial role to causal homeostatic mechanisms. I argued that a number of practical and theoretical problems crop up as we examine this role in further detail. However, it might be objected that I am over-interpreting what is meant to be a metaphor. If that’s so, I may be read as challenging the aptness of the metaphor. Anyway, I believe that an account of natural kinds ought to rest on firmer theoretical foundation than a metaphorical similarity to other known entities and processes—especially when talk of such entities already presumes some conception of “kindness”.

It may be possible to articulate a conception of causal homeostatic mechanisms that would circumvent the concerns I have articulated here—I do not claim that any of the considerations I have sketched above are decisive. But they give me sufficient pause to want to offer a different account. It seems to me prudent to focus on the intended *effect* of the existence of essences and the

operation of causal homeostatic mechanisms in virtue of which natural kind categories contribute to our epistemic practices: that the clustering is, in a sense to be discussed, *stable*.

## 5 Stable Property Cluster Kinds

### 5.1 The Basic Idea

Let us return to the example of the roulette wheel. Peacocke's demand that some *condition* ensure the repeated red-spinnings might be seen as a way of ensuring the stability or robustness of this occurrence. The modal fragility of such an occurrence—the sense in which had various things gone very slightly differently with the spinning, we wouldn't have seen fifty reds—seems to imply a *temporal* fragility that cancels any inductive warrant we might possess. This seems quite plausible. If some truth could easily have been false, then who's to say that it won't *go* false any time from now? On reflection, however, we can see that the implication does not hold in general. There are lots of facts that are modally fragile in the sense that they very easily might not have occurred—for example, that Cletus the clumsy archer won the archery tournament yesterday—but which are not in any danger of *ceasing* to be true in the future.

Even if one rejects this modal-to-temporal implication, one might still insist that our inductive and explanatory practices need to be grounded by more than just dumb luck; it must be more than an *accident* that we are right. This general idea seems to be behind many of the defenses of the Grounding Claim mentioned in §3. But when it comes to the projectibility of a kind, the ontological ground—an essence, a causal homeostatic mechanism—is only a means to an epistemically-significant end (and if the previous section's arguments are on the right track, in the case of cluster kinds, the proffered homeostatic mechanisms are neither always necessary nor sufficient means). Peter Lipton expressed a similar sentiment in commenting on Kornblith's application of the HPC account to the problem of inductive knowledge; he wrote: "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], p. 493).<sup>26</sup>

Accordingly, I suggest that an account of natural kinds would do better to focus on the special sort of stability a cluster of properties might possess in virtue of which it is apt for induction and explanation rather than focusing on the *something* causing that stability. Hence my proposed name: the *Stable* Property Cluster account of natural kinds. As with the HPC account, SPC kinds are associated with potentially-loose clusters of properties. Unlike the HPC account, it requires only that these properties be sufficiently stably coinstantiated to accommodate the inferential and explanatory

---

<sup>26</sup> In like fashion, Sören Häggqvist argues that "the demand for underlying mechanisms, even short of demanding internal micro-mechanisms, is still excessive. It is not at all clear why the lack of such mechanisms should impair the soundness of a kind" ([2005], p. 80).

uses to which particular sciences put such categories.<sup>27</sup> The shift of focus from mechanisms to stability scores three significant goals. First, the SPC account evades the problems with mechanisms' role in the HPC account discussed above. Even categories associated with clusters of properties whose sociability are gradually being disrupted by heterodynamic selection regimes can underpin our epistemic practices in virtue of their having sufficient stability for the purposes of the relevant sciences. Ditto for categories whose cluster's stability is not maintained by any mechanism in particular. Second, it achieves an attractive degree of neutrality. Stability, as I shall understand it, is a high-level concept that is independent of its particular realizers and their analysis. Third, it represents a more general account of natural kinds able to encompass the above-mentioned kinds as well as strict essentialist kinds, kinds with historical essences (Griffiths [1999]; Okasha [2002]), and HPC kinds whose clusters' stability are largely secured by homeostatic causal mechanisms (in the right context). The stability that lends itself to a kind's inductive and explanatory utility is, as philosophers are apt to say, multiply realized.

## 5.2. Two Conceptions of Stability

What might it mean to call a cluster of properties “stable”? One first stab might focus on the instantiation of the clustered properties by a particular individual—an instance of the kind associated with that cluster. Say that a property cluster kind  $\phi$  is *instance-stable* when (to a first approximation) satisfiers of  $\phi$  (individuals of the kind  $\phi$ ) do not easily relinquish the relevant cluster of properties. Once instantiated, the instances of the properties in  $\phi$  resist their non-instantiation—perhaps collectively by constituting a homeostatic mechanism.

Instance stability is both too strong and too weak to characterize natural kinds. It is too strong inasmuch as it implies that kind membership is “sticky”. Once a particular thing satisfies the cluster of properties associated with a kind  $\phi$ , it resists becoming not- $\phi$ . But while this may be characteristic of some kinds (cases where kind-essence and individual-essence are somehow bound together), other objects apparently change their kind quite readily. Instance stability is too weak in that it does not sufficiently account for kinds' epistemic role. To see this, let us consider a schematic example.

Suppose that  $\phi$  is a kind associated with a cluster of properties  $P, Q, R, S, T$ . We can use brackets to denote that these properties are clustered without presuming the involvement of other abstracta (to wit, sets):  $[P, Q, R, S, T]$ ; for convenience, let us also use the name of the kind enclosed in brackets as a notational equivalent:  $[\phi]$ . We can think of ‘ $[\phi]$ ’ as functioning linguistically as a predicate: e.g., that some particular  $j$  is a member of cluster kind  $\phi$  could be symbolized as ‘ $[\phi]j$ ’ or ‘ $[P, Q, R, S, T]j$ ’ (depending on how specific we wished to be)—so long as we do not think of this as reducing to either ‘ $Pj \wedge Qj \wedge Rj \wedge Sj \wedge Tj$ ’ or ‘ $Pj \vee Qj \vee Rj \vee Sj \vee Tj$ ’. For on the cluster kind view, for  $j$  to be a member of  $\phi$  is not for it to have *all* of (or at least *one* of)  $P, Q, R, S, T$ . Rather, it

---

<sup>27</sup> As I will discuss in §6, these two qualifications carry with them some interesting (and I think welcome) consequences.

is for  $j$  to have a *cluster* of those properties. For now, I leave this notion at an intuitive level—something along the lines of “*a goodly many of the properties in question*”. We can represent  $\phi$ ’s instance-stability as the claim that for all  $x$ ,

$$[P, Q, R, S, T]x \rightarrow \blacksquare [P, Q, R, S, T]x$$

using ‘ $\blacksquare$ ’ for now as a sort of “robustness” operator (we’ll take up the precise interpretation of this operator shortly). However,  $\phi$ ’s being an SPC kind in this sense does not do justice to the inference pattern mentioned in §4, where the observation that  $j$  has  $P, Q, R$  gives us good reason for expecting that  $j$  has  $S$  and  $T$  as well. What we want out of clusters is not mere “sociability”—that once a cluster of properties are together instantiated, they are hard to scatter —, but *cliquishness*. Peg, Quinn, Ralph, Sarah, and Tim form a clique, say. Spotting Peg, Quinn, and Ralph at the mall means that Sarah and Tim are probably there as well. Nothing is implied about how long they’ll stay. Perhaps they flit from place to place, but when a few of them are around, you can bet that the others will be as well.

Call this conception of stability *cliquish stability*.<sup>28</sup> This is a rather more “abstract” variety of stability: a cluster of properties can be cliquishly-stable without its being instance-stable. The idea is to capture the fact that some properties are clustered in such a way that possession of some of them reliably (if imperfectly) indicates the possession of whole cluster (if not *each* property in the cluster) *at that time*. It need not imply that a particular that possesses any of these properties will *continue* to possess them.

To be more precise about this, it will be convenient to introduce another somewhat imprecisely defined notion: a “sub-cluster”. Consider again our property cluster  $[\phi]$  (that is,  $[P, Q, R, S, T]$ ). Let a sub-cluster of  $[\phi]$  include some but not all of the properties in  $[\phi]$ . ‘Some’ here is meant to be interpreted not as the familiar existential quantifier of first-order logic, but according to its *more* familiar colloquial usage: as an indefinitely-plural quantifier (as when I tell you that “some philosophers got inebriated at the Smoker” to assure you that you were not alone).  $[P, R, T]$  would thus be a sub-cluster of  $[\phi]$ ; as would  $[Q, R]$ , and so on. For convenience, assign arbitrary names to these sub-clusters by simply subscripting  $[\phi]$ :  $[\phi_1]$ ,  $[\phi_2]$ ,  $[\phi_3]$ , and so on. Now let us say that a property cluster  $[\phi]$  is cliquishly-stable when for all  $x$  and for many sub-clusters  $[\phi_1], [\phi_2], [\phi_3], \dots$ :

$$\blacksquare (([\phi_1]x \Rightarrow [\phi]x) \wedge ([\phi_2]x \Rightarrow [\phi]x) \wedge ([\phi_3]x \Rightarrow [\phi]x) \wedge \dots)$$

---

<sup>28</sup> I am tempted to call this ‘Matthew Stability’, after the bit in the New Testament (at Matthew 18:20) where Jesus promises that where two or three gather together as his followers, he is there among them. But that hits a bit too close to home (nominally-speaking).

where the ‘ $\Rightarrow$ ’ is to be read as probabilistic entailment.<sup>29</sup> And again we have the “black box” of robustness. What to say about this?

The option I’d like to explore here takes its cue from Marc Lange’s treatment of laws as members of a certain type of stable set.<sup>30</sup> The basic idea is that certain sets of truths are maximally invariant under counterfactual perturbations. We can express this property of sets this way: Given any counterfactual supposition consistent with the members of the set, had that supposition been the case, then all the members of the set would still have been the case; and ditto for an arbitrarily nested sequence of counterfactual suppositions: had anything at all compatible with the set been the case, then had anything *else* compatible with the set been the case, (and so on), then the members of the set would still have been true. Lange calls this special kind of stability “non-nomic stability” (for extensive discussion, see Lange [2000], [2009]). Take some logically-closed set of truths  $\Gamma$ . This set possesses non-nomic stability if and only if for each member  $m$  of  $\Gamma$ :

$$p \Box \rightarrow m,$$

$$q \Box \rightarrow (p \Box \rightarrow m),$$

$$r \Box \rightarrow (q \Box \rightarrow (p \Box \rightarrow m)), \dots$$

for any non-nomic claims  $p, q, r, \dots$  which are logically consistent with the members of  $\Gamma$ .<sup>31</sup> Lange conjectures that the set of laws is the only non-trivially non-nomically stable set. This affords a sharp distinction between the facts which are laws and those which are accidents.

However, Lange allows that there are different sets which are stable on more restrictive ranges of counterfactual suppositions. This is particularly useful for thinking about the biological sciences. Some biological generalizations, though they clearly could have been false, possess a significant degree of stability in the face of these more restrictive ranges counterfactual suppositions. Consider an example. Lange mentions the belief among anthropologists that “any person of entirely Native-American heritage is blood type O or blood type A” ([2000], p. 13). Though a historical accident—“research has suggested that all Native Americans are descended from a very small band that crossed

---

<sup>29</sup> Such entailment, of course, comes in degrees and can be understood in different ways. I will suggest in §6 that different sciences may set their own tolerances for what counts as sufficiently probable connections between sub-clusters and whole clusters in the definition of cliquish stability. Thanks to Rachel Briggs for helping me think about this.

<sup>30</sup> There are other ways of thinking about stability, of course, which bear interesting relations to Lange’s; see, e.g., (Mitchell [2000]), (Mitchell [2002]), (Woodward [2001]), and references therein.

<sup>31</sup> In Lange’s ([2009]) treatment of laws, he reformulates slightly the notion of “non-nomic stability” described in his ([2000]) by using “might” rather than “would” counterfactuals, resulting in a stronger version of stability (which he calls “sub-nomic stability” in the later work). In the later definition, counterfactuals of the form ‘ $p \Box \rightarrow m$ ’ are replaced by those of the form ‘ $\neg(p \Box \rightarrow \neg m)$ ’ (“it’s not the case that if  $p$  were true,  $m$  might not be true”). The revised formulation of stability is stronger, in that ‘ $\neg(p \Box \rightarrow \neg m)$ ’ implies ‘ $p \Box \rightarrow m$ ’ but not vice versa; see Lange ([2009], p. 29) and notes thereabouts for discussion. To simplify matters (and to cohere with Lange’s pre-2009 discussions of laws), my discussion will ignore his post-2009 elaborations on stability.

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. It would still be the case had a very wide array of facts been different. To repurpose another of Lange’s examples ([2004], p. 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. Of course, the blood type generalization might not still have been true had, say, the winter of 10,273 BCE been slightly warmer. The point is that if you are an emergency room doctor, the blood type generalization is stable enough for you to rely on; for it is unlikely that you would be very interested in counterfactuals involving the weather twelve thousand years ago. What matters to you is what manipulations in the here and now might save your patient.

Lange suggests we think of such restrictions of the range of counterfactuals under which certain generalizations are stable as being defined by the “interests” of the relevant special sciences. Change the above example slightly; Lange claims that

it is of medical interest to know whether a given heart attack might have been less serious had epinephrine been administered sooner, or had the patient long been engaged in a vigorous exercise regimen, or had she been wearing a red shirt, or had the Moon been waxing. But it is not of medical interest to know whether the heart attack might have been less serious had human beings evolved under some different selection pressure. A physician might blame a patient’s untimely death on her smoking, but not on human evolutionary history. (Lange [2004], p. 107)

While I am sympathetic to this basic idea—there are clearly some counterfactual antecedents which are of perennial interest to certain fields and many more others which are *not* —, there are some pressing concerns about the details of how interests apportion modal space. But let’s set these aside for the moment and look at how to adapt Lange’s basic conception of non-nomic stability to the SPC view of kinds.

Suppose we understand the black box in the above definition of cliquish stability in a broadly Langian way: that a property cluster  $[\phi]$  is cliquishly-stable when for all  $x$  and for many sub-clusters  $[\phi_1], [\phi_2], [\phi_3], \dots$ :

$$p \square \rightarrow (([\phi_1]x \Rightarrow [\phi]x) \wedge ([\phi_2]x \Rightarrow [\phi]x) \wedge ([\phi_3]x \Rightarrow [\phi]x) \wedge \dots),$$

$$q \square \rightarrow (p \square \rightarrow (([\phi_1]x \Rightarrow [\phi]x) \wedge ([\phi_2]x \Rightarrow [\phi]x) \wedge ([\phi_3]x \Rightarrow [\phi]x) \wedge \dots),$$

$$r \square \rightarrow (q \square \rightarrow (p \square \rightarrow (([\phi_1]x \Rightarrow [\phi]x) \wedge ([\phi_2]x \Rightarrow [\phi]x) \wedge ([\phi_3]x \Rightarrow [\phi]x) \wedge \dots), \dots$$

where  $p, q, r, \dots$  meet the following conditions:



- (a) They are consistent with the probabilistic entailment relationships from sub-clusters to clusters;
- (b) They are consistent with the natural laws (i.e., no counterlegals);
- (c) They meet the relevant applicability standards.

The justification of conditions (a) and (b) is fairly straightforward: (a) is isomorphic to Lange’s requirements for  $p, q, r, \dots$  in his definition of non-nomic stability. We cannot expect some fact to be stable on the assumption of its negation: this would be tantamount in this case to insisting that something remain a natural kind even if it failed to be a natural kind! The justification of (b) is likewise parasitic on Lange’s construction. It assumes that the laws are at least partly responsible for facts about the relationships among a cluster’s properties.<sup>32</sup>

Condition (c) is the interesting—because open-ended—condition. Here we apparently return to the worries about Lange’s invocation of the interests of certain special sciences. One worry is that if interests play a significant role in the definition of stability, then we immediately foreclose on the “naturalness” of a system of natural kinds defined in these terms. We hearken back to Craver’s worry about interests playing too significant a role in mechanism-individuation in the HPC account.

Relatedly, we might wonder what defines a discipline’s interests. Is it even credible that disciplines single out (explicitly *or* implicitly) certain ranges of counterfactual antecedents for consideration of whether certain other facts would remain true on their supposition? *Prima facie*, it seems far easier to make sense of certain counterfactual antecedents *not* being of interest to a particular discipline. Were you to ask one of the doctors whether she was interested in whether the heart attack would still have been as severe if the humans had followed a different evolutionary trajectory, the answer surely would be “no”. But asking the doctor whether she is interested in what the scenario would have been like under different fashion or lunar conditions doesn’t seem likely to elicit a different response. One might claim that the reason our doctor won’t admit to being interested in the phase of the moon is that she already knows (or judges with a high confidence) that the moon’s phase has (practically) nothing to do with her patient’s heart condition. Perhaps this is enough to qualify the moon’s phase as of medical interest. This strikes me as a rather odd thing to say.

I prefer to think of condition (c) as being defined in terms of “relevance” rather than interests. Interests do play a role in determining which counterfactuals are relevant to the evaluation of cliquish stability, but only in a roundabout way. To illustrate, consider an example. Medicine is

---

<sup>32</sup> Actually, (b) is stronger than we really need, since there will be some counterlegals on which certain cluster relationships continue to hold. But this overkill affords some simplicity and does no harm that I can see. I am also not sure how to make it weaker without making it too weak. We should not, for example, insist that the  $p, q, r, \dots$  be consistent with all the facts of the form ‘ $[\phi]x$ ’, because cliquish stability does not presume that individuals are *necessarily* members of a certain kind. I leave this as an open question.

interested in saving lives, let's say. The inferential and explanatory work of the medical sciences pursues this goal in part by focusing on circumstances that are within our power to control: hence the relevance to medical practice of the counterfactual suppositions involving different amounts of drugs, exercise, or time in the ambulance; and hence the *irrelevance* of suppositions involving evolutionary contingencies or circumstances likely to bear on them. It is not simply that such "frozen accidents" are without our power to change—for nor is it within our power to change how long a certain patient *spent* in an ambulance once they are at the hospital. Rather, the accidents of evolutionary history are not similar in informative ways to circumstances which *are* manipulable. In contrast, reflecting on whether a certain patient would have lived had he arrived at the hospital sooner, *is* potentially instructive for future cases. This is why shirt color and lunar phase are also relevant (in the present sense) to medical concerns. They are circumstances which are similar to those we can either manipulate (shirt color) or at least be sensitive to (lunar phase).

This is not, of course, to suggest that "manipulability" will be what restricts the range of counterfactual suppositions we consider in assessing cliquish stability. Consider a particular species taxon whose members possess a characteristic cluster of properties. Biological inquiries that concern reasonably short timescale interactions—e.g., those of ecology—will rightly regard counterfactual suppositions involving the remote evolutionary past as irrelevant—but not simply because we cannot manipulate what went on millions of years ago (or relevantly similar states of affairs). For once again, it need not be within our power to manipulate conditions that almost certainly *will* be relevant to ecological concerns (such as the rise and fall of a predator's population). Why, then, consider this counterfactual in assessing the cliquish stability of the property cluster associated with our taxon? In this case, it seems to me that the simplest thing to say is that we can expect such circumstances to eventually come to pass and that in order for our kind to bear any epistemic fruit in the contexts and timeframes with which we are concerned, the cluster ought to be largely insensitive to those changes.<sup>33</sup>

Property clusters which are cliquishly-stable for a given science, project, research program, or what have you, offer certain fixed points for those inquiries in the sense that for possible manipulations relevant to those pursuits, we may count on finding the clustered properties together, where we find some of them. So possibly, some clusters are only natural kinds for particular domains of inquiry. This result parallels Lange's treatment of special science laws—though it is not uncontroversial for this reason. In particular, one might worry that this would squash any hope of the SPC account offering support for the Realist Presumption that many of our categories "carve nature at its joints".

---

<sup>33</sup> My descriptions of these examples are necessarily schematic and tentative. The question of how disciplinary norms (of classificatory and investigative methodology) inform how a science assesses what natural kinds exist is complex and remains wide open. This is a matter I am presently pursuing by examining a number of case studies across the biological sciences; see my ([2013]), ([2014]), and ([manuscript]).

Before examining this issue (in the final section), I wish to address two natural worries about shifting our theoretical focus to stability over mechanisms and essences. First, *won't any theoretical unification at the epistemic front come with a semantic cost?* Probably; I tend to doubt that the SPC account will be able to generate (without supplementation) any claims about how reference to natural kinds works. It may simply be that the semantics of natural kind terms may not exhibit much theoretical unity. Given the gain of genuine theoretical unity elsewhere, this is a potential cost I am willing to pay.

Second, *does not the identification of homeostatic mechanisms or microstructural essences contribute to the accommodation demands of our best sciences? Wouldn't adopting the SPC account be tantamount to renouncing these epistemic benefits?*<sup>34</sup> I do not think so. We can grant that the project of uncovering certain homeostatic mechanisms underlying the stability of some properties can contribute to the construction of epistemically fruitful classification schemes without supposing that such identification is *necessary* or that the mechanisms should be part of the philosophical analysis of natural kinds.

Putting stability at the ground level of an account of a natural kind offers an attractive level of metaphysical neutrality. It turns out that we can say much about stability—indeed, *more* than what has so far been said about HPC's causal mechanisms—without being forced to engage in controversial metaphysics. But the SPC analysis need not constrain scientific practice in any substantive way. Uncovering facts about an essential microstructural property (if such there be), or homeostatic mechanism, or ensemble of thereof, may well be an important part of our determining whether some properties *are* stable.<sup>35</sup>

## 6 Interests and Realism about SPC Kinds

We now turn to what is likely to seem the most controversial part of the SPC account of natural kinds. SPC kinds can be, as I will put it, “*domain-relative*” in several senses.<sup>36</sup> First, there is the question of how many properties are expected to be clustered together for something to count as a kind. Second, the norms and aims of certain domains may require different levels of cluster cohesiveness—that is, different disciplines may tolerate different degrees of flexibility in the clustering required by their respective kinds (see Wilson [2005], p. 113). Perhaps property clusters defining physical kinds like electrons or quarks are supposed by those disciplines to be perfectly

---

<sup>34</sup> I'm grateful to an anonymous referee for raising this second worry.

<sup>35</sup> When we look at *how* the investigation into such homeostatic mechanisms works, we see, I believe, much more than the a mere check on their existence (even as concerns only the identification of projectible categories). This is a subject I aim to address in future work.

<sup>36</sup> My comments here share some obvious overlap with those of Boyd (1999, 148), though I try to go a bit further than he does there.

clustered (conjunctive) kinds while those of higher biological taxa like families may be quite loose.<sup>37</sup> A third, closely related, sense in which cluster kinds can be domain-relative is in how the probabilistic entailment relations described in §5.2 are understood: how likely is it that the instantiation of a certain sub-cluster betokens the instantiation of the whole cluster? Fourth, we have the interest-informed relevance condition (c) above circumscribing how we think of a cluster's stability. Since a particular cluster of properties can meet the requisite conditions for one domain but fail to meet them in another, we may allow that at least some collections of things only instantiate natural kinds from the perspective of particular sciences, or to pick a more neutral term: *domains*.<sup>38</sup> But if some kinds are domain-relative, the question of what kinds there are *tout court* is not generally tractable. What we can legitimately ask instead is what kinds various domains of inquiry in fact recognize (or *would* recognize) given their present aims, interests, and norms.

A different relativity about natural kinds may attach to certain physical contexts: call this *context-relativity*. The point can be easily made using the language of homeostatic mechanisms, but applies *mutatis mutandis* to SPC kinds. If certain mechanisms only successfully maintain the stability of a cluster in particular contexts, then such clusters fail to be natural kinds unless relativized to those contexts. Of course, contexts where the properties in the cluster typically go uninstantiated *anyway* will not generate this effect. Remember: we are not talking about instance-stability. Many particulars of a kind lose their properties in some circumstances (e.g., a protein denatures outside of its usual temperature range, a cell ceases its characteristic functioning, and so on). The context-sensitivity at issue here is a more subtle affair: where the property cluster may continue to be instantiated, though the mechanism(s) maintaining its stability no longer function. For example, the cluster of properties defining certain cell types may only be stable when considered *in vivo* (rather than in a Petri dish, say).

An HPCer might respond to my objections from §4.1 (concerning the sufficiency of homeostatic mechanisms to stabilize a cluster of properties) that the work context-relativization does in the SPC account can equally be applied in the HPC account, effectively defeating that objection. If it works for me, shouldn't it also work for them?<sup>39</sup> Perhaps so; but what is being contextually-relativized? Not, presumably, the simple *existence* of a homeostatic mechanism—for this would be transparently ad hoc. Perhaps the existence of a sufficiently *stable* homeostatic mechanism (for the relevant discipline's norms and interests)? In this case, the HPC account seems to be just converging

---

<sup>37</sup> I conjecture that these two requirements tend to be “inversely proportional” in the sense that sciences which demand fewer properties to count as a cluster (like physics) tend to require their clustering to be more perfect; biological sciences, on the other hand, tend to countenance (if not require) more properties to be included in a cluster but allow their clustering to be rather more imperfect.

<sup>38</sup> Thinking about the present relativity in terms of domains rather than in terms of, say, particular branches of the special science allows us to recognize in addition to them: (1) non-scientific domains of inquiry where particular clusters may count as natural kinds, (2) domains which are internal to individual special sciences (particular research programs, lines of inquiry, &c.), or (3) assemblages of various special sciences.

<sup>39</sup> I'm again indebted to an anonymous referee for raising this objection.

on the SPC account. But look: this needn't be a competition. The SPC account is, in my view, a refocusing on a phenomenon that has been implicitly recognized by HPCers all along. I am hopeful that they will see such theoretical convergence—and compatibility with the scientific focus on mechanisms of particular kinds—as reason to join me in exploring the concepts, applications, and consequences of the SPC account in more detail.

I will close this discussion by briefly considering one a consequence of the SPC account on our thinking about natural kinds as an ontological category. Allowing the interests and norms of a domain—even a particular research project—to influence whether a certain category counts as a natural kind might seem like a hefty dose of pragmatism to swallow. While some may applaud this, many with a sympathy for the Realist Presumption will worry. A theory of natural kinds, they might insist, should tell us about the objective divisions in the world that pre-exist our classificatory activities. Otherwise, we cannot make sense of some theories doing better than others at “carving nature” nor how certain schemes of classification can be in error.

I appreciate the worry, but ultimately think it is overstated. Though there's a sense in which we cannot be mistaken about what norms and interests to adopt concerning certain classifications (I suppose that such norms are not truth-apt), I think we can come to see ourselves as having *gone wrong* in adopting them. Perhaps we held them in the first place due to some genuine mistake (taking some putative homogenizing effect to be more important than it actually is, for example). Of course, we certainly can be mistaken about whether a certain category is associated with properties that stably cluster *given* certain norms and interests (assuming that there are facts about property instantiations and what subjunctive claims which are true independently of us). But there are other ways of evaluating our norms. We may, for example, find that *failing* to relativize the evaluation of a certain cluster's stability is unfruitful—for example, by foreclosing on certain makeable inference that would otherwise stem from recognizing a category as a context-relative kind. This, of course, presumes the existence of other “meta-norms”.

So while a domain's norms and interests are relevant to what natural kinds there are, it's not the case that we can arbitrarily “define nature's joints into existence”. Nor do natural kinds await our classificatory activity in order to come into being. For the norms and interests relevant to a cluster's stability often pre-exist those activities. They do not, however, pre-exist us.<sup>40</sup> Thus a critic might point out that there is no live sense in which the SPC account is a realist account of natural kinds—for it seems that there is no sense in which there were SPC kinds before science came on the scene a few thousand (or a few hundred) years ago. But surely there were different natural kinds of things!

I think that there are two compelling things to say in response. First, it is not clear to me that we should be aiming for a realist conception of natural kinds to begin with. I put a higher priority on maintaining some of the key realist intuitions about classification—that we can be in some sense

---

<sup>40</sup> Except, perhaps, in an attenuated sense of being possible ways of arranging scientific (or other) inquiries. Thanks to Marc Lange for raising this possibility.

mistaken about our systems of classification, that we can classify things in better and worse ways, that our best classification schemes can contribute in familiar ways to our inductive and explanatory efforts, and so on.

Second, one may hold out for a sense in which *certain* special categories—electrons, say—are natural kinds in a norm-neutral way. I think I can assent to the spirit of such a request, if not the letter, with minimal retraction of what I’ve been pressing by (so to say) crossing Mill with Whewell. Recall Whewell’s ([1840]) much cited idea about the naturalness of a system of classification stemming from the convergence of different systems of classification on the same categories. Though neat, this idea won’t help us make sense of electrons having discipline-independent objectivity, since the non-physical sciences (for the most part) do not have a great deal of explicit truck with fundamental particles. Here, however, we might invoke another nifty idea from John Stuart Mill ([1872]) about objects as “permanent possibilities of sensation”. Perhaps there are some clusters of properties such that no matter *how* a discipline adjusted its norms and aims (compatible with the discipline maintaining an understanding of the natural world in view), the category that cluster described would be fit to play a robust epistemic role in the discipline. We might say that such categories exhibit a “permanent possibility of Whewellian convergence”. This allows us to see that the pluralism resulting from the SPC account’s domain- and context-relativity need not extend to *all* kinds.

While this goes some ways toward accommodating realist intuitions, I readily admit that the SPC account exhibits some distinctly non-realist features. The context- and discipline-relativity of some kinds show, I think, that natural kinds (on the SPC view) are not an ontological category (cf. Lowe [2006]). Nor is it obvious how they could be reducible to facts in other ontological categories—to universals, for instance. Against this idea, Bird suggests that:

a simple reduction of kinds to combinations of universals is available along the lines proposed by Armstrong. Not all kinds can be dealt with so easily, for example those in biology. Nonetheless, the strategy can be extended, by considering kinds as homeostatic property clusters. Although Boyd does not see that latter in ontological terms, we can construe them as sums of properties, just as complex particulars are the sums of their component parts. (Bird [2012], p. 103)

Yet treating a cluster of properties as a *conjunction* trades away what is arguably most distinctive about the approach: its looseness and corresponding ability to accommodate the messy patterns of biological variation and similarity. The conjunctive approach is far too strong.<sup>41</sup>

---

<sup>41</sup> Richards makes essentially the opposite mistake by understanding property cluster accounts of kinds as *disjunctions* of properties: “essences [on the HPC view] are a disjunction” ([2010], p. 154). This approach is inappropriately weak. Suppose we take a kind K as being defined as being P  $\vee$  Q  $\vee$  R. Two individuals could possess this disjunctive property while sharing no single property in common. Disjunctive similarity is dirt cheap. The cluster approach requires more.

What, then, *are* SPC kinds if not conjunctions of universals? Rather than recognize a *sui generis* ontological category—of *clusters*, say —, I prefer to think of being a natural kind as a sort of *status* that things or pluralities of things (from various ontological categories) can have.<sup>42</sup> This “natural kindness” is understandably treated as a fixed, objective matter when it is highly insensitive to the whims of our classificatory norms and practices. But I suspect that the tendency to nominalization of what is potentially a domain- and context-relative status has sent us down some blind alleys. What the SPC account offers us is a flexible, high-level approach to understanding the various ways in which various categories (scientific and otherwise) can be regarded as genuine features of the world in organizing and facilitating our epistemic contact with the world. They are genuine features of the world *for* the relevant domains.

Clearly many questions about the approach remain. I will close by mentioning two that stand out as especially urgent. First, how in detail should the intuitions that SPC kinds are “genuine”/“real” features of the world be squared with the various sorts of relativity I mentioned above? Second, how should we understand the metaphysics (and epistemology) of cliquish stability (even from within a particular context)? Should we also follow Lange to primitivism about subjunctives or propose some different account of what makes subjunctives true?<sup>43</sup> I would prefer to not take a stance about this difficult question, elevating the concept of cliquish stability to a high-enough theoretical level to avoid the fray below (in something like the manner of Lange [2005b]), but doing so may be unavoidable.

## Funding

U.S. National Science Foundation (SES-0924376)

## Acknowledgements

I have been accumulating debts of gratitude to people who have helped me with this paper and these topics for some time now. Previous incarnations of this paper and separate packagings of these ideas have been presented at Duke University, the 2010 PSA, the University of Bristol, the University of Sydney, and the 2011 ISHPSSB: many thanks to those very helpful audiences and to others who read this paper and its forebears and who gave me helpful suggestions. I am particularly indebted to Ron Amundson, Matt Barker, Alexander Bird, Robert Brandon, Rachel Briggs, Mark Colyvan, John Dupré, Marc Ereshefsky, Michael Goldsby, Paul Griffiths, Matt Haber, Chris Haufe, Philip Kitcher, Marc Lange, Jason Leddington, P.D. Magnus, Jay Odenbaugh, Maureen O’Malley, Anya Plutynski, Ken Waters, John Wilkins, Rob Wilson, Achille Varzi, and Joel Velasco. I hope those who I have doubtless omitted will forgive my forgetfulness—it’s been a long time! Thanks also to the extremely

---

<sup>42</sup> This is one of the reasons why I believe that we need to recast the old debate about the ontological status of biological species—are they individuals or natural kinds? —, a subject I address in detail in my ([2014]) book *Are Species Real?*.

<sup>43</sup> see (Lange [2005a]), (Lange [2009]), and (Haufe & Slater [2009]) for more discussion.

helpful and challenging suggestions of two anonymous referees, this paper is significantly better than even its penultimate draft.

## REFERENCES

- Barros, D. B. [2008]: 'Natural Selection as a Mechanism', *Philosophy of Science*, 75, pp. 307–322.
- Bechtel, W. [2006]: *Discovering Cell Mechanisms: The Creation of Modern Cell Biology*. Cambridge: Cambridge University Press.
- Bechtel, W., & Abrahamsen, A. [2005]: 'Explanation: A Mechanist Alternative', *Studies in History and Philosophy of Biological and Biomedical Sciences*, 36, pp. 421–441.
- Beebe, H. [2011]: 'Necessary Connections and the Problem of Induction', *Noûs*, 45, pp. 504–527.
- Bird, A. [2007]: *Nature's Metaphysics: Laws and Properties*. Oxford: Oxford University Press.
- Bird, A. [2012]: 'Are Any Kinds Ontologically Fundamental?', in T. Tahko (ed), *Contemporary Aristotelian Metaphysics* (Cambridge: Cambridge University Press)
- Boyd, R. [1988]: 'How to Be a Moral Realist', in G. Sayre-McCord (ed), *Essays on Moral Realism* (Ithaca: Cornell University Press)
- Boyd, R. [1991]: 'Realism, Anti-Foundationalism and the Enthusiasm for Natural Kinds', *Philosophical Studies*, 61, pp. 127–148.
- Boyd, R. [1999]: 'Homeostasis, Species, and Higher Taxa', in R. A. Wilson (ed), *Species: New Interdisciplinary Essays* (Cambridge: MIT Press)
- Brandon, R., & Ramsey, G. [2007]: 'What's Wrong with the Emergentist Statistical Interpretation of Natural Selection and Random Drift', in M. Ruse & D. L. Hull (eds), *The Cambridge Companion to Philosophy of Biology* (Cambridge: Cambridge University Press)
- Chakravartty, A. [2007]: *A Metaphysics for Scientific Realism*. Cambridge: Cambridge University Press.
- Craver, C. [2007]: *Explaining the Brain: Mechanisms and the Mosaic Unity of Neuroscience*. Oxford: Oxford University Press.
- Craver, C. F. [2009]: 'Mechanisms and Natural Kinds', *Philosophical Psychology*, 22, pp. 575–594.
- Crick, F. H. C. [1968]: 'The Origin of the Genetic Code', *Journal of Molecular Biology*, 38, pp. 367–379.
- Devitt, M. [2008]: 'Resurrecting Biological Essentialism', *Philosophy of Science*, 75, pp. 344–382.
- Ellis, B. [2001]: *Scientific Essentialism*. Cambridge: Cambridge University Press.
- Ereshefsky, M. [2010]: 'What's Wrong with the New Biological Essentialism', *Philosophy of Science*, 77, pp. 674–685.
- Ereshefsky, M., & Matthen, M. [2005]: 'Taxonomy, Polymorphism, and History: An Introduction to Population Structure Theory', *Philosophy of Science*, 72, pp. 1–21.



- Ghiselin, M. [1974]: 'A Radical Solution to the Species Problem', *Systematic Zoology*, **23**, pp. 536–544.
- Godfrey-Smith, P. [2011]: 'Induction, Samples, and Kinds', in J. K. Campbell, M. O'Rourke & M. H. Slater (eds), *Carving Nature at its Joints* (Vol. 8, Cambridge, MA: MIT Press
- Griffiths, P. E. [1997]: *What Emotions Really Are: The Problem of Psychological Categories*. Chicago: University of Chicago Press.
- Griffiths, P. E. [1999]: 'Squaring the Circle: Natural Kinds with Historical Essences', in R. A. Wilson (ed), *Species: New Interdisciplinary Essays* (Cambridge: MIT Press
- Hacking, I. [1990]: 'Natural Kinds', in R. B. Barrett & R. F. Gibson (eds), *Perspectives on Quine*. (Cambridge: Blackwell
- Hacking, I. [1991]: 'A Tradition of Natural Kinds', *Philosophical Studies*, **61**, pp. 109–126.
- Häggqvist, S. [2005]: 'Kinds, Projectibility and Explanation', *Croatian Journal of Philosophy*, **5**, pp. 71–87.
- Harman, G. [1965]: 'Inference to the Best Explanation', *Philosophical Review*, **74**, pp. 88–95.
- Harman, G. [1973]: *Thought*. Princeton: Princeton University Press.
- Haufe, C., & Slater, M. H. [2009]: 'Where No Mind has Gone Before: Exploring Laws in Distant and Lonely Worlds', *International Studies in the Philosophy of Science*, **23**, pp. 265–276.
- Havstad, J. C. [2011]: 'Problems for Natural Selection as a Mechanism', *Philosophy of Science*, **78**, pp. 512–523.
- Hull, D. L. [1978]: 'A Matter of Individuality', *Philosophy of Science*, **45**, pp. 335–360.
- Kornblith, H. [1993]: *Inductive Inference and its Natural Ground*. Cambridge: MIT Press.
- Kripke, S. [1980]: *Naming and Necessity*. Cambridge: Harvard University Press.
- Lange, M. [2000]: *Natural Laws in Scientific Practice*. New York: Oxford University Press.
- Lange, M. [2004]: 'The Autonomy of Functional Biology: a Reply to Rosenberg', *Biology and Philosophy*, **19**, pp. 93–109.
- Lange, M. [2005a]: 'How Can Instantaneous Velocity Fulfill Its Causal Role?', *The Philosophical Review*, **114**, pp. 433–468.
- Lange, M. [2005b]: 'Laws and Their Stability', *Synthese*, **144**, pp. 415–432.
- Lange, M. [2005c]: 'Reply to Ellis and to Handfield on Essentialism, Laws, and Counterfactuals', *Australasian Journal of Philosophy*, **83**, pp. 581–588.
- Lange, M. [2009]: *Laws and Lawmakers*. Oxford: Oxford University Press.
- Lange, M. [2011]: 'It Takes More Than All Kinds to Make a World', in J. K. Campbell, M. O'Rourke & M. H. Slater (eds), *Carving Nature at its Joints* (Cambridge: MIT Press
- Lipton, P. [1996]: 'Review of Kornblith, *Inductive Inference and its Natural Ground*', *Philosophy and Phenomenological Research*, **56**, pp. 492–494.
- Lowe, E. J. [2006]: *The Four-Category Ontology: A Metaphysical Foundation for Natural Science*. Oxford: Oxford University Press.

- Machamer, P., Darden, L., & Craver, C. F. [2000]: 'Thinking About Mechanisms', *Philosophy of Science*, **67**, pp. 1–25.
- Matthen, M., & Ariew, A. [2002]: 'Two Ways of Thinking about Fitness and Natural Selection', *Journal of Philosophy*, **99**, pp. 55–83.
- Mayr, E. [1963]: *Animal Species and Evolution*. Cambridge: Harvard University Press.
- Mill, J. S. [1872]: *A System of Logic* (8th ed.). London: Longman, Green, and Co. (Reprinted by Spottiswoode, Ballantyne & Co., 1965).
- Millikan, R. G. [2000]: *On Clear and Confused Ideas*. Cambridge: Cambridge University Press.
- Millstein, R. L. [2006]: 'Natural Selection as a Population-Level Causal Process', *British Journal for the Philosophy of Science*, **57**, pp. 627–653.
- Mitchell, S. [2000]: 'Dimensions of Scientific Law', *Philosophy of Science*, **67**, pp. 242–265.
- Mitchell, S. [2002]: 'Contingent Generalizations: Lessons from Biology', in R. Mayntz (ed), *Akteure—Mechanismen—Modelle* (Frankfurt: Campus Verlag
- Okasha, S. [2002]: 'Darwinian Metaphysics: Species and the Question of Essentialism', *Synthese*, **131**, pp. 191–213.
- Peacocke, C. [2004]: *The Realm of Reason*. New York: Oxford University Press.
- Putnam, H. (1975). Is Semantics Possible? In his (Ed.), *Mind, Language and Reality: Philosophical Papers* (Vol. 2). Cambridge: Cambridge University Press.
- Quine, W. V. O. (1969). Natural Kinds. In his (Ed.), *Ontological Relativity and Other Essays*. London: Columbia University Press.
- Richards, R. A. [2010]: *The Species Problem*. New York: Cambridge University Press.
- Sankey, H. [1997]: 'Induction and Natural Kinds', *Principia*, **1**, pp. 239–254.
- Searle, J. [1958]: 'Russell's Objections to Frege's Theory of Sense and Reference', *Analysis*, **18**, pp. 137–143.
- Skipper, R. A., & Millstein, R. L. [2005]: 'Thinking about Evolutionary Mechanisms: Natural Selection', *Studies in the History and Philosophy of Biological and Biomedical Sciences*, **36**, pp.
- Slater, M. H. [2005]: 'Monism on the One Hand, Pluralism on the Other', *Philosophy of Science*, **72**, pp. 22–42.
- Slater, M. H. [2013]: 'Cell Types as Natural Kinds', *Biological Theory*, **7**, pp. 170–179.
- Slater, M. H. [2014]: *Are Species Real?* London: Palgrave–Macmillan.
- Slater, M. H. [manuscript]: *The Nature of Biological Kinds*.
- Walsh, D. M. [2007]: 'The Pomp of Superfluous Causes: The Interpretation of Evolutionary Theory', *Philosophy of Science*, **74**, pp. 281–303.
- Walsh, D. M., Lewens, T., & Ariew, A. [2002]: 'The Trials of Life: Natural Selection and Random Drift', *Philosophy of Science*, **69**, pp. 452–473.
- Whewell, W. [1840]: *Philosophy of the Inductive Sciences*. London: Parker.
- White, R. [2005]: 'Explanation as a Guide to Induction', *Philosophers' Imprint*, **5**, pp. 1–29.

- Wilkerson, T. E. [1995]: *Natural Kinds*. Brookfield: Ashgate Publishing Company.
- Wilson, R. A. [1999]: 'Realism, Essence, and Kind: Resuscitating Species Essentialism?', in R. A. Wilson (ed), *Species: New Interdisciplinary Essays* (Cambridge: MIT Press
- Wilson, R. A. [2005]: *Genes and the Agents of Life*. Cambridge: Cambridge University Press.
- Wilson, R. A., Barker, M. J., & Brigandt, I. [2007]: 'When Traditional Essentialism Fails: Biological Natural Kinds', *Philosophical Topics*, 35, pp. 189–215.
- Woodward, J. [2001]: 'Law and Explanation in Biology: Invariance is the Kind of Stability That Matters', *Philosophy of Science*, 68, pp. 1–20.
- Woodward, J. [2002]: 'What Is a Mechanism? A Counterfactual Account', *Philosophy of Science*, 69, pp. 366–377.