Less Work for Theories Natural Kinds*

MATTHEW H. SLATER

(Forthcoming in *Philosophical Quarterly*)

What sort of philosophical work are natural kinds suited for? Scientific realists often contend that they provide the 'aboutness' of successful of scientific classification and explain their epistemic utility (among other side hustles). Recent history has revealed this to be a tricky job — particularly given the present naturalistic climate of philosophy of science. As a result, we've seen an explosion of different sorts of theories. This phenomenon that has suggested to some that philosophical theorizing about natural kinds has reached a sort of 'scholastic twilight' and that the concept (or family of concepts) has outlived its utility: perhaps there's *no* work natural kinds are suited for. While I think this pessimistic take is unwarranted, I will argue that it is worth rethinking the roles to which a reasonably naturalistic account of natural kinds can be fruitfully put. Natural kinds deserve a shorter work week.

1. Employment for Natural Kinds

My title is meant to call to mind David Lewis's famous essay, 'New Work for a Theory of Universals' (1983). As with universals (in Lewis's treatment), Natural kinds have been put to work in all manner of philosophical projects. The preeminent project, of course, is providing a 'metaphysics of classification' (Chakravartty 2011: 157). Construed more generally (and perhaps more metaphysically neutrally), we might think of this project as addressing what scientific categories are *about* (when they're about anything at all). How a theory of natural kinds is seen as doing so varies, but many approaches involve 'distinguish[ing] natural kinds from arbitrary categories' (Magnus 2014: 472) or otherwise 'enabl[ing] fruitful investigation into non-arbitrary classification' (Conix and Chi 2021: 8999).[1]

Unsurprisingly, many view such non-arbitrariness through a naturalistic lens, seeing it as a matter of the 'coordination' between scientific categories and natural kinds — the idea that they should 'to some degree line up with one another' (Franklin-Hall 2015: 932).[2] As Franklin-Hall notes: '[m]ost contemporary accounts of natural kinds presume that we learn about the identity of the natural kinds…by inspecting the categories and classifications and use of mature sciences'

* I received feedback on various prior packagings of the ideas in this paper during presentations at the University of Rijeka, Case Western Reserve University, The Society for the Metaphysics of Science Conference in Halifax (2023), Reed College, The University of Sydney, and The New Zealand Association of Philosophy Annual Meeting in Waikato (2023). I am grateful for the helpful comments and feedback from audience members on these occasions. I'm also indebted to Matt Barker and two anonymous reviewers for this journal who offered detailed and constructive comments on a recent ancestor of this paper.

[1] As Koslicki put it, '[n]atural kinds are best construed, not so much as kinds found within nature, but rather as classifications that are in some sense not arbitrary, heterogeneous, or gerry-mandered' (2008, 789).

[2] see also Weisberg (2006).

(*ibid.*). Khalidi agrees: 'Since science provides us with the best insight into the kinds that exist in nature, all the categories of science can be corrigibly considered natural kinds' (2013: 43). One could read such identifications as implying a bidirectional relationship of 'legitimation': that 'successful scientific taxonomies are those whose taxons correspond to natural kinds' (Bokulich 2014: 465).[3] At a first approximation, then, we might say that a theory of natural kinds aims to tell us what in the world our scientific categories are *about*.[4]

Others emphasize additional (or alternative) epistemological goals: 'accounting for' inductive–explanatory practices in science (Boyd 1991, 1999), explaining how the world is 'inductively knowable' (Kornblith 1993), or even *solving* Hume's problem (Sankey 1997). More abstract 'side hustles' have also been pursued, such as serving as truth-makers for laws of nature (Ellis 2001, 2002; Bird 2007), explaining 'reference magnetism' and other forms of metaphysical 'joint-carving' (Lewis 1983, 1984; Sider 2012); some have gone on to bring this work to bear on debates in moral philosophy (Dunaway and McPherson 2016).

Even confining our discussion to the overtly science-relevant roles (as I shall do), *this is a lot of work — too much*, I will suggest. By contrast, some argue that there's *no* work that a theory of natural kinds is suited to and that the concept should be consigned to the dustbin of outmoded philosophy (Hacking 2007).[5] Though he allowed that some classifications 'are more natural than others,' Hacking contended that 'there is no such thing as a natural kind' (203). My defense of natural kind theorizing in the philosophy of science will seek to defend the practice, in the first instance, by taking seriously Hacking's observation of the explosion of divergent approaches to natural kinds.

Thus, I begin (in §2) with a brief discussion of Hacking's meta-theoretic argument for natural kinds eliminativism. I dub it thus because the argument hinges less on any substantive objection to extant accounts of natural kinds, but rather on the splintering of philosophical research programs into distinct 'sects' following a period of consolidation under a shared paradigm;[6] he found these sectarian divisions sufficiently deep and entrenched as to resist reassembly (206). The field, in his view, was descending into a sort of 'scholastic twilight.'

At first glance, the argument might seem quite dubious. Theoretical disunity, after all, can be a healthy development in science, particularly when it occurs in response to a generally appreciated problem. Might not a similar pattern obtain for philosophy? An optimist taken with

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[3] This is Bokulich's characterization of the 'traditional philosophical answer' to the question of what 'distinguishes a scientifically useful taxonomy from an unhelpful one?' (*ibid.*); I do not take it to be her considered view.

[4] See also Magnus (2018: 1427).

[5] or worse (Ludwig 2018).

[6] 'the heady days of the 1970s,' he wistfully dubbed them, 'when Saul Kripke and Hilary Putnam did so much to give sense and use to the idea of a natural kind' (2007: 227).

a Kuhnian-style cyclical view of theoretical change may suspect that Hacking mistook the breaking dawn for twilight. Perhaps a novel unifying paradigm will soon illuminate the philosophical landscape. Reflecting on what *led* to the splintering of philosophical research programs, I argue, should temper this optimism. Indeed, such reflection points initially to a conclusion more pessimistic than Hacking's: that there is *no* way, compatible with a thoroughgoing naturalism, to satisfy the myriad desiderata natural kinds enthusiasts advocate. At best, this thought goes, we should accept a sort of 'eliminative pluralism' about natural kinds. I discuss this possibility in §3.

As a general matter, however, eliminative pluralism about a concept can be more difficult to establish than is often assumed. Arguments for this position can founder on a dilemma threatening either the pluralism or the eliminativism. In §4, I discuss Matthew Barker's (2019) argument to this effect in the context of eliminativism about natural kinds. As I interpret them, many recent positive accounts of natural kinds have opted to respond to the eliminativist challenge (if implicitly) by attempting to identify a unifying, general concept of natural kind along the lines sketched above.

In the final section (§5), I suggest another direction that merits further attention: *contract the scope of natural kinds theorizing to focus on a more cohesive and thus more theoretically tractable phenomenon*. Not only would doing so reduce the tension between several desiderata for a theory of natural kinds, but it would address what I will suggest is an overreach of many unificationist accounts. It *may* be plausible to claim that a theory of natural kinds can help us understand a broad swath of 'non-arbitrary classification' in science; but this does not entail that such theories ought to exhaust all there is to know about classificatory rectitude. I conjecture that a more productive and cohesive role for natural kind theorizing should abjure providing a univocal metaphysics of scientific classification.

2. A Crisis for Natural Kinds Theorizing

As noted, Hacking's natural kinds eliminativism stemmed in the main from the plurality of accounts he observed. Such accounts have only proliferated further in the intervening years, with many accounts diverging substantially from one another in their basic approach or details.[7] The plurality of views suggested to Hacking that natural kind theorizing would only continue on this entropic path, ultimately fizzling out. This is what one might expect to happen for a theoretical discipline with no genuine subject matter: without some worldly constraints on our theorizing,

[7] see, *inter alia,* Bird (2007), Chakravartty (2007), Wilson (2007), Koslicki (2008), Hawley & Bird (2011), Magnus (2012), Khalidi (2013), Ereshefsky & Reydon (2015; 2023), Franklin-Hall (2015), Slater (2015), Spencer (2016), Boyd (2021), Godman (2021), Lemeire (2021), Tahko (2022), and Brigandt (2022).

theorists can break in any of a number of different directions (compare the myriad responses to puzzles about phlogiston or the luminiferous aether).

But what justifies eliminativism over the obvious alternative: that there *are* natural kinds and there simply remains a great deal of contention about how best to characterize them? To return to the scientific analogy, why assume that our situation is one of *phlogiston* and not *electricity* (in the early days)? Alternative explanations of the trend line Hacking described — indeed whose continuation he *predicted* — are easy to come by. As abductive arguments go, this one seems weak.

Thus, it's worth asking what Hacking didn't: what, precisely, brought us to this point? What has changed since essentialism's dominance? Two major shifts within philosophy of science seem especially salient. First, the 1970s saw sustained attention to the philosophy of biology and other special sciences beyond the stronghold of physics as the representative science for philosophers of science. Second, and perhaps relatedly, was the growing overlapping consensus on naturalism — sometimes referred to as 'the turn to practice' — in the philosophy of science (Soler *et al.* 2014; Kendig 2016). These points come together in attempts to apply essentialist models of natural kinds to biological species (Kitts and Kitts 1979; Caplan 1981; Wilkerson 1986). Philosophers of biology were, it's fair to say, deeply unimpressed with the 'new essentialism' of Kripke and Putnam and the work of their later acolytes. Many philosophers of biology recognized that the structural essentialist model did not apply very cleanly to the biological examples that interested them most. Without surveying the (mild) temptations to essentialism about species[8] or the justified reasons for skepticism,[9] the blowback from philosophers of biology was swift. Essentialism made poor sense of the breadth of categories employed across a wider swath of the sciences.

From this basic starting point, we can make out something reminiscent of the pattern that Kuhn imputed to the sciences prior to periods of revolution: of anomaly leading to crisis, inter-paradigm chaos, and then on to the adoption of a novel paradigm. As Kuhn has it, anomaly is fundamentally a violation of 'paradigm-induced expectations' (1962: 53). Though *some* anomalies are to be expected — difficulties 'in the paradigm-nature fit' — they need not lead to any revolutionary changes: 'most of them are set right sooner or later' (82). To evoke *crisis*, anomalies needed to pile up, be especially recalcitrant in the face of attempts at resolution by influential practitioners, and generally be seen as significant. The attention lavished on the problem of applying essentialist theories of natural kinds to the special sciences — for example, in discussions of the metaphysics of species — certainly suggests that the apparent non-

[8] for defense, see Oderberg (2007: chapter 9) or Devitt (2008, 2010, 2023).

[9] for overviews, see Barker (2010) and Slater (2013a: §3.2).

application of essentialism to such categories (at least by philosophers of biology) was seen as an anomaly amounting to crisis for essentialism.[10]

This leads us to the notable effect of crisis for Kuhn: the progressive weakening of the hold of a paradigm on practitioners and resultant reopening of foundational questions. Distinct answers to these foundational questions led to divergent approaches — something characteristic of both the pre-paradigm and inter-paradigm phases. Indeed, Kuhn's prediction that '[t]he early attacks upon the resistant problem [will follow] the paradigm rules quite closely' (83) neatly comports with early attempts to accommodate biological species via 'historical essences' (Okasha 2002; LaPorte 2004) only somewhat removed from the initial strictures of the essentialism of the 1970s.[11] Whatever the merits of such accounts for species,[12] historical approaches to other messy biological categories such as cells, tissues, organs, psychological states, and such don't seem particularly compelling (Murphy 2006: 340; Slater 2013b; DiFrisco *et al.* 2020). Once the doors of essentialism were blown off their hinges, philosophers of science of various stripes took up the challenge of offering an account able to do better justice to a broader range of cases than essentialists initially considered. And as the Kuhnian model predicts, being progressively unconstrained by the initial paradigm, these attempts took a variety of forms.

An optimistic take on this dynamic sees our present dialectical situation mirrored in episodes of the history of science in which dominant but erroneous paradigms gave way to improved pictures through the cleansing fire of crisis. Consider Kuhn's discussion of the early 'electricians', who conceived of electricity as a fluid (1962: 17–18). Though mistaken, that identification led to experimental work that operated a depth practically 'unimaginable' for a community divided by disagreements about foundations (25). Such depth, Kuhn held, was necessary in many cases to reveal the more subtle cracks in a theory — hence his advocacy of 'the Baconian dictum' that 'truth emerges more readily from error than from confusion' (18). It's often better to work within a *mistaken* consensus paradigm than to never reach such a consensus paradigm in the first place.

The early congealing of essentialism into the dominant theoretical paradigm concerning the metaphysics and semantics of natural kinds likewise appears instrumental in affording philosophical attention to its application beyond the cases first considered by the theory's originators. Though one might reasonably wonder whether a generalized Kuhnian model of theory change is descriptive of theory change in *philosophy*,[13] in at least this respect, the model

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[10] This is not to say, of course, that all practitioners would have seen things this way — as it is in the sciences as well.

[11] One might even be tempted to construe Boyd's Homeostatic Property Cluster (HPC) account (e.g. Boyd 1999) as a minor variation on the essentialist paradigm — e.g., viewing homeostatic property clusters as akin to traditional essences suitable for messy biological categories.

[12] for recent defenses, see Godman (2018) and Godman & Papineau (2020).

[13] Especially where it comes to the Kuhn's aforementioned point about a shared paradigm being in some sense crucial for achieving the depth needed to smoke out subtle anomalies — a point that applies most forcefully when it

seems apt: the recognition that essentialism about natural kinds could not be neatly applied to what seemed to many be paradigm examples of natural kinds — species in particular — initiated a kind of crisis among naturalistically-minded philosophers of science (if not metaphysicians). By weakening the grip of the essentialist paradigm, freeing practitioners to rethink foundational questions treated as settled, this crisis reinitiating the search for an acceptable paradigm that better met their needs and opened up many avenues for doing so. Being the dominant paradigm, on this view (as often seems the case), contributed to its own demise.[14]

Suppose we find this to be a compelling general explanation for the *origin* of the panoply of theoretical options concerning natural kinds. The relevant next question, of course, is whether this should afford any confidence that the myriad accounts that have sprung up in the wake of essentialism's demise will eventually be consolidated under a general theoretical rubric. As the study of electricity went from a shared but erroneous paradigm (the fluid theory), through inter-paradigm chaos, to the shared consensus paradigm of modern physics (Quantum Electrodynamics), should we likewise expect theorizing about natural kinds to eventually emerge from chaos? If not, Hacking's pessimistic conclusion yet threatens. In the next section, I will describe a reason for thinking that consolidation may be difficult to achieve.

3. Desiderata in Tension

Theoretical aims and desiderata are often in tension. When this is the case, negotiating their relative priority can be tricky — both in the sciences and in philosophy. One reason for this is that such aims and desiderata are plausibly *normative stances* and, as such, matters of voluntary adoption rather than the certain product of rational argument or empirical observation (van Fraassen 2002; Chakravartty 2017). Kuhn clearly recognized but did not name this dynamic; it is what sealed the fate of simple-minded Popperian falsificationism.

Consider an example from the early days of essentialism's crisis. It was not uncommon at that time for committed essentialists to apply it to species and other biological categories (see, e.g., Bigelow *et al.* 1992) and merely accept what seem to others as untoward consequences. John Dupré offered a sharp, naturalistic criticism of work by T.E. Wilkerson (1988) who construed biological species as natural kinds in the structural essentialist mold as individuated by genetic essences. Dupré chided that 'when one's aim is to illuminate the practice and metaphysics of contemporary science, [one's theories] must surely be tempered by a passing acquaintance with

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comes to *experimental work* — there are obvious disanalogies between scientific and philosophical theories. My application of the model to philosophy is thus intended as tentative and thematic.

[14] Granted: defenders of either intrinsic or historical essentialism might contest this characterization; my comments are intended as sociological generalizations comparable to Kuhn's (who does not require that a crisis be seen as such by *all* theorists). Thanks to an anonymous reviewer for this journal for pushing this point.

the deliverances of, say, the last hundred years of scientific inquiry' (1989: 248). Undeterred, Wilkerson bit the bullet, claiming that 'the wildly heroic' solution to problems stemming from the genetic heterogeneity of species is simply to 'abandon the assumption that species are excellent examples of natural kinds. There are natural kinds…but the kinds are typically not species. Indeed, in many cases the kind will be very much narrower than the species, and may only have one member' (1995: 132).

At first glance, Wilkerson appears to prioritize theoretical simplicity or elegance over naturalistic deference to classificatory practice in science, whereas most philosophers of science these days would reverse this priority. To the extent that naturalism is regarded as a non-negotiable (or anyway, high-priority) constraint, Wilkerson is simply in the wrong (one person's hero is another's villain, as the saying goes). On reflection, this isn't quite so clear. It would be one thing for Wilkerson to naively insist that there is such a thing as the 'genetic structure of a lemon' (i.e., a particular kind of *organ* of a certain species of tree) or to argue that present species taxa are misconceived because they are not drawn along the lines of genetic essences. But notice that he is not urging a *revision to* or *abandonment of* classificatory practice in biological systematics. Rather his conclusions about natural kinds are merely untethered to the categories employed by working scientists.

Alternatively, an essentialist could simply give up on biological kinds entirely, perhaps securing realism about species via alternative means — for example, by treating species as individuals in the manner of Ghiselin (1974) and Hull (1978) — and restrain their theoretical ambitions to the (perceived-to-be-)pristine domains of chemistry and physics.[15] If these are sins against naturalism, they are sins of omission rather than commission and perhaps less serious as such — representing at most a missed opportunity.[16] That opportunity, of course, is to furnish a metaphysics for the full panoply of scientific categories.

I construe such a demand as falling into a familiar general theoretical desideratum that we might call *broad scope* or *breadth of application*.[17] It turns out, I submit, that in the context of naturalism, achieving broad scope for an account of natural kinds stands in some tension with another longstanding desideratum more specific to theorizing about natural kinds gestured at above: capturing realist intuitions about successful classification. I'll call this (*realist*) *aboutness*. We might also think of it as akin to the focus on what Magnus calls natural kinds' *ontology,* as

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[15] cf. Havstad (2018).

[16] Some commentators have argued in a similar vein that we should see discourse on natural kinds as bifurcating into two separate discussions: one centered on the philosophy of language approach (where a priori metaphysical intuitions seem to dominate) and the other on the philosophy of science where naturalism dominates (Reydon 2015; Crane 2021).

[17] cf. Kuhn (1977: 322).

contrasting with questions about their *taxonomy* — i.e., what distinguishes natural kinds from 'arbitrary' (or non-existent or ill-considered) categories (2014, 2018). As he puts the critique of essentialism, the 'essentialist accounts let an answer to the ontology question shape their entire account of natural kinds. They are ultimately willing to accept as natural kinds only those categories that meet their ontological strictures, but this yields a horribly impoverished conception [from the perspective of making sense of non-arbitrary scientific categories]' (2018, 1430).

Whence the appeal of the *aboutness* desideratum? Presumably any reasonable answer would presume a realist perspective of some sort. Consider Peter Godfrey-Smith's statement of what he dubs 'Common-sense Realism Naturalized':

> We all inhabit a common reality, which has a structure that exists independently of what people think and say about it, except insofar as reality is comprised of thoughts, theories, and other symbols, and except insofar as reality is dependent on thoughts, theories, and other symbols in ways that might be uncovered by science. (2003: 176)[18]

Within this common reality, I take it that we can readily understand what it is to be right or wrong about the existence of particular *objects*[19] — whether we are talking about the existence of a family of raccoons living under my porch or a planet outside of orbit of Uranus. What about the *categories* of *planet* or *raccoon*, though? These too seem susceptible to error or revision (Bokulich 2014; Slater 2017). What do such *categories* purport to be about?

The scientific realist's stance is that these cases should be treated equivalently. We ought to take our mature scientific theories 'at face-value, seeing them as truth-conditioned descriptions of their intended domain, both observable and unobservable' (Psillos 1999: xvii). Larry Laudan (no realist himself) was even more explicit about the role of kinds for the scientific realist: 'To have a genuinely referring theory is to have a theory which 'cuts the world at its joints', a theory which postulates entities of a kind that really exist' (1981: 24). Thus the scientific realist can conceive of *natural kinds* as an ontological category sitting alongside *objects* (and perhaps other entities) in a general 'metaphysics for scientific realism' (Ellis 2001: 2; Lowe 2006).

We are now in a better position to appreciate the tension between *naturalism*, *scope*, and *aboutness*. Consider first a metaphysically robust essentialism — either along the lines originally spelled out by Putnam or by later *scientific essentialists* like Wilkerson, Ellis, or Bird. Such an account clearly and paradigmatically satisfies the *aboutness* desideratum but apparently faces a hard choice between *broad scope* and *naturalism*. Opting for scope, as we've seen, entails

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[18] I see these exceptions as getting at something akin to Russ Shafer-Landau's (2003) conception of 'stance-independence' as a more precise gloss on the sense of objectivity or reality that extant theories of 'Metaphysical Realism' are getting at (e.g., Putnam 1981).

[19] setting aside various conundrums concerning the metaphysics of objects, of course.

revisionary consequences for scientific practice that apparently flies in the face of naturalism. Opting instead for naturalistic deference seems to require contracting the scope of one's theory — as we observed in the example of Wilkerson.

Of course, essentialism is only one sort of 'metaphysically robust' account of natural kinds. The fact that we see the foregoing tension arise in this context does not show that it is in any sense inevitable (though our suspicions on this count might be raised). Indeed, one of the throughlines of the contention over different theories of natural kinds has been variation in what we might think of as the degree of metaphysical involvement in such theories — from those with heavier metaphysical commitments (Boyd 1991, 1999; Lowe 2006; Bird 2007; Hawley and Bird 2011; Khalidi 2013) to those more (or exclusively) focused on scientists' epistemic practices (Häggqvist 2005; Magnus 2012; Ereshefsky and Reydon 2015; Franklin-Hall 2015; Slater 2015; Spencer 2016).

As a second case, consider the recent contention over the most influential post-essentialist account of natural kinds: the late Richard Boyd's Homeostatic Property Cluster (HPC) account. Boyd is admirably clear in his writings about what he took a central role of his account as 'accommodating' our inferential and explanatory practices to something in the world, which for him were 'causal structures' (1999, 146–7).[20] Here we might add the desideratum mentioned briefly in §1 above: *explaining the epistemic utility of natural kinds*.[21] As in Mill's classic (1872) discussion of Real Kinds,[22] Boyd's emphasis in this was on *projectibility*:

> natural kinds reflect a strategy of deferring to nature in the making of projectability judgments: we define such kinds *a posteriori* in ways which reflect actual causal structure precisely because we are unable to identify or specify projectable generalizations without doing so. (1991: 139)[23]

The HPC approach accordingly exhibits the further virtue of addressing both the *aboutness* and *epistemic utility* questions in a unified way, while also appearing flexible enough to apply to the biological categories that vexed essentialists. One might thus expect a concomitant expansion of scope compared to essentialism.

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[20] Others followed the same broad path, filling out some of the details, but largely sounding the same theme (Kornblith 1993; Griffiths 1999; Wilson 2005; Chakravartty 2007; Wilson *et al.* 2007); see also Boyd's (2010b), (2010a), and (2021).

[21] Such a desideratum is detectable in Putnam's early writings if one reads between the lines — for example, in his identification of essentialism's explanatory significance for the 'holding together' of a kind's 'normal distinguishing characteristics…by deep-lying mechanisms' (1975: 139). I suspect he took for granted the role that scientific categories often play in inductive inference and explanation and took such stability as a foundational element of this epistemic potential.

[22] as well as Quine (1969) and Goodman (1983).

[23] see also Keller (2003: 102–103) and Griffiths (2004: 903).

Again, matters are not so clear. While some commentators (advocates and detractors alike) often assume that the HPC account can be regarded as a *general* account of natural kinds (Kornblith 1993: 7, 35; Bird 2007: 210–211; Ereshefsky and Reydon 2015: 972), others reject this interpretation (Macleod 2010), viewing the HPC account as purpose-built for the unruly categories of the special sciences (Wilson *et al.* 2007: 218). Magnus (2014) points out that the HPC account is ill-suited to accommodating fundamental physical kinds, nor (arguably) kinds from essentialism's (alleged) stronghold of chemistry.[24] If so, it is not at all clear whether (or to what extent) Boyd's theory scores a *scope improvement* over essentialism.[25] On the other hand, interpreting the HPC account as having a wide scope (details of how to do this would need to be worked out), seems to come at the cost of naturalism (Ereshefsky and Reydon 2015; Spencer 2016; Ereshefsky and Reydon 2023).[26] I take this as further support for the desiderata-in-tension picture.

A general explanation for these tradeoffs suggests itself. Whether it involves essences, natural properties, causal mechanisms, or causal nodes, a (somewhat) metaphysically robust account of natural kinds seems likely to be (fairly) univocal. But the more widely we cast our net in attempting to accommodate classificatory practice in science, the more varied this practice will be and thus the more challenging it becomes to accommodate them philosophically. This dovetails with Magnus's contention that natural kind theories often conflate questions of ontology and taxonomy to their detriment.[27] Choices between disunity and narrow scope and naturalism become salient.

At this point, we can appreciate a deeper potential explanation for Hacking's datum of the explosion of opposed accounts of natural kinds. It is not the 'crisis' of essentialism's naturalistic failure *per se* but the underlying tensions within traditional desiderata for accounts of natural kinds that I suspect best explains this diversification. A natural reaction to the prima facie difficulty of offering a univocal, wide-scope metaphysics of scientific classification is to divide and conquer, cleaving to intrinsic essentialism, historical essentialism, HPC, SAI, and whatever other approaches are needed, each in their own ideal domain(s) of application, whatever they turn out to be. Investigators found that they could resolve the tensions between core and penumbral desiderata for an account of natural kinds in myriad ways. The loss of faith in the

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[24] see also Slater (2015, 380–1).

[25] Indeed, in the treatment recommended in my (2013a: 138–140), we should expect the same sort of mismatch between species taxa and HPC kinds as that Wilkerson 'heroically' embraced. So one might regard this as a similar violation of the naturalism desideratum.

[26] I will have more to say about these arguments in the next section.

[27] His suggestion is that we can focus on the *taxonomy question* and 'characterize natural kinds without doing deep metaphysics' — or anyway, without cleaving to a *univocal* metaphysical approach (2018, 1436). I lack the space here to discuss his view in detail.

dominant theoretical paradigm of essentialism was akin to the opening up of an array of theoretical niches to be explored. Of course, it is still possible to give an eliminativist reading of this situation: if we have all of these specific accounts, what need is there for *natural kind* (cf. Brigandt 2022: 359)? Might we, in other words, adopt eliminative pluralism about natural kinds?

4. Eliminative Pluralism and a Response Thereto

The concept of eliminative pluralism is best known through Marc Ereshefsky's (1992) approach to the species problem (the notorious plurality of distinct species concepts). His argument runs roughly as follows: there is a plurality of distinct, specific species concepts on offer — each of which carries its own theoretical advantages and drawbacks. Such concepts carve up basal lineages differently. Thus…

> Instead of referring to basal lineages as 'species', biologists should categorize those lineages by the criteria used to segment them: interbreeding units, monophyletic units, and ecological units. The term 'species' is superfluous beyond the reference to a segmentation criterion; and when the term is used alone it leads to confusion. The term 'species' has outlived its usefulness and should be replaced by terms that more accurately describe the different types of lineages that biologists refer to as 'species'. (Ereshefsky 1992, 680; see also Barker 2019, §3)

The similarities between this position about species and the parallel position about natural kinds should be apparent. Rather than advocating an overarching concept of natural kind, the eliminative pluralist about natural kinds contends that we should recognize that there is little to be gained in attempting to characterize such a concept and that it should simply split into various, more specific projects.[28] Ludwig, for example, argues that a *general concept* of natural kind has 'outlived its usefulness' (2018: 48) and that the attempt to formulate such an account 'has become an obstacle that stands in the way of further progress in philosophy of classification' (47). If anything, we're better off with the more specific accounts with constrained applications and intents.[29]

As Barker (2019) has recently shown, however, eliminative pluralist positions exhibit a certain fragility. He puts the point in terms of a general dilemma facing arguments for eliminative pluralism about a given concept. Let x be the concept for which elimination is being proposed and let $x_1, x_2, \ldots x_n$ stand for the plurality of more specific concepts that we ought to recognize instead. Now, either $x_1, x_2, \ldots x_n$ share 'some features and relationships that are of

[28] As I've noted, Magnus (2018) might opt to view this theoretical pluralism as coming in response to the *ontology* question about natural kinds, if not the *taxonomy* question.

[29] Other recent eliminativist arguments (e.g., Brigandt 2022; Papale and Montminy 2023) have followed this basic form — especially stemming from the sense that a general concept of natural kind is not called for. While there is a certain common core to these arguments, each adds their own particular spin that I will not be able to address here.

general interest to scientific [or philosophical] theorizing, or not.' If they *do*, then this looks like a 'promising basis for recognizing the general superordinate concept *x*'; if not, then 'it becomes strikingly unclear what warrants recognizing each putative category…in the plurality of x_1, x_2, … x_n as having scientific [or philosophical] interest distinct from every other category in that plurality, since the same type of doubt about the scientific interest of superordinate concept *x* also afflicts each category in the proposed plurality' (Barker 2019: 672). In the first case, the eliminativism is threatened, in the second, the pluralism is threatened.

As I read them, many of the recent wide-scope, practice-focused approaches to natural kinds can be read as attempts to grapple with the first horn of Barker's dilemma by meeting his 'integrationist' challenge (Conix and Chi 2021). Given the tensions between desiderata I have pointed out, however, it seems that *deep* and *specific* integration will remain difficult to achieve. Naturalism and wide-scope are often promoted by eschewing metaphysics entirely. Hasok Chang (2016), for example, views natural kinds as those categories that merely sufficiently effectively aid inquiry, even if that inquiry is later seen as wrongheaded (as with phlogistonian chemistry or physics involving caloric). Franklin-Hall (2015) takes a similar approach in her 'properly anti-realist account,' treating natural kinds as 'categorical bottlenecks': 'categories that well serve actual inquirers' along with 'neighboring agents' — inquirers *somewhat* different from us (940). Magnus, while focusing the epistemic contribution an account of kinds should explain to induction and explanation, does not posit *any* metaphysical constraint on how exactly this success comes about — only that the taxonomy that excluded such a kind wouldn't be as successful (2012: 48).

While such accounts may succeed in meeting the *naturalism* and *scope* desiderata (as typically understood), they do so at the cost of scratching the realist *aboutness* itch of supplying a 'metaphysics of scientific classification'. One might thus doubt that this would, as Ereshefsky and Reydon recently put it, 'give us some guidance in determining whether a classification is indeed a classification of natural kinds' (2023, 238) and thus serve as offering a *complete* integration of different, more specific natural kind accounts. Rather, their adoption represent a *choice* to focus on one subset of desiderata over others — favoring epistemology over metaphysics (to put it simply). Other attempts at integration have sought to inject a modest amount of metaphysics back into an otherwise epistemically-focused and broad scope account of natural kinds. Like Boyd and Magnus's approaches, my (2015) Stable Property Cluster (SPC) account focuses on the inductive and explanatory roles of many scientific categories while abjuring causal structure requirements in favor of a more general and flexible account of modal stability.

Yet presumably the SPC account would be subject to the same criticism Ereshefsky and Reydon direct towards the HPC account of not being sufficiently naturalistic or broad in its application. They claim that 'many successful research programs in science offer classifications that do not meet HPC Theory's prescriptions' (2015: 970); more specifically, they argue that we

should let go of the 'limiting assumption[s] that all scientific classifications ought to support inductive practices' (985) or capture similarity among kinds.[30] Such criticisms are part and parcel to their general complaint that extant accounts of natural kinds 'are not naturalistic enough' (2023: 238). The problem, they contend, is that such theories take the form of providing an 'overarching criterion' of the form 'All natural kinds have some feature X' in such a way that 'neglects large parts of classification in science' (241). Philosophers' 'tendency,' they conclude 'to propose overarching accounts of natural kinds is mistaken: universal approaches [of this sort] to successful classifications in science fail to capture the breath of classificatory practices in science' (243–4).

Thus, Ereshefsky and Reydon urge an alternative approach, what they call the Grounded Functionality Account or GFA: roughly, that we should look generally to the functionality of a category in science — explaining not just how the kinds they name facilitate induction and explanation, but, more broadly, how they help scientists achieve their epistemic (and non-epistemic!) goals — hence the 'functionality' — in a way that is grounded 'on an aspect of the world rather than *merely* on the interests and actions of human beings' (2023, 251, their emphasis). It is beyond the scope of this paper to evaluate in detail whether the GFA suffices as the integrationist account of natural kinds to rule them all and evade Hacking and Ludwig's eliminativism (see §5 of their 2023). But it is worth noting again a cost of broad scope. Whereas cluster accounts of natural kinds seem poised to explain in some detail why certain categories are apt for particular sorts of inductive and explanatory roles in science (adducing varying degrees of metaphysical commitment to do so), the GFA offers at most a very general dependence relation. Ereshefsky and Reydon do not tell us *what aspects* of the world (either in general or in a particular case) successful classification or its use depends on, just that these aspects are stance-independent. In this case, the salient tension may be between *broad scope* on the one hand and *informativeness* or *specificity* on the other. Again, a choice between which desideratum to prioritize seems inescapable.

What of the other horn of Barker's dilemma? Should natural kinds enthusiasts acquiesce to eliminativism of the superordinate concept of *natural kind* in order to defend their finer-grained fiefdoms? Of course, they would still have to meet the challenge of explaining why these more specific accounts are of philosophical interest if this interest *does not* derive from their being accounts of the general overarching concept. This need not be seen as an unmeetable challenge. If an approach embracing mere polysemy of 'natural kind' does not satisfy, perhaps a diversity of more substantive approaches — still *somehow* related to scientific classification — might suffice. Ludwig (2017, 2018), for example, commends stable property clusters and categorical bottlenecks

[30] See also Magnus (2011) on what he dubs 'similarity fetishism'.

(among other finer-grained accounts) as a useful conceptual tools for achieving various epistemic purposes that need not be seen as falling under the umbrella of natural kinds.[31] Such a position seems broadly on par with the views of Brigandt (2022), and Papale and Montminy (2023). It eliminates natural kinds *as such* while allowing that some of the specific accounts describe unrelated (or perhaps only *loosely* related) phenomena.

I believe that the best prospect for avoiding such a concession to the eliminativist involves taking a closer look at what the traditional desiderata for a theory of natural kinds requires and whether the tension I described in §3 can ultimately be lessened or avoided altogether. Can we, in other words, avoid the spur to theoretical pluralism that got us into this mess in the first place? I turn to this possibility next.

5. A Principled Scope Contraction

As I noted in §1, many natural kind enthusiasts seem to assume that a philosophical account of scientific classification will simply turn out to *be* an account of natural kinds. Such a stance is reflected as well in Ereshefsky and Reydon's criticism of the HPC account. As we've seen, their concerns have been couched in terms of insufficient *naturalism*. But while *some* scientists have expressed interest in the question of whether their categories are mere products of human interest,[32] it far from obvious that a general tenet or practice of science has it that their categories must be theorized under the rubric of natural kinds. Ereshefsky and Reydon's concern, then, seems better expressed as one of *insufficient scope*: there are scientific categories to which HPC does not apply but *should*, insofar as it purports to be an acceptable account of natural kinds (2015: 970).

The suppressed premise here, of course, is that a theory of natural kinds *should* apply to *all scientific categories* — indeed to scientific 'classificatory practice' generally (2023, 244).[33] While I cannot show *decisively* that this interpretation of the broad scope desideratum is *too* broad — an acute case, perhaps, of what Chang (2022: 79) snarkily dubbed 'correspontitis' — we can gesture in this direction by noting just how *suffused* with classification the *everyday (successful) practice of science* is. When we describe study designs and methodologies, for example — *is the study experimental or observational? If the former, is the experimental design, e.g., a Solomon 4-square, simple before-and-after, or what? &c.* — we are, *in a sense*, categorizing the world. Or consider an

[31] 'it remains unclear,' he writes 'what could be gained by asking which subset should be identified with natural kinds' (2018, 47).

[32] Have we "made' [our categories] in the process of looking?' (Briggs and Walters 1997: 361), are physiological categories "real' or are they just imaginations of our regularity-seeking brains?' (Wagner 2014: 251–252).

[33] This interpretation is supported by their identification (quoted in the previous section) of theories of natural kinds with 'approaches to successful classifications in science' (2023, 243–4).

experimenter learning how to use a certain scientific apparatus who gradually acquires a knack for discerning what belongs to the category of *a clean run of the apparatus* (meaning, potentially, the one *right here on the lab bench* rather than something more general). Ditto for other categories important to the practice of science (*HPLC grade reagents*, *samples that must be stored in the –70°*, &c.).[34]

Though these examples have little credibility as instances of natural kinds, nor are they obvious cases of 'arbitrary categories' (Magnus 2014: 472) much less 'useless' or 'unhelpful' ones. At a minimum, this suggests that we should not equate natural kinds with non-arbitrary or useful scientific categories. I conjecture that doing so has been encouraged by the insufficient attention to the false dichotomy between 'arbitrary' and 'non-arbitrary' classification — a distinction that often serves as proxy for stance-*dependent* and stance-*independent* classification. Good scientific classification is often in *some sense* arbitrary — for example, if *some* classificatory decision is needed and any among a number possibilities would work. But then again, decisions unconstrained (or loosely constrained) by empirical fact can clearly be made in *non-arbitrary* ways when we prioritize certain (admittedly stance-*dependent*) interests or pragmatic goals. Such decisions, then, are in different senses both arbitrary and non-arbitrary.[35]

Perhaps such examples can be set aside by distinguishing between *classification* and *categorization* or via distinctions within such concepts — such as classification embedded in theories (in some sense) *about the world*. Even when it comes to the classifying denizens of the stance-independent world (as it were), it is plausible that there is a distinction to be drawn between categories enjoying projectibility or what Millikan (following the coinage of Gelman and Coley 1991) discussed as 'rich inductive potential' (2000: 17) and categories that serve a much shallower but still important 'bookkeeping' or 'mapping' functions, such as higher-level categories like *fibrous protein* or *squamous cell*.[36] Theoretically-richer functions — for example, uncovering theoretically significant features of 'the Tree of Life' (as discussed in Ereshefsky and Reydon 2015: §3.2) — may not enable any distinctively inductive or explanatory triumph, but that needn't imply that they are not scientific facts worth knowing (as the existence of a planetary body outside the orbit of Uranus, say, would be a fact worth knowing).

Then again, as Anjan Chakravartty has aptly noted in response to Ereshefsky and Reydon's claim that (phylogenetic approaches to) biological taxonomy pick out non-inductive categories, it certainly *seems* that such taxa have inductive import; he writes:

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[34] for further discussion along these lines, see Slater (forthcoming: §8.5) and Barker & Slater (manuscript: ch.2).

[35] This comports with a point pressed by Ludwig (2018): there are many different ways for a category to be non-arbitrary — not all of these ways must be 'reified' as natural kinds.

[36] For numerous examples of this sort, see any histological text, such as Ross and Pawlina (2011). Of course, this is unlikely to be a *sharp* distinction — a complication I address in my forthcoming book.

> Biological taxonomy is not stamp collecting. It serves inductive success. The point of demarcating on the basis of lineages is not to identify branches on a tree for its own sake and stop there, but rather to identify groups of organisms that are evolutionarily significant: ones that are subject, as collectives over time, to biological processes described in terms of parameters including selection, adaptation, and genetic drift. Investigating and illuminating all of this involves inductive reasoning in the service of retrodictions and evolutionary explanations regarding those kinds. (2023: 67)[37]

Perhaps Ereshefsky and Reydon merely have in mind a narrower conception of inductive inference (something like Millian/Millikanian *rich inductive potential*). It may be true that properties of many historically-delimited taxa are not generally projectable in the same sense that, say, many chemical kinds are. If their point is that this does not make them gerrymandered, scientifically-unimportant kinds, then we surely ought to agree. Where we can reasonably *disagree* is in seeing this as a scope shortcoming for a theory of natural kinds.

Here is an alternative picture that I find attractive and recommend for further consideration. There is a robust phenomenon of certain categories embedded in (apparently) successful scientific practice exhibiting rich inductive potential — a phenomenon that seems to be, to at least some degree, mind-independent and grounded in empirically-ascertainable facts about the world (Massimi 2014: 416). For historical reasons,[38] it is reasonable to think of such categories as *natural kinds* even if doing so (on reflection) notably contracts their traditional scope of application. On this view, because not all scientific categories exhibit this potential, not all scientific categories correspond to natural kinds. We evade eliminative pluralism altogether (integrationist and pluralist horns alike) by recognizing a more modest remit for an acceptable account of natural kinds — one that largely avoids the tensions I identified in §3. Some may view this move as a violation of the *naturalism* desideratum.[39] But, as we've seen, that desideratum has often been expressed in an implausibly maximal way; avoiding the charge of implausibility would require articulating a principled contraction of what naturalism requires.

Judgments of theoretical success in general always presume some standard of success. These are, of course, often matters of nuanced judgment and negotiation. This fact is illustrated by another familiar lesson from Kuhn's description of theoretical change in science. One of the notable dynamics he pointed to involves scientists rethinking certain theoretical desiderata or questions. The move from the phlogistian paradigm to the oxygen theory, for example, deprived

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[37] Cracraft makes a similar point forcefully and succinctly: 'The importance of species concepts is not restricted to the seemingly arcane world of systematics and evolutionary biology. They are central to solving real-world practical problems that affect people's lives and well-being,' such as identifying disease vectors or crop pests (2000: 6–7).

[38] By which I have in mind the Millian/Millikanian tradition of *rich inductive potential* (Hacking 1991; Griffiths 2004).

[39] Thanks to an anonymous referee for this journal for pressing me on this.

theorists of previous explanations (e.g., of what made something a metal). Sometimes investigators accept such 'Kuhnian losses' because of the felt promise of successor paradigms or their ability to avoid the anomalies faced by their ancestors (Haufe 2024). Sometimes it becomes clear that a question or desideratum was, in one way or another, misconceived. Kepler was (in)famously obsessed with the question of why five planets?! Subsequent work in physics and astronomy downplayed the significance of this question; it doubtless has an answer (presuming a particular concept of *planet*), but uncovering it was not judged to be particularly revealing to the more cohesive and productive paradigm of Newtonian mechanics.

It is in something like this sense that I believe that contracting the *scope* desideratum — and concomitant nuancing of the oft-cited 'make sense of non-arbitrary classification' desideratum — increases the chances of satisfying many of the traditional desiderata for theories of natural kinds. This does not directly address the question of what such an account should look like. I have my suspicions on this front that I cannot adequately defend here.[40] I may thus be interpreted as offering a friendly amendment to accounts like the HPC, SPC, or other accounts of natural kind that focus on the phenomenon of categories exhibiting rich inductive potential: such accounts need not — and perhaps *should not* — be billed as accounts of (successful / non-arbitrary) scientific classification *writ large*, despite the way they are sometimes incautiously pitched.[41]

While I hope that it will seem plausible that contracting the remit of a theory of natural kinds will help with the joint satisfaction of the desiderata for such theories, it may not of course satisfy *all theorists*. That's as it is in the scientific case as well. Some may continue to see integration as an overriding good and opt for it at the expense of the other desiderata I've discussed. Nonetheless, I commend contraction as an option worthy of serious consideration. Insofar as we deem developing a philosophical theory (or perhaps a range of theories) of *scientific classification* as a worthy goal, we can pursue such theories independently from theorizing about natural kinds — just as scientists came to see providing a theory of combustion and metallicity as distinct endeavors. It is open to philosophers interested in providing a *general* account of scientific classification — even those not suffering from any nagging 'realist itches' — to describe such accounts as 'theories of natural kinds'. If my above suspicions of what such a general account of scientific classification will likely look like are on the right track — especially as concerns the above points about 'arbitrary vs. non-arbitrary classification' are concerned — I might suggest a different name. But such semantic squabbles are not where the real action is. The important question is how the resulting accounts — of *scientific classification* or of *natural kinds* — are to be *judged*; and here, I want to suggest, more deliberate attention and discussion is warranted. What

[40] see previously cited books.

[41] cf. Boyd (2010a: 215) and Slater (2015, 375; *mea culpa*).

project(s) are philosophers discussing natural kinds engaged with? While this question obviously has a (relatively shallow) semantic dimension, I view the question of our theoretical desiderata as a substantive normative matter susceptible to philosophical argument.

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