Cell Types as Natural Kinds

Talk of different types of cells is commonplace in the biological sciences. We know a great deal, for example, about human muscle cells by studying the same type of cells in mice. Information about cell type is apparently largely projectible across species boundaries. But what defines cell type? Do cells come pre-packaged into different natural kinds? Philosophical attention to these questions has been extremely limited (see, e.g., Wilson 1999 and Wilson, Barker, and Brigandt 2007). On the face of it, the problems we face in individuating cellular kinds resemble those biologists and philosophers of biology encountered in thinking about species: there are apparently many different (and interconnected) bases on which we might legitimately classify cells. We could, for example, focus on their developmental history (a sort of analogue to a species' evolutionary history); or we might divide on the basis of certain structural features, functional role, location within larger systems, and so on. In this paper, I will sketch an approach to cellular kinds inspired by Boyd's homeostatic property cluster theory, applying some lessons from this application back to general questions about the nature of natural kinds.

1. Cell Types in Scientific Practice

It's easy to be impressed with both the difference between and similarity among cells. A neuron and a erythrocyte resemble each other about as much as an orangutan resembles an oyster. But just as individual orangutans resemble each other in ways that are epistemically fruitful to biologists, so do individual erythrocytes resemble each other sufficiently strongly and in particular respects to make them important pivots in our epistemic efforts.

The analogy between species and cell types also applies to the overall *structure* of their diversity. Though there are of course resemblances between species, we do not see a continuum of similarity among distinct organisms. Biological diversity is "clumpy". This sort of empirical fact is highly suggestive to enthusiasts about natural kinds, such as Brian Ellis. He writes at the outset of *Scientific Essentialism*:

> The distinctions between the chemical elements, for example, are real and absolute. There is no continuum of elementary chemical variety which we must arbitrarily divide somehow into chemical elements. The distinctions between the elements are there for us to discover, and are guaranteed by the limited variety of quantum mechanically possible atomic nuclei. (Ellis 2001, 3)

One of Ellis's chief goals in that book is to offer an essentialist account of natural kinds that places nomic facts on a secure footing.[1] As such, his essentialism is uncompromising: no wonder his warm-up example comes from chemistry, a domain of science kind-enthusiasts often mine for examples. Biological essences have appeared to be comparatively fewer.[2] In dark moments, some may even despair of identifying biological natural kinds at all.

Given a certain stance about natural kinds, however — one which prioritizes their roles in scientific practice — such despair is unwarranted. The biological sciences are up to their necks in commitments to different natural kinds at various levels of organization. I want to focus on the case of cellular kinds. Now, you will rarely, if ever, hear biologists use the phrase 'natural kind' — but 'cell type' is extraordinarily common. The language of type is encouraged in the first place by the "clumpiness" of cellular diversity noted by many biologists — one classic text even notes that "there is no continuum of adult cell types intermediate in character" (quoted in Vickaryous and Hall 2006, 2; see http://www.ncbi.nlm.nih.gov/books/NBK28393/). In the second place, any neutrality of the language of type is belied by the epistemic uses to which biologists put cell types. Different cell types can readily be identified morphologically (e.g., via histological investigations) —identifications which in turn reliably indicate that a particular cell will have other properties and dispositions characteristic of a cell of that specialized type.[3] Importantly, just as biologists commonly drop reference to species when discussing homologous genes (orthologs), cell types also cross species boundaries, allowing us to learn about our own physiology from model organisms. While I cannot survey here the variety of epistemic uses of cell types (both explanatory and inferential), hopefully it is plausible that we have compelling reason to make sense of such types having more than an arbitrary existence.

The present paper begins work on this project. I begin (in §2) by briefly motivating a *general* approach for accommodating cell types in a natural kinds framework[4] — as non-essentialist property-cluster kinds —, evaluating competitor proposals along the way. However, I will argue (§3) that the details of this case legislate for a altering some of the core theses involved in the most developed and well-known property-cluster account: Boyd's Homeostatic Property Cluster (HPC) account of natural kinds (Boyd 1988, 1991, 1999). While the HPC account departs in important ways from traditional essentialism, it carries on what I'll call a "bottom-up" stance to natural kinds in its theoretical use of causal mechanisms. This stance, I argue, faces a number of theoretical and practical problems. I will sketch an account of natural kinds of cells departs from this (§4) and close

[1] It is thus notable that the above "No Continuum Argument" lacks any reference to essences or essentialism; see Mumford (2005) for a useful discussion of these issues.

[2] Just how few is of course controversial.

[3] Subject, of course, to variations due to cell cycle, context, stimuli, and so on.

[4] One important question that I will not address in much detail here: what is connoted by 'natural' as a modifier of 'kind'? I persist in using the phrase 'natural kind' to signal my belief in the continuity between the account of kinds I offer and more traditional accounts. As will become clear in the final two sections, though, "naturalness" will take on a somewhat different cast on my account.

(§5) with some reflections on the general project of developing an account of natural kinds in the context of recent criticism that this endeavor has taken on scholastic hue.

2. Candidate Theories of Cellular Kinds

Suppose that the above gives us good prima facie reason for thinking that cells types — like species — are real.[5] So what philosophical account of their reality might we offer? If previous inquiries into the subject can be a reliable guide, we have three basic options: (A) treat them as essentialist natural kinds; (B) treat them as non-essentialist, cluster kinds; (C) treat them as individuals. I advocate (B). We can get there fairly readily by considering the merits of (A) and (C) and eliminating them.

Consider first the essentialist approach. As typically conceived, Essentialism has three tenets:

1. That the essential properties be intrinsic,
2. That they be possessed by all and only the members of a kind, and
3. That they explain why members of the kind have a series of superficial properties more or less in common. (See Ereshefsky 2010, §2.1 for discussion)

In the case of species, some sort of suite of genetic properties have been the obvious candidate for essences — particularly in fulfilling (3) (Devitt 2008; Wilkerson 1995). While biologists have been more willing to question the primacy of this explanatory link in recent years, the real difficulty with genetic essentialism about species have always been with the first two conditions (Wilson 1999, 190; Okasha 2002, §4; Barker 2010). The main problem is the lack of genetic homogeneity among members of a species — the 'all' direction of tenet (2) fails.[6]

Interestingly, essentialism faces something like the reverse problem when it comes to cellular kinds. Even while it was known that differences between cell types devolved from different combinations of gene products, it was not always clear how this was achieved. The apparent permanence of cellular differentiation in suggested that genes were selectively lost during development (Alberts *et al.* 2008, 411). This turns out not to be the case.[7] Within an individual organism, the genetic code of each cell is largely conserved — making it a poor candidate for an essence. Genetic essences fail the 'only' direction of tenet (2).

Of course, such considerations tell only against a particular *candidate* essence, not essentialism full-stop. Perhaps there are other properties that might fulfill each of the three tenets. One can imagine different ways of filling out the basic genetic essentialist line — say, by construing cellular

[5] We will take a more critical look at this supposition in the final two sections.

[6] Certain aspects of biological practice — to wit, a broadly historical orientation in biological systematics — also tell against (1). I will circle back to this issue shortly.

[7] In fact, this explanandum (permanence) was overstated. In eukaryotes, cells maintain their differentiated status only in particular contexts — say, in the company of other cells of that type. Outside those contexts, they tend to de-differentiate.

essences as certain kinds of regulatory adornments and packings of the genome. I am not overly sanguine about the prospects of this sort of suggestion, however. For one thing, a purely structural description of these modifications would seem likely to produce an overly fine-grained system of kinds. From the perspective of differential protein production and its consequences for cellular structure and function — the qualities by which biologists typically individuate cells — it does not matter *how* transcription is regulated. For two, as cells go through their various cycles, the ways in which differential protein expression is achieved may come and go (for a good discussion of the complications of cell cycle on our cell-type specification, see MacLean and Hall 1987, §2.4). Finally, even if something along these lines looked at all appealing, some cells — like erythrocytes and sieve-tube cells in plans —, lack nuclei in their terminal stage of development.[8]

Other non-genetic intrinsic candidate essences do not readily spring to mind.[9] Rather than survey other implausible options — many of which would fail tenet (3) — let us follow the trail blazed by neo-essentialists about species (e.g. Griffiths 1999; LaPorte 2004; Okasha 2002): perhaps we can relax essentialism to allow for *extrinsic* properties to define cellular kinds, dropping tenet (1). One might attempt to divide cell types on the basis of their developmental histories within the organism — their "developmental phylogeny". On its face, this suggestion looks promising. In multicellular organisms, cells differentiate in regular patterns during development. In many (relatively) simple organisms the developmental pathways of cells and tissues have been mapped in detail. And it's plausible that such histories satisfy tenet (3): since different developmental processes normally trigger the gene-regulatory events that give cells their distinctive qualities, there's a straightforward sense in which these histories *explain* why cells have the characteristic properties they have.

But the analogy with historical essences for species is highly imperfect. Unlike species, cells do not fit into a *single* phylogenetic tree. Rather, development in each organism defines its own tree. These trees, of course, resemble each other in specific ways. Certain developmental events — e.g., cleavage, gastrulation, and the establishment of different germ layers (Gilbert 2000, 26) — can be grouped at different levels of organization. Can we then define cells on the basis of their "phylogenetic location" on certain developmental tree *types* — or more specifically, on the basis of their location with respect to various types of developmental events?

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[8] As Maureen O'Malley correctly reminds me, this does not imply that they no longer engage in transcriptional regulation. And indeed, while their transcriptional activities are quite low (below normal detection limits), recent studies suggest that mature erythrocytes contain "diverse and abundant microRNAs" that play important roles in signaling and other maintenance functions (Chen *et al.* 2008, 2).

[9] There is one exception: many cell types have very characteristic (primary) functions — e.g., delivering oxygen, digesting foreign materials, producing certain neurotransmitters, &c. — in virtue of characteristic products or structures. Could we divide cell types on the basis of such characteristic features? In practice we often can (and do); however, it seems to me that such features are generally treated as *diagnostic* rather than as *defining* what it is to be a cell of that type. For further discussion of the difficulties with this approach, see Wilson (2005, 104–107).

This general proposal faces a number of difficulties. The first is primarily conceptual. How might one identify these different developmental event types? One obvious and common strategy is to define them in terms of their products — for example, particular types of tissue, organ systems, and cells. But this introduces an obvious circularity: we cannot informatively use kinds of developmental events to define kinds of cells if the latter are also used to define the former. It is not at all clear how else one might proceed here, particularly when it comes to extending developmental event kinds across species boundaries.

Second, and closely related to this point, reflection on the level of developmental similarity across species suggests that an inter-specific developmental taxonomy of cells will be (at best) rather more granular than what biologists typically countenance. While we can, it seems, identify very basic inter-species stages in early development, it's doubtful that the more refined developmental event types needed to define the cell types biologists recognize across species boundaries exist.

Third, such a classification scheme is likely to be rather revisionary even at the level of individual organisms. Some cell types — cartilage cells, for example — have their origins in different germ layers in the embryo.[10] And some cells of one developmental heritage can be induced to take on the intrinsic qualities and functions of cells of very different heritages (as revealed in laser ablation studies, for example). Accordingly, biologists are prepared to countenance such "developmental interlopers" as being of the same type. While "transdifferentiation" may be relatively rare (apart from experimental manipulation), these studies do show that cells indistinguishable in their structure, position, and function can have very different developmental trajectories (Tosh and Horb 2009, 111). As the paleontologist James Valentine summarizes: "Cells that seem morphologically identical and are found in the same tissues, or in seemingly identical tissues in different regions, can have different developmental histories" (Valentine 2003, 37).

One might set such difficulties aside, however, and attempt to simply apply the methods of cladistics to cell types. Vickaryous and Hall take essentially this approach, suggesting that we "categorize cell types on the basis of shared-derived characteristics" (2006, 7). But while this approach may suggest interesting alternative cellular classification schemes — revealing commonalities between cells that previously went unnoticed — without a literal analog of a unitary "tree of life" for species, it's not clear how this sort of "cellular cladism" bears on the question of realism about cell type.

What of option (C): that cell types are in fact individuals in analogy to the species-as-individuals thesis (lc. Ghiselin 1974; Hull 1978)? To my knowledge, no one has argued for this view. Indeed, the only person I know to one has even *considered* the idea brought it up in order to note its implausibility. Rob Wilson thinks that our disinclination to treat cells, among other biological categories, as individuals reflects badly on the (much more popular) individuality thesis for species:

[10] Some have their origin in the neural crest, some in the mesoderm. I thank Brian Hall for suggesting this example.

> It seems to me telling that while traditional realism is rendered implausible for [biological categories that are intrinsically heterogeneous and relationally taxonomized] for much the same reasons that we have seen it to be implausible for species, there is little inclination in these other cases to opt for either an individuality thesis about the corresponding "taxa". . . . (2005, 104)

But what *explains* our disinclination to treat cell types as individuals? I suspect the disanalogy between the way in which species and cells are supposed to form "trees" looms large here too. Whereas "members" of a particular species can be reconceptualized as *parts* in virtue of their causal–spatiotemporal connection, instances of particular types of cells lack the same kind of causal cohesion. There is not a single tree of which cells of a particular type might be considered "chunks" (Hull 1999, 31).

Advocates of the species-as-individuals view have been fond of saying that organisms outside the familiar phylogeny — an organism from Alpha Centauri that resembles (can mate with, shares the same ecological niche, is genetically identical with, &c.) Earthly tigers — should not count as a member of the *Panthera tigris.* They contend that their metaphysics explains and justifies this norm of classification. However, the transplanation studies mentioned above reveal that this norm's unpopularity when applied to cells. Biologists are apparently willing to treat cells from outside the normal developmental trees (products of cellular transdifferentiation, either naturally or artificially-induced) as cells of their "most recently adopted" type. This seems to tell strongly against the individualist metaphysics for cells.

Perhaps there are ways of finessing the above worries. I do not know how to do this, however. Let us thus consider an alternative: understanding cell types on the homeostatic property cluster kind model.

3. The HPC Approach to Cellular Kinds

Richard Boyd's Homeostatic Property Cluster (HPC) approach to natural kinds has rightly garnered considerable attention from philosophers of biology who despair of accommodating the heterogeneity common in biological categories on an essentialist (or individualist) model. The HPC account is built for flexibility, allowing that such kind may be associated with a *cluster* of properties, no single one (or subset) of which are necessary for being a thing of that kind.

This alone makes it a more plausible way of conceptualizing cell types than the theories we've already considered. It apparently accords nicely with biological practice. Rob Wilson focuses on neural cell types:

> Standard taxonomic presentations of [two particular types of cells] proceeds by introducing a list of features that each cell type possesses, including typical original location in the neural crest, the types of dendritic connections they typically make to other cells, the neural pathways they take, and their final locations and functions. Adrenergic cells are heterogenous with respect to any single one of these properties or

> any set of them and it is for this reason that they do not have in essence as conceived by traditional these properties tend to cluster together, and it is this feature of the form that the heterogeneity takes that allows us, I think, to articulate a view that stop short of individuality and pluralism. (Wilson 2005, 106)

Of course, it takes more than having a list of properties more or less in common for some category to be a natural kind. The third tenet of essentialism mentioned above addresses our use of natural kinds in inference and explanation by providing a particular "ontological ground" for these practices: essences explain why natural kinds are projectible.

The HPC account replaces essences with the clusters of properties themselves. As generally conceived, such clusters — more precisely, *instantiations* of many of the clustered properties — comprise causal homeostatic mechanisms that maintain the coherence of the cluster. It is in virtue of this coherence that categories associated with such clusters are apt to play the roles they play in our epistemic practices. Essences are inessential to natural kinds.

Despite this picture's attractions, I think that it too faces a number of difficult problems — specifically, concerning the theoretical role of mechanisms. I have discussed these problems in a more general context elsewhere [reference excised for blind review]; my present focus will be on the ways in which these problems become salient for the application of HPC to cell types. The HPC account retains from the traditional approach a kind of "bottom-up" stance about how kinds are to be defined. I will argue that a top-down (or at least multi-level) approach is more appropriate to the complex ways in which cells are understood.

The bottom-up stance is quite apparent in the traditional essentialist approach to kinds. Putnam explicates the explanatory role of natural kinds thusly: 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, 139). Such properties and mechanisms are "deep-lying", I suppose, in at least a mereological sense. Natural kinds in chemistry are often defined recursively in terms of structures formed by constituent sub-kinds (Slater 2005, 25–26): water has the properties it has in virtue of the fact that its essence is having a particular structure of *other* natural kinds. Mereological "deepness" thus begets explanatory deepness; and explanatory deepness in turn grounds natural kinds' reality.[11]

So the thinking goes — more or less — for HPC kinds too. Early on, Boyd emphasized the importance of causal homeostatic mechanisms for grounding the reality of kinds. He writes that that kinds "cut the world at its joints" in the sense that "successful induction and explanation always require that we accommodate our categories to the causal structure of the world" (Boyd 1991, 139). Other commentators have focused on the individuative role of such mechanisms. In his detailed discussions of the HPC approach, Paul Griffiths takes a strong stance on the individuative role of

[11] I take this to be a standard, though rarely explicitly mentioned, train of thought about natural kinds; it is beyond the scope of this paper to justify it.

explanatory mechanisms. In general, he writes, "Phenomena with the same explanation should be placed together and phenomena with different explanations drawn apart" (Griffiths 1997, 171). Categories that are not held together with causal mechanisms, on the other hand, should be rejected (Griffiths 1997, 191).

As I indicated above, there are some general concerns about both of these roles for mechanisms. First, there is an unanswered question of how to precisely analyze phrases like 'the causal structure of the world'. Second, side-stepping issues about *causal structure*, the vagaries of individuating particular causal mechanism seem poised to infect HPC kinds with an undue degree of subjectivity. Carl Craver has pursued this line of thought forcefully: "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). Third, there are some distinctly theoretical problems with using mechanisms to individuate natural kinds. For once again, we must often rely on *types of* (rather than token) mechanisms. It would be natural to want to understand such types via the HPC approach itself (it seems doubtful that biological mechanisms will exhibit the sort of pristine homogeneity that makes them amenable to essentialist treatment). But this will either initiate a circle or a regress.

A set of more specific concerns includes apparent violations of Griffiths' stance about kind individuation. It does not appear to universally be the case that we divide phenomena with different explanations or treat categories not associated with a homeostatic mechanism as natural kinds. Consider a particular cell. It features, let us suppose, a certain cluster of properties by which we individuate cells of that type. This cluster is cohesive in the following sense: within certain tolerances and circumstances, properties from the cluster are reliably found together. The cluster is "stable" — not in the sense that any time it is instantiated[12] it *stays* instantiated, but in the sense that the *pattern* of instantiations is stable across relevant counterfactual suppositions and (to some extent) across time. Simply put: it is a robust fact about the world that certain cells have features P, Q, R, S, T such that subsets of those features reliably betoken the existence of all of them.[13]

Now, what explains the robustness of this fact? The essentialist posits an essence; the HPCer posits a mechanism. Both accounts have these explanations serving a critical individuative/semantic role. In the context of the HPC account, if the stability of the same cluster of properties P, Q, R, S, T is maintained by two distinct mechanisms, we have two HPC kinds.[14] This in itself may not be objectionable. But consider: what is the mechanism that maintains the stability of the cluster associated with our (unspecified) cellular kind?

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[12] I use the idiom of "clusters being instantiated by an object" as a shorthand way of saying that (sufficiently many) properties in the relevant cluster are instantiated by that object (for the relevant purposes). Again, I will address the issue of these qualifications in the finale.

[13] I offer a more precise characterization of what I call 'cliquish stability' in §5.2 of my (manuscript).

[14] This possibility may depend on two further possibilities: (1) that the same properties can instantiate distinct mechanisms; or (2) that some mechanisms may be exogenous to the cluster (as, for example, Boyd 1999, seems to allow).

As it happens, we face an embarrassment of riches. The stability of the characteristic properties of our cellular cluster (call it 'C') depends on the proper operation of the various mechanisms operating within and without the cell — not only for the continued operation of a particular cell itself, but in view of the various "quality-control" and environmental-maintenance mechanisms embedded in the larger organism. A host of other separately-identifiable mechanisms and conditions — facts about developmental canalization, ecological factors relevant to development (Gilbert and Epel 2008), selective factors, and so on — are complicit in the stability of C. But focus for now just on the first two: suppose that we have an intracellular mechanism (really an assemblage of various mechanisms) and an extracellular mechanism that underpins C's stability. What is *the* (emphasis definite article) explanation of C's stability?

The obvious response is to "sum the mechanisms". Suppose we have an account of mechanisms up our sleeve (e.g., Machamer *et al.* 2000; Bechtel 2006). If it allows for the two lower-level mechanisms to be reckoned as parts of a larger mechanism, then we *also* have a single mechanism as required by our strict HPCer (just as a single block of wood can be composed of several smaller blocks of wood somehow fused together). The problem is that such a multiplication of mechanisms raises the chance that we will over-multiply our kinds. Suppose that in two species, A and B, different quality-control mechanisms hold sway (though the same mechanism type acts intracellularly in the relevant cells of both species). Presumably, we then have two distinct *total* mechanisms and so two different cellular kinds. If this result unacceptably complicates our epistemic lives — say the differences in the extracellular mechanisms are incidental to their stability-maintaining operation (they might exhibit themselves elsewhere) —, it is unacceptable. Since such situations seem perfectly possible (and likely actual) and since we *should not* in those situations multiply our categories, I conclude that HPC cannot, as it stands, accommodate our practices viz. cellular kinds. And since I believe that the theoretical problems with causal mechanisms discussed above are serious, it again appears that we should be searching for an alternative.

4. Cell Types as SPC Kinds

Both the essentialist and HPC accounts of natural kinds take what I have called a bottom-up stance about kind individuation. Yet, in a manner of speaking, this stance leads to opposite problems for each account: while essentialism overestimates the homogeneity of biological kinds, the HPC account underestimates it. Fortunately, I believe that a (relatively) simple fix to the HPC account gets things *just right*. In brief, the fix is this: drop the requirement that cluster kinds must be individuated by the mechanisms maintaining their stability. Indeed, drop the requirement that there be mechanisms at all! As Peter Lipton remarked in commenting on Kornblith's application of the HPC account to the problem of inductive knowledge: "Essences are supposed to hold together observable properties in stable clusters, but it is not made clear why this should make for a more inductively knowable world than one where that stability is a brute fact" (1996, 493).

While I don't think that stability is *often* a brute fact (not, at least, at the levels with which biologists are generally concerned), it seems clear that we often treat the identity of mechanisms that maintain the stability of a cluster of properties associated with a kind as inessential to the identity of our kinds. When it comes to our inductive and explanatory practices, what matters is simply *the stability* of the relevant clusters, not the various means through which such stability is achieved. The stability that lends itself to a kind's inductive and explanatory utility is often, as philosophers are apt to say, multiply realized.[15]

The account of natural kinds that emerges — what I call the *Stable* Property Cluster (SPC) account of natural kinds — improves on HPC by replacing the requirements regarding causal homeostatic mechanisms with a specific definition of stability. This has the obvious advantage of evading the problems I've mentioned here, as well as offering a metaphysically neutral (yet theoretically specific) framework for understanding natural kind phenomena. In so doing, it effectively *unifies* previously theoretically disparate "kinds" of natural kinds: we can see essentialist and HPC kinds as different points on a spectrum of stability — a spectrum that might end with Lipton's brute-stable kinds![16]

This is not the occasion to spell out the details of the SPC account or tally its virtues.[17] But I hope that it is reasonably clear how it can apply to the case of cell types. Cells are (imperfectly) associated with characteristic clusters of properties. Biologists recognize and use such types in a variety of contexts, across both organisms and species — evincing a commitment to the stability of their associated clusters. They often attempt to learn about the motley ways in which such stability is achieved. Sometimes this reveals classificatory discontinuities; other times it does not. At the end of the day, norms of biological practice — concerning, among other things, the degree to which properties cluster, the sorts of properties we ought to attend to, and degree and circumstances of their stability — tell us whether a particular category counts as a natural kind. Natural kinds, on the SPC view, are thus intimately connected with practice.

Let's consider a quick example of one of the many ways in which such nuances can get played out. Normally, glial cells serve as a sort of neuronal "glue", helping to support, nourish, and buffer neurons. Presumably such dispositional properties would be included in any cluster associated with glia, along with morphological properties unrelated to these functions. However, when glia are removed from the organism, they may lose some of their characteristic dispositions while retaining many of the structural properties by which we recognize them — that is, the cluster comes apart in some contexts.

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[15] I have not, of course, defended the view that stability is what matters (see my manuscript for details); but hopefully the fact that both the essentialist and the HPC strategies are directed towards securing this stability makes these claims plausible (as promissory notes).

[16] Not that Lipton ever (to my knowledge) expressed any commitment to such things.

[17] See my aforementioned manuscript, particularly §§5–6, for the full story.

Yet it may yet be correct to say that this cell type is a natural kind in virtue of the fact that its property cluster is stable in the context where it normally functions and hence where it is epistemically most useful to us.[18] What should we say of the particular cell in the Petri dish, though? Of what kind is it? The particulars of the case matter, but there are a few major possibilities.

First possibility: the cell retains enough of the cluster of properties associated with the kind for the relevant domain — perhaps it retains its overt morphology but not its functional competence — but in its new solo context, those properties are unstable. Second possibility: the cell lacks sufficiently many properties to count as a member of the kind *glial cell* (and the glial cluster is unstable outside the proper organismal context). In either case, I think, we can retain the common practice of speaking of the cell as a glial cell. In the first case, we might consider the cultured (or frozen or …) cell as a member of the *category* glial cell, but not treat that category as a natural kind in that extra-organismal context. In the second case, we could reckon it as a cell as a glial cell "by courtesy" — in virtue of its *history* of having been a glial cell and not its intrinsic features. We might extend this courtesy insofar as the cell can reliably teach us about its kin.

A theory of natural kinds was never (or *should* never have been) meant to provide *blanket* inductive license to project properties associated with a kind to an individual possessing some of those properties. Rather, it helps us understand how certain categories *do* serve this role, when they do. It's well known that inductive inference occurs only against the backdrop of background knowledge. This background can defeat the *prima facie* epistemic warrant provided by a category's kindhood. My interpretation of the background of the present case suggests that biologists treat cells *in vitro* as of the relevant kind in the first sense noted above in virtue of their largely unidirectional epistemic utility for shedding light on cells of the same kind *in vivo*. The case is isomorphic to that of medical students learning human anatomy and physiology by dissecting cadavers (or even to paleontologists learning about extinct species). Are such objects of the kind *human*? A "yes-and-no" answer seems compelling. They retain many of the properties of living human organisms in virtue of having a particular causal history (death need not disrupt the relevant structural properties when the cadavers is properly treated and stored), and so, studied in the proper context, allow us to reliably discover facts about living humans.

5. Conclusion

Ian Hacking has famously suggested that philosophical research into natural kinds has become "scholastic" in several senses. I want to comment on one in closing: Hacking contends that the project is scholastic in its centering on "an inbred set of degenerating problems that have increasingly

[18] That is not to say, of course, that such cells are not epistemically useful *in vitro* — doubtless, much of our understanding of the structure and function of different cells comes from careful histological work in contexts where the specific cells have *lost* many of their characteristic functions (being fixed, stained, frozen, metal-coated, &c.). But the target of these studies is typically the physiological role these cells play in the their "native environments".

little to do with issues that arise in a larger context" (2007, 229). Does this criticism hit home? That depends, for one, on how we construe this mix of metaphors — in particular, what issues we reckon arise in a this "larger context" and whether those problems are the important ones to deal with.

It seems to me that one might plausibly respond to the latter question by urging pluralism about importance. The biological sciences will clearly not grind to a halt if, as Hacking suggests, we were to forswear use of the term 'natural kind'. But that doesn't mean that there aren't interesting and important questions — to *some* people, anyway — to ask about whether there are biological natural kinds and how they are addressed by the biological sciences. It may simply be that little of practical importance hangs on the answer to this question.

This answer concedes too much, though. In investigating the patterns of diversity of cells within and among organisms of the same and different species — how it arises, how it is maintained, and why it matters to us — we are, in my view, investigating natural kinds of cells. In employing talk of cell type in certain ways, biologists are evincing a *commitment* to some cells being natural kinds. Perhaps not *all* cell types identified by biology are natural kinds in the sense I have in mind. Some may be taxa of convenience, contributing to our epistemic ends only by organizing talk. But I strongly suspect that there is a common phenomenon behind our seeing erythrocytes and electrons alike as different kinds of items, each important to their respective sciences. That phenomenon — I think — is the stable clustering of properties captured by the SPC account.

Now, I have offered here only the sheerest sketch of this account and its application to the case of cells. But hopefully others will join me in continuing the pursuit of greater understanding of the metaphysical and epistemological foundations of biological classification. Even if Hacking's criticism overreaches, he offers us an important reminder of the danger that our philosophical inquiries into science can have a tendency of losing contact with science over time. That is a tendency we should indeed fight.

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