

Naturalized Metaphysics and the Contention over the Ontological Status of Species¹

Matthew H. Slater
Bucknell University

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The aim of this chapter is to begin exploring the connection between two debates at the intersection of metaphysics and the philosophy of science. First, we have the longstanding debate over the ontological status of biological species: are they individuals, natural kinds, or what (Ruse 1987)? Second, we have the more recent contention over “naturalized metaphysics”. What is it? What should it be? Can it be vindicated as intellectually superior to its complement within metaphysics more generally? I consider three case studies in the former debate in order to argue that attempts to employ naturalistic considerations as epistemic trump cards are likely to be unsuccessful and diagnose why this is and what it means for the prospects of naturalized metaphysics.

1. Naturalized Metaphysics and the Metaphysics of Species

In their classic text on plant evolution, botanists Briggs and Walters asked: “When we look at nature, are the ‘units’ we recognise and name already there to be recognised or have we ‘made’ them in the process of looking?” (1997, 361). Are species, in other words, *real*?² Assuming that species *are* real and there to be discovered in advance of our efforts to name and characterize them, what sorts of things are they? As Michael Ruse once asked, are they “natural kinds, individuals, or what?” (Ruse 1987).

Such questions concerning the metaphysics of species raise issues that are paradigm instances of scientific metaphysics (or as Stanford might prefer, ‘the metaphysics of science’; see Chapter 7 of the

¹ For helpful comments on previous drafts and presentations of this material, I am indebted in particular to Matt Barker, Richard Boyd, Celso Neto, Jay Odenbaugh, Zanja Yudell, and audiences at the 2013 Pacific APA and at Lewis & Clark College.

² As Coyne and Orr (2004) point out, “Most biologists certainly *act* as if species are real.... Yet a vocal group of biologists, including many botanists, dissent, claiming that species are subjective divisions of nature made for human convenience” (9).

present volume). As such, the general topic seems a fine test case for proposals on how best to “naturalize” metaphysics, particularly if considering them might help us get more precise about what naturalized metaphysics is and why it is supposed to be more valuable than non-naturalistic (or “analytic” or “speculative”) metaphysics. Yet no one (to my knowledge) has explored the relevance of the species-metaphysics debate to broader questions about the status of naturalized metaphysics debate in any detail. This is somewhat surprising given that arguments in these debates appeal to explicitly naturalistic considerations. Defenders of the dominant metaphysics of species, the Species-as-Individuals (SAI) thesis, often center their arguments for SAI on its tight relationship with common practices in systematics — in purported contrast with competing views that diverge from scientific practice in important ways. Moreover, the contending views in this domain directly intersect with subjects of independent, longstanding metaphysical interest (such as mereology, natural kinds, theories of properties, natural laws, and so on).

One might attempt to explain this lack of attention by pointing out that inasmuch as SAI-ists clearly and loudly prioritize their connection with hard-nosed, ground-level science over airy metaphysics, the case will fall squarely within naturalist territory and will thus be relatively unrevealing about the outer frontiers of a properly naturalized metaphysics. This assumption, I argue, is mistaken. Though generally sympathetic to naturalism — vague and amorphous though it is for me at this point — I do not believe that SAI represents a very clear case for the naturalist. My hope is that this unclarity makes it an interesting and potentially revealing case.

The reason that SAI is an interesting case for considering naturalized metaphysics is, in part, because it is false — or so I have previously argued at length elsewhere (Slater 2013, §§4.1–5.3). Now, this conclusion (if it holds up) tells us little itself about the twin conceptual and methodological questions of what naturalized metaphysics is and how it should be justified and

pursued. After all, naturalists need not guarantee that uncontroversial metaphysical characterizations of the relevant science will be easily derived from that science — even for cases apparently well within the naturalist’s wheelhouse. (Nor, more obviously still, should they guarantee that all of the science on which metaphysical claims are premised is true.) However, I will argue that the particular *grounds* for taking SAI to be false turn out to be revealing for efforts to naturalize metaphysics, for they are largely metaphysical and *a priori* in character. Thus, if I am right about SAI, we cannot expect to carry off the project of naturalizing metaphysics simply by always insisting that scientific considerations should trump prior metaphysical convictions; metaphysical convictions may sometimes take priority.

Now, of course, I might be wrong about SAI. My space here to make the case is limited, so I shall have to settle for a mere sketch of the argument. *If* I’m wrong, then I offer the case as a schematic depiction of what seems to be a genuinely open possibility — a cautionary fable, if you like. As a consequence, it seems to me that naturalized metaphysics will require a more subtle and open-textured characterization than has yet been offered, and (as a result) will not be very useful as an all-purpose club against *a prioristic* analytic metaphysics (or against scientific ontologies that adduce reasons falling under that umbrella in their support). We might do better to cleave to the spirit and motivations of naturalized metaphysics, in its full application across the sciences, by assuming a more flexible, pragmatic stance about what counts as good and what bad in the metaphysics of science.

2. The Naturalist Club

Demarcation projects in philosophy have a checkered history. This fact is not lost on proponents of naturalized metaphysics, as they have faced many of the same difficulties in calibrating a principled

and defensible criterion for naturalistically acceptable metaphysics between uninterestingly weak and implausibly strong endpoints. Sometimes, as Penelope Maddy points out, naturalism marks out “little more than a vague science-friendliness”; in order to qualify as *unnaturalistic*, one would have to insist — quite radically — that, for example, “metaphysical intuitions show quantum mechanics to be false” (2007, 1). Even the most imperialistic metaphysicians would abjure that level of science-unfriendliness.

How else might naturalists ratchet up the requirement of “science friendliness”? Another possibility would be to limit the range of acceptable metaphysical projects: e.g., those involved in reading off metaphysical theses from scientific theories. Anjan Chakravartty offers some examples: “what ontology of objects and processes is described by the mathematical formalism of theories in fundamental physics [or, we might add, from the special sciences]? Is natural selection a force that acts on some or other biological entity, or is it simply a statistical outcome of causal interactions acting at other levels of description?” (2013, 31). Other naturalists play variations on this basic theme. Ritchie’s (2008) “Deflationary Methodological Naturalism”, for instance, emphasizes the need for continuity between metaphysics and particular scientific projects and cautions against concluding from the latter richer philosophical theses (such as physicalism, realism, &c.). Morganti offers naturalistic metaphysicians a choice: one should “only do metaphysics as long as it can be immediately applied for the interpretation of science or, alternatively, develop metaphysics independently and then seek application of parts of it” (2013, 22).

On reflection, however, it seems doubtful that mere restrictions to a certain range of projects will ensure a robustly naturalistic orientation. Consider Ladyman and Ross’s examples of analytic metaphysicians drawing conclusions about the fundamental mereological atoms that “[lack] any basis in contemporary science” (2007, 19). They might be engaged in a project that overlaps

considerably with those pursued by particle physicists. But if the thesis that there are “extended simples”, for example, turns out to conflict with quantum mechanics, then topical cohesion aside, these metaphysicians might feel justified in arguing for the falsity of one of the most empirically successful scientific theories in human history!³ More than simply asking the same questions or operating in the same domain, a full-blooded metaphysical naturalist would also want to insist on a tighter connection between science and metaphysics.

But what should the nature of this connection be? Should metaphysics be relegated to serving as a “handmaiden” to the sciences (cf., Paul 2012), dedicated to merely working out the ontological commitments of our best theories (as Chakravartty’s first example illustrates)? Should our metaphysical theories derive exclusively and directly from scientific conclusions? Should methods used in metaphysics be modeled on used in science? Prominent naturalists have tended to broach such questions by criticizing actual metaphysical practice. For example, Ladyman and Ross criticize the emphasis by analytic metaphysicians on satisfying *a prioristic* intuitions as a key desiderata in theory construction and confirmation.⁴ We can identify two related worries here. First, it’s not clear how we might go about vindicating the general reliability of these intuitions. Second, we seem to have some reason to *doubt* their reliability. Ladyman and Ross argue that science has been a consistent source of surprise in precisely the sense that we habitually discover that our intuitive judgments about the world are wrong; we allow science to *show* us this (2007, §1.2.1). Given that

³ Conflicts of this sort can, of course, also arise from a simple lack of awareness rather than any outright hostility to science (or eagerness to see its claims refuted). We might place under this heading the early biological essentialists — apparently science-friendly in disposition — who assumed that biological species would be like chemical species in possessing some “deep-lying” microstructural essence. (How we should think of modern “scientific essentialists” more generally vis-a-vis naturalism, I leave to another occasion.) In any case, a certain level of ignorance can seem like willful disdain given the relative ease with which relevant science might be brought to bear on the metaphysics in question (Ladyman and Ross 2007, 5; Humphreys 2013, 56). A tricky question is how to characterize the proper kind of attention to recent scientific developments.

⁴ See also Ladyman’s Chapter 6 in the present volume.

preserving and systematizing these intuitions is not — and should not be — emphasized in science, why should things be any different in metaphysics?⁵ Pointed questions like this, coupled with cautionary tales of theses proffered as *necessarily true* turning out to be *actually false*, are sometimes interpreted as showing that we ought to have metaphysics either defer to the deliverances of science or adopt its methods.

But there are many forms that such deference can take. It can be absolute, in something the way that some Logical Positivists envisioned, perhaps leaving no room at all at the intellectual table for metaphysics. Or naturalists can identify *some* scope for a suitably reformed metaphysics to contribute to respectable discourse, in which case the question is *how* it should be reformed and constrained. Maclaurin and Dyke contend that the central problem with non-naturalistic metaphysics is that (when it makes ontological claims that extend beyond mere conceptual analysis) it “achieves no practical benefit because [non-naturalistic metaphysical theories like mereology] have no observable consequences” (2012, 301). Correspondingly, they allow that metaphysical debates that *do* have observable consequences will be naturalistically acceptable.⁶

While there is perhaps something to this thought that reflects an apparently common frustration among naturalistically-inclined philosophers that many metaphysical debates appear to be empirically inert, it has proven difficult to formulate and justify an approach that solves the Goldilocks problem above without imposing implausibly strong constraints on science.⁷ Ladyman and Ross attempt to avoid this dilemma and give naturalized metaphysics precise content by

⁵ I am merely presenting, not advocating, this line of thought. For two different kinds of critical discussions of Ladyman and Ross on the role of intuition, see Dorr (2010) and Maclaurin and Dyke (2012, §4).

⁶ They offer the debate about the philosophy of time as an example; see Katherine Brading’s Chapter 2 in this volume for one way in which this might play out.

⁷ For critical discussion of Maclaurin and Dyke along these lines, see McLeod and Parsons (2013) — to which Dyke and Maclaurin (2013) respond.

proposing a rather uncompromising series of principles, “referenced to the institutional factors that make science epistemically superior” (2007, 34). One such principle is *the Principle of Naturalistic Closure* (PNC):

Any new metaphysical claim that is to be taken seriously at time *t* should be motivated by, and only by, the service it would perform, if true, in showing how two or more specific scientific hypotheses, at least one of which is drawn from fundamental physics, jointly explain more than the sum of what is explained by the two hypotheses taken separately.... (37)

The PNC distinguishes acceptable from unacceptable metaphysics by placing a different kind of topical constraint on acceptable metaphysics: it should only concern itself with a certain kind of scientific unification project.

I cannot do justice here to Ladyman and Ross’s defense of this thesis; in brief, it turns on this unification project being a worthy enterprise that is not credibly left to any particular branch of science (and thus something that metaphysics *could* pursue without gross epistemic negligence). But as Melnyk points out, that this *is* a legitimate philosophical niche for metaphysics to fill does not entail that it is the *only* such niche (2013, 82–83). Nor does it seem obvious that fundamental physics ought to be so prioritized.⁸

Rather than seeing naturalistic deference as leading to a constraint on legitimate goals or topics of concern, one might see it instead as a sort of high-level methodology comprising a general encouragement to premise one’s philosophizing on developments in relevant sciences (or using their

⁸ I do not pretend that these comments add up to a strong argument against the PNC; pursuing this thread further, however, would take us too far afield from my present concerns. For more detailed critical discussion and defense, see the chapters in this volume by Waters and Ladyman.

methodological tools) together with a “science-first” policy for conflict resolution: namely, *resolve all apparent conflicts between metaphysics and science by siding exclusively with science*. This sort of “meta-methodology” would presumably only be regarded as a necessary condition of naturalistic metaphysics. Ladyman and Ross hint at something of this flavor in the lead-up to their statement of the PNC:

Since science just *is* our set of institutional error filters for the job of discovering the objective character of the world — that and no more but also that *and no less* — science respects no domain restrictions and will admit no epistemological rivals (such as natural theology or purely speculative metaphysics). With respect to anything that is a putative fact about the world, scientific institutional processes are absolutely and exclusively authoritative. (Ladyman and Ross 2007, 28)⁹

Much more could be said by way of explicating and justifying different approaches to naturalistic metaphysics. But let us take this as a key naturalistic commitment and see how it plays out in the context of discussions of the metaphysics of species.

3. Anti-Essentialism: A Naturalist Success Story?

Even before we had the beginnings of a theory of the *origin* of species, some pre-Darwinian naturalists (in the older sense) recognized the existence of species and sought to understand their properties and relations. From a certain vantage point, such questions might be thought to involve metaphysical speculation from the get-go. What *are* species? Into what ontological category do they fit? The development of evolutionary biology from Darwin into the early 20th century did little to

⁹ I take “no epistemological rivals” in a weaker sense than it was perhaps intended to imply the science-first policy of conflict-resolution.

settle this question explicitly, at least in the minds of (even naturalistically-inclined) philosophers. Quine, for instance, addresses the question of species' ontological category *en passant* in his famous discussion of ontological commitment: “when we say that some zoological species are cross-fertile, we are committing ourselves to recognizing as entities the several species themselves, *abstract though they are*” (1953, 13, my emphasis). A popular way of implementing this basic thought, of course, involves treating species as natural kinds. While Quine himself doubted that natural kinds would play any enduring role in mature sciences (1969), his successors were generally inclined to view natural kinds as key targets of scientific investigation (Kornblith 1993). And to many philosophers, species seemed like *paradigmatic* natural kinds in precisely this sense — along with such kinds as gold, water, electrons, and so on. Perhaps inspired by the growing sophistication of molecular biological techniques at the time, Kripke (1980) and Putnam (1975a, 1975b) famously attempted to subsume them under the essentialist model, suggesting that, like the physical and chemical paradigms, species would also be characterized by underlying micro-structural essences. More robust arguments for this essentialist interpretation followed over the following decades (see, e.g., Caplan 1981; Wilkerson 1995; Devitt 2008).

But this nascent metaphysics of species — that they are natural kinds characterized by essences — did not remain in favor for long. Several influential evolutionary biologists had already been campaigning against treating species as abstract types in either a Platonic or Aristotelian conception (see, e.g., Mayr 1959, 28–29; Ghiselin 1966, 1974). Biologically-sophisticated philosophers gradually took up the thread and pronounced the abject failure of natural kind essentialism as applied to biological species. Elliott Sober wrote in a well-known article that “Essentialism about species is today a dead issue, not because there is no conceivable way to defend it, but because the way in which it was defended by biologists was thoroughly discredited” (1980, 353) and a poor

explanation of the patterns of variation we observe. Others focused less on the manner of essentialism's defense than on the empirical falsity of its core commitments: that for each species, there is some underlying microstructural (presumably genetic) property shared by all and only its members which makes them the sorts of things they are. Increasing understanding of the nature of development and the extent of intraspecific genetic variation rendered these claims significantly less plausible.

So we have here what seems to be a pretty obvious naturalist victory. A metaphysics of species is proposed and subsequently falsified by empirical investigation. Of course the story is a bit more complicated than this brief gloss lets on — but perhaps only a bit. For one, the historical narrative offered by Mayr (and taken up by many philosophers of biology) concerning the development of essentialism has been questioned (Winsor 2003, 2006). For two, Sober's pronouncement of essentialism's death might have been, as they say, greatly (or anyway *somewhat*) exaggerated (Devitt 2008). Essentialism about biological taxa persisted since the 1980s in two forms that we might call *historical* and (tendentiously) *bullet-biting*. The former involved abandoning one of the standard tenets of essentialism: that a natural kind's essence must be an intrinsic property. Perhaps species kinds could be seen as defined by *historical essences* — e.g., by their position on the tree of life in relation to other taxa (Griffiths 1999; LaPorte 2004; Williams 2011).

Set this strategy aside for a moment and consider, by contrast, the bullet-biting strategy for defending essentialism. One might pursue it in two different ways. First, one could propose simply revising the boundaries of the relevant kinds so that they could be characterized by intrinsic essences (e.g., Wilkerson 1995, 132). Second, one might leave recognized taxa alone and argue that they just

must have essences, even if so far unidentified.¹⁰ Devitt, for instance, argues from the existence of lawlike generalizations about species (e.g., ‘All Indian rhinos have a single horn’) that these generalizations must be explained by intrinsic biological essences. Though this gambit leaves biological classification more or less unmolested, it does seem to entail that key pieces of biological *practice* — the way in which systematists have gone about describing species and their relations — is seriously misguided. Devitt’s argument would, at the very least, imply that biologists have been systematically ignoring a key explanatory burden in biology (cf. Lewens 2012b, 752). This alone might suggest to the naturalistically-inclined metaphysician that something must be amiss in Devitt’s arguments (or in the other bullet-biting strategies).¹¹ Contemporary biological practice, like most cases of scientific practice, is motivated by its record of bearing fruit (or promise for doing so) on matters deemed significant by the community pursuing it. As such, it should not be abandoned (or significantly modified) lightly.¹²

Contrast the historical essence strategy mentioned above. In that case, it was (interpreted as) standard practice of evolutionary biologists to treat species as historically-defined kinds so that, as Griffiths puts it, “nothing that does not share the historical origin of [a given] kind can be a member of [that] kind” (1999, 219). Though this approach does less violence to large portions of biological practice — effectively leaving alone cladistically-inclined classification schemes —, it does apparently foreclose on what seem to many live options about how to characterize species. Suppose one was interested in pursuing species concepts based on shared gene clusters (Mallet 1995), interbreeding

¹⁰ Maybe also even if there is specific reason for *doubting* that there are such properties.

¹¹ Though Devitt offers some brief comments concerning the use of genome sequencing projects in revealing the intrinsic essences of various taxa — suggesting that he *is* concerned with biological practice —, one does not often observe biologists discussing these projects in terms of the discovery of species’ essence.

¹² Devitt’s arguments may of course fail on their own terms, as I have argued they do (2013, §3.3); see also Barker (2010).

(Mayr 1963; Coyne and Orr 2004), or phenetic similarity (Sokal and Sneath 1961; Lewens 2012a).

The restriction on sharing a historical origin Griffiths posits would have to be grafted onto these species concepts artificially and without internal scientific motivation. This is analogous to the awkward situation Callender discusses concerning the “strange new laws” some metaphysics are forced to postulate in order to accommodate their metaphysical views about “extended simples” to findings in theoretical physics (2011, 39).

So a comparatively mild naturalistic rebuke seems appropriate here as well. To those with even slightly heterodox (or pluralist) approaches on the species problem, historical essentialism (biological motivation notwithstanding) overreaches. Of course, this rebuke may not move those antecedently committed to classification strategies which already incorporate Griffiths’ stricture. This raises a question about what degree of scientific ecumenicalism a naturalized metaphysics should allow. But this is a question I must leave aside here. For now, let us suppose that, a few complications aside, the anti-essentialist consensus in the philosophy of biology is a case of due attention to the relevant science refuting otherwise appealing metaphysical approaches. I want to now turn towards two other cases — one positive, one negative — that do not transfer quite as cleanly. We start with the positive: the claim that species are individual objects with organisms as their parts (rather than natural kinds). We’ll then consider a criticism of an alternative conception of species as natural kinds.

4. Individualism

The thesis that species are individuals (SAI) was originally motivated in large part by the so-called “species problem”. As standardly described, the problem is how to formulate a satisfying response to the longstanding dispute amongst biologists about how to characterize species (Wilkins 2009). One

can interpret this challenge in two different ways: first, there's the challenge of actually *resolving* the ground-level dispute by, say, showing conclusively (or satisfactorily) that a particular conception of species was correct; second, there's the challenge of squaring the prevalent sense that species are in some sense real, objective features of the world with the fact that even after decades (if not centuries), biologists cannot agree on how they should be defined. What sort of features are they?

SAI was originally proposed by the evolutionary biologist Michael Ghiselin as a solution to the species problem construed in this second sense.¹³ Here's the basic thought: rather than thinking of species as natural kinds characterized by intrinsic essences, we should regard them as spatiotemporally-extended concrete particulars — persisting composite objects made up of their organisms. Unlike paradigmatic objects such as rocks or tables, these species objects would typically be scattered *at a time*, but they would nevertheless be connected *over* time by the sorts of lineage-forming relations (such as interbreeding and gene-exchange) that biologists often employed for demarcating species in the first place. The objectivity of the parthood relation's obtaining would secure the objectivity of species against the threat posed by the species problem. Whatever disagreement there was about how to conceptualize species, there would be some fact of the matter about how organisms were united into larger, persisting individuals, rendering (so the thought went) some concepts objectively better than others at mirroring this structure.

But why accept SAI? That it *would* solve a problem (if true) does not, by itself, show that it *is* true. Without surveying all of the relevant arguments (see Slater 2013, §4.2 for more), it's worth mentioning a representative few. In the first place, concerns about essentialist models of natural kinds (alternatives to which were not clearly seen when SAI was first proposed) loomed large. In a

¹³ Indeed, the (1974) paper in which SAI received its fullest initial presentation was titled "A Radical Solution to the Species Problem".

two-category ontology — of universals and particulars (or abstracta and concreta), say — a simple eliminative inference suggested that, if they are not abstract types, species must be concrete particulars. This move cohered nicely with Ernst Mayr’s campaign to embrace “population thinking” as an alternative to “typological thinking”. On this line of thought, species are certain sorts of groups of populations (or better: *meta-populations*), and these aren’t like *properties* that things can have; they’re *things* with properties themselves, with a certain spatiotemporal extent — in other words: they’re individuals.

A second, and related, style of argument focused on pressing for an analogy between the way biologists think of organisms and how they think of species — for example, in how much disruption or change they can endure or in the level of “cohesion” among their parts (Hull 1978, 347; 1989, 84; 1999).

A third argument strategy highlighted the explanatory power of the SAI thesis. Why are species spatiotemporally restricted (Ghiselin 1987, 128)? Why are there no natural laws about particular biological species (Hull 1977, 150)? Why are biologists hesitant to speak of organisms as “instances” of species — of a particular organism exemplifying the property of “tigerhood”, say (Ghiselin 1974, 536)? All of these questions, the argument goes, would receive satisfying and unifying answers on the thesis that species are individuals. Thus we should tentatively conclude that SAI is probably right.

A certain naturalistic orientation is clear in each of these broad styles of argument. The first is premised on empirical considerations coming directly out of biology. It is an extension of the apparently successful naturalist critique of essentialism (setting the aforementioned complexities aside). The second stems from the attitudes of biologists in how they think about and refer to species. Responding to philosophers’ objections to the argument-by-analogy strategy (e.g., Kitcher 1989), Richards notes that:

Those who work in the biological sciences typically don't see such a distinctive and important disanalogy between individual organisms and individual species taxa.

There are, I believe, several reasons based on familiarity with biodiversity, tradition and disciplinary practices.... A full consideration of biodiversity reveals the bias in philosophers' commonsense notions of individuals, and its focus on vertebrates and humans. (2010, 163–164; see also de Queiroz 1999, 67)

So perhaps biologists' training and experience in their science disabuses them of the need to see individual objects as spatially cohesive — rather like experience in physics disabuses us from naïve conception of objects as solid and continuous.¹⁴ The final argument strategy is transparently naturalistic, using explanatory patterns of inductive inference familiar from the sciences and drawing on purported facts that while familiar to biologists might seem surprising to the uninitiated layperson. After all, this gap between expert and lay beliefs about species could be cited as a main factor behind philosophers' essentialist foibles when it came to species.

Relatedly, some SAI advocates have argued in a Quinean spirit that the way that biologists refer to species strongly legislates for the individualistic interpretation. Unlike the lay who might think of, e.g., *being a tiger* as a property that things can possess, these SAI advocates will point out that the practice of demarcating species historically indicates that 'tiger' functions instead as a singular term. Here's Coleman and Wiley's version of the argument:

Biological theory is replete with generalizations that seem to be about particular things called "species." The generalizations "There are species" and "Species are

¹⁴ Again, I am not granting these points here. My present focus is descriptive and interpretive. Barker and Wilson (2010) have argued compellingly that the kind of cohesion enjoyed by the members of a species is not especially compelling as an integrating force.

variable” appear to be statements that are truly or falsely said about species and not about the organisms that comprise a species.... Thus, one way to interpret discourse about species is to understand at least some of the expressions used to talk about particular species taxa as genuine singular terms (i.e., as terms referring to particular things rather than designating kinds of things). This manner of interpreting talk about species comprises what we call an “objectual account of species.” (2001, 499–500)

This argument from ontological commitment would of course be stronger if it could be shown that biological discourse could *only* be captured by treating species names as singular terms — something that Coleman and Wiley attempt later in their article.

Even if none of these arguments are conclusive, it would not be unreasonable to feel swayed by their collective weight. For they each appear to be grounded, in one way or another, in our best biological science. Add to this the seeming consensus among philosophers of biology and biologists attending to the matter, and it can start even to seem *foolish* to suppose that one could argue against SAI from a metaphysical perspective — particularly if one felt any naturalist sympathies. Would this not be tantamount to a non-naturalistic stance? I think that the answer is *no*; but vindicating this answer requires articulating a different vision of naturalized metaphysics. While I won’t be in position of completing this task in any detail in this chapter, I want to set off in this direction by considering a way in which (apparently) purely metaphysical considerations tell against this popular naturalistic metaphysics of species and thinking about how we should consider this from a naturalistic perspective.

The considerations in question emerge from attention to the metaphysics of vagueness and indeterminacy over the past few decades, specifically as they concern the possibility of indeterminate

parthood, identity, and existence. A good starting point is Gareth Evans' much discussed (1978) paper purporting to demonstrate the incoherence of ontic indeterminate identity — that is, the incoherence of identity statements which are indeterminate in truth-value *not* due to any semantic defect or imperfect knowledge. Like many later commentators, Evans took his argument as addressing the coherence of the idea that “the world itself might *be* vague. Rather than vagueness being a deficiency in our mode of describing the world, it would then be a necessary feature of any true description of it” (208). The argument takes the form of a *reductio*. Suppose that some thing(s) *a* and *b* are indeterminately identical (and that this is a metaphysical, not a semantic fact due to referential ambiguity). Another way of putting this is to say that individual *a* has the property of *being indeterminately identical to individual b*. However, *b* doesn't have that property: it's definitely itself! So there's some property — *being indeterminately identical to b* — that *a* has but *b* lacks. Since, by Leibniz's Law, something can't have different properties than it has, it must be that *a* and *b* are in fact distinct; and if they are provably distinct, they are not indeterminately identical after all — contra our supposition.¹⁵

Evans' result has been extended to contest the possibility of indeterminately *existing* objects. In Ted Sider's argument, we begin again from a supposition (for *reductio*) that there are indeterminately existing objects. From this it follows that there are numerical sentences expressing how many objects there are — sentences of the form ‘ $\exists x \exists y \exists z \dots (x \neq y \ \& \ x \neq z \ \& \ y \neq z \dots)$ ’ — that are indeterminate in truth-value. But there is nowhere for that indeterminacy to lie, as the vocabulary of such sentences contains only sentential connectives, unrestricted quantifiers, and identity, none of which can admit

¹⁵ Lots more could be — and has been — said about the Evans argument and the issues surrounding ontic vagueness; for helpful discussion, I would see in particular Lewis (1988), van Inwagen (1988), Heck (1998), Moore (2008), and Barnes (2010).

of indeterminacy. So the sentences are not indeterminate in truth-value after all; *reductio* achieved (Sider 2001, 125–130).¹⁶

What does the prohibition against ontic indeterminate existence and identity have to do with SAI? The problem is that biological discourse on species concepts exhibits little interest in obeying this dictate. Worse, it displays the opposite commitments. Every species concept of which I'm aware allows some indeterminacy in its application.¹⁷ Yet this discourse is what tells us how organisms are united into species — including, whether some organisms comprise one species or multiple species. Thus if we interpret SAI as a metaphysically robust account of what species are — that they are *real, left-alone objects* — our best biology leaves us with an apparently intolerable conclusion: that some objects only indeterminately exist (or are indeterminately each other).¹⁸ I conclude that the SAI thesis is false.

Call this the Indeterminacy Problem for SAI. Now of course there are a number of ways in which an SAI sympathizer might respond. Let us set aside responses that significantly weaken the

¹⁶ As with Evans' argument, Sider's has not been without its critics — to whom Sider has responded (see Koslicki 2003; Sider 2003; Liebesman and Eklund 2007; Sider 2009; Barnes 2010). And while there is certainly room to press further, my sense is that its conclusion is extremely plausible. Even Barnes's (2010) critical discussion of Sider (2003) allows that "Indeterminate existence is deeply problematic when interpreted *de re*. The claim that there is some thing such that it's indeterminate whether that thing exists is hard, if not impossible, to make sense of" (960). David Lewis expressed this same thought when he asked (rhetorically) of a putatively indeterminately existing object: "What is this thing such that it sort of is so, and sort of isn't, that there is any such thing?" (1986, 212).

¹⁷ I document this in some detail in §4.3.2 of (Slater 2013), but to get the flavor, we can consider a popular species concept like Mayr's "Biological Species Concept" (BSC) according to which a species is a group of "interbreeding natural populations that are reproductively isolated from other such groups" (Mayr 1963, 17). However, reproductive isolation — at least, as it is used in this species concept — is not generally a sharp notion. Whether two populations count as members of one species or two sometimes depends on the degree to which their members can exchange genes (Sterelny and Maclaurin 2008, 28). One might argue that it's possible to sharpen this criterion and avoid the threat of indeterminacy. But while there are certainly *some* pairs of organisms for which such isolation is very much an all-or-nothing matter, a general insistence on sharpness leads to some implausible consequences that biologists are not at all eager to embrace. Indeed, many explicitly acknowledge a certain level of vagueness in their account (see, e.g., van Valen 1976, 70–71).

¹⁸ One might hope for an epistemic or semantic resolution of the Indeterminacy Problem. Alternatively, one might attempt to downplay the significance of ontic indeterminacy — for example, by suggesting that it is in fact *everywhere*. I discuss why I believe that these strategies will fail in (Slater 2013, 92–95).

metaphysical import of the thesis — say, by construing it as a sort of *façon de parler* or a semantic thesis of some kind. These may be worth considering, but they are of little interest in the context of a discussion of naturalized *metaphysics*. One might attempt to contain the problem for SAI without engaging directly with the metaphysics of indeterminacy by reinterpreting the SAI thesis in a non-mereological way (Haber 2013, 2015). At best, I think this postpones answering key questions: How are *biological parthood* and existence (or *biological existence*?) conceptually tied? What is the metaphysics of *biological parthood* such that it evades the indeterminacy problem? Does it do so, for example, by taking a deflationary approach to the metaphysics of species? If not, then so long as SAI involves ontological claims about what exists and what is identical with what, it's not clear to me how the precise formulation of a theory of biological parthood will be able to address this problem (Slater 2013, 89).

A more direct way of responding, of course, engages directly with the Evans/Sider arguments. Might a naturalist simply “recalcitrate” (as Quine put it) and take the existence of indeterminately existing (or indeterminately identical) species for granted and use it as evidence that the Evans/Sider arguments are unsound? Of course; but doing so is unsatisfying. Where specifically does the argument go wrong (cf. Dorr 2010)? Suppose that the SAI-naturalist replies as follows: *I don't need to tell you where these arguments go wrong. I have the falsity of the conclusion — that indeterminately-existing things are possible — directly from some very well-confirmed science (evolutionary biology and systematics). All you have is your a priori argument that this is impossible. But since such empirical considerations trump a priori, we can see that these arguments must go wrong somewhere.*

This response won't do as is, however. First, it's obviously false that the empirical should *always* trump the *a priori*. Imagine an investigator coming to a conclusion at odds with some piece of pure mathematics; she would scarcely consider thinking “well, so much for arithmetic!” Rather, she would

double check her work and look for a mistaken assumption or botched procedure, explaining to colleagues that the numbers just didn't add up.¹⁹ Now, one might object at this point that mathematics constitutes a principled exception and that our imagined naturalist's reply should be interpreted as committed to the thesis that the empirical always trumps *a priori metaphysics*. Presumably few naturalists would wish to advocate a blanket version of this claim. But then the question is what — besides the *a priori* or *a posteriori* status — of the competing claims justifies the dismissal? If there are exceptions to this policy, how should we recognize them? Relative epistemic weight matters and is not exhausted by the epistemic modality by which we come to believe something.

Relatedly, and more to our present concerns, E.J. Lowe argued that certain physical scenarios involving quantum entanglements reveal the falsity of Evans' conclusion about the impossibility of ontic vague identity (Lowe 1994). Without descending too far into the detail, he considers a scenario in which an electron is absorbed by an atom and another electron is later emitted. Is the emitted electron the same as the absorbed electron? Lowe claims that there's no saying one way or the other (due to quantum entanglement); but not because we don't know enough or because our names are fuzzy. No, the indeterminacy is ontic. So there we go: ontic indeterminate identity, contra Evans, is possible. And this is shown by one of the best-confirmed scientific theories in the history of science.

The case is not so simple, however (for discussion, see Noonan 1995; Hawley 1998; French and Krause 2003). Nor is the comparison obviously helpful for supporters of SAI. For one, and to Lowe's credit, he did not simply invert a *modus ponens* into a *modus tollens*; he offered a diagnosis

¹⁹ This is not, of course, to say that the reverse tie-breaking principle should be employed. As noted above, we've seen purportedly *a priori* axioms of Euclidean Geometry overturned by results from physics. And lest we chalk that episode up as a curious historical anomaly to be reclassified somehow, note that more recent thinkers have felt pressed, if not moved, to reconsider other *a priori* claims on the basis of empirical discoveries. For example, Quine (1981) entertained the possibility that certain interpretations of quantum mechanics might even compel us to drop such logically sacrosanct principles as Bivalence.

of the Evans argument. More significantly, as the critical discussion of the paper made clear, a good deal of interpretation of the robust experimental and theoretical results of quantum mechanics is necessary in order for Lowe's case to do the job it was meant to do. One has to do some metaphysics of science in order for the experimental results to bear on further metaphysical matters. This appears to be a case in Chakravarty's general point that "even on the most metaphysically austere, contemporary conception of the sciences...there is a metaphysical dimension to [scientific] inquiry" (2010, 70).

This illustrates a second reason why we should find the recalcitrating response to the Indeterminacy Problem is unsatisfying: the conflict is not *simply* between a metaphysical thesis about ontic indeterminate existence and a robust empirical result; in fact, both theses have metaphysical content. Suppose that the SAI-advocate argues on naturalistic grounds that the closer proximity of their metaphysical thesis to empirical science breaks any imagined epistemic tie: on the one hand, we have a metaphysical thesis that is continuous with current science; on the other, we have speculative *a priori* metaphysics at great remove from empirical science. I am unhappy with this characterization of the dialectical situation, however. In the first place, the asymmetric labeling of these sources as "speculative" begs important questions; anyway, conclusions in science are often themselves highly speculative. But set this concern aside and grant that the relevant empirical results in biology are as epistemically secure as you like. This is just one part of the story. While there is a certain temptation to gauge the relative epistemic priority of the competing theses on the degree to which they involve metaphysical or empirical premises, the contest also should involve the strength and character of the inferences from these premises. Here matters become shakier and dependent on further metaphysical assumptions, especially when the inferences are eliminative (e.g., when one argues that species must be individuals because they are not natural kinds), as these inferences assume that we have seen

clearly all of the relevant alternatives — something that history shows is often unreasonable to assume (Stanford 2006).²⁰ Consider Haber’s claim that “species are spatiotemporally located, concrete entities, with a beginning and an end, and have parts, not members, is a function of how lineages are generated, and of the facts of reproduction” (2015, 15). But it’s not at all obvious how the biology of reproduction entails these further characterizations without the intervention of further metaphysical theories. Biology can tell us how lineages in fact form, but it does not tell us how they must form or how we should conceptualize them metaphysically without significant interpretation.²¹

5. Homeostatic Property Cluster Analyses

Let us examine a final example in which naturalistic grounds for advocating a particular thesis on the ontological status of species do not clearly carry the day. In the case of SAI, we considered concerns over a positive metaphysical account despite its being naturalistically motivated. This may be disappointing for naturalists, but perhaps it simply represents an overreach. It thus may be instructive to consider a more modest critical argument against a particular metaphysical perspective on species that proceeds on naturalistic grounds but which also (I think) fails.

The case involves Boyd’s Homeostatic Property Cluster (HPC) account of natural kinds (Boyd 1999a), an account that many philosophers of biology argue evades the criticisms levied against essentialistic conceptions of natural kinds (see, e.g., Rieppel 2007; Wilson *et al.* 2007). Species might be natural kinds, on this view, in virtue of sharing a cluster of properties, perhaps imperfectly, whose broad stability/sociability is maintained via the operation of certain homeostatic mechanisms. In my view, the HPC account faces a number of serious theoretical issues and problems for its application

²⁰ For some suggestions of previously unconceived alternative on this front, see (Slater 2013, Chapter 5).

²¹ I am indebted to Celso Neto for comments that (hopefully) clarified this section.

to species taxa; I am not an advocate (Häggqvist 2005; Reydon 2009; Slater 2013, §6.2.2; 2015).

But it has recently come under some criticism that I believe represents naturalist-overreach.

A chief critic has been Marc Ereshefsky who has argued in several papers that HPC theory offends from prevailing views in biological systematics. Naturalistic concerns are clearly front and center in his argument: “The HPC account of biological taxa should be informed by, or at least consistent with, biological systematics” he writes; “But that does not seem to be the case: the historical essentialism of HPC theory is not historical enough for contemporary biological systematics” (2005, 17). Why not? The answer stems from some critical comments Boyd levies against constraints certain systematists insist upon but which Boyd finds to be unmotivated (1999a, 180–182; 2010) — in particular, he allows that HPC kinds might be para- or polyphyletic. Here Ereshefsky objects: “This should not rest easily with those who believe that [species or] higher taxa must be monophyletic” (2007, 296).²²

But the issue here is a bit more subtle than Ereshefsky allows. We can agree to the insistence that HPC theory ought to be informed by and consistent with empirical results in biology; but it is not clear that it must be *just as demanding* as a particular school of systematics that not all biologists happen to advocate. This would be a very strong and implausible interpretation of consistency. Does consistency with biology require that biologists be disposed to adopt (or at least not reject) the theory in question? That’s not obvious either. I argued above that SAI’s popularity among biologists was not particularly probative; likewise, the fact that some biologists may be expected to reject HPC

²² A quick and dirty primer on some of these terms: taxa are monophyletic if they consist of all and only the descendants of a common ancestor; advocates of the monophyly requirement are sometimes called ‘cladists’, though (confusingly) this term is also sometimes used interchangeably with more general “phylogenetic” approaches to systematics that see historical approaches to taxonomy as fundamental. Different phylogenetic approaches might recognize paraphyletic taxa, which contain *only* descendants of a common ancestor, but not *all* of them (it might leave a branch off) or polyphyletic taxa, dropping the “*only*” requirement as well: they may comprise some descendants of multiple lineages.

theory does not, in and of itself, show that it is inconsistent with biology. After all, biologists — even those with a phylogenetic orientation — disagree with each other about how to organize taxa (de Queiroz 1999; Wheeler and Meier 2000). Consistency with biological systematics will remain elusive as long as extant theories of biological systematics are not consistent with each other.

But set this biological conflict to one side. Suppose (what is radically contrary to fact), that biologists *were* united in advocating monophyly as a constraint on how species and higher taxa ought to be individuated. Would this not reinstate the force of the naturalist criticism of HPC? Perhaps it would make the case stronger, but again I don't believe that HPCers ought to feel forced to capitulate. For just as we are entitled to differentiate between the epistemic worth of particular empirical theses, we should take seriously the potential for acknowledging epistemic differences between such theses and prevailing classificatory practices. While it is tempting to place all aspects of scientific practice onto the same epistemic pedestal as its acknowledged fruit, we should recognize the possibility that such practices do not contribute equally to the epistemic worth of science and so do not clearly merit trumping force when apparently in conflict with metaphysical theses. Similar considerations can be applied to naturalistic arguments stemming from the ways in which a particular community of scientists talk. If there are other possible ways of classifying, investigating, and talking that would lead scientists to comparative success, we ought to be cautious reading off conclusions from these considerations.

I will return to some consequences of this stance in the final section. But before we move away from our final case study, it's worth noting two further contrasts between the (in my view) successful anti-essentialist case and this unsuccessful anti-HPC case. In the former case, essentialists were making a claim with content that conflicted with a well-established empirical result in biology — e.g., that species taxa share a common “genetic structure” that makes them the species they are. That

seems not to be true (nor should we expect it to be true, given other things we know about evolution, development, and the sources of variation). In the latter case, it's less clear that the conflict between HPC and cladists has the same empirical character. Cladists may say "biological taxa are monophyletic", but it is not evident that this is an empirical claim in the same way that "biological taxa are genetically heterogeneous" as much as it is a *normative* claim about how we ought to group organisms together — that is, "biological taxa should be grouped according to monophyly". Such a claim is not, as it were, read off from the data. Granted: it is an oversimplification to suppose any scientific claim can be so read, or regarded as "true on inspection", but arguably there is a qualitative difference here.²³

There's more to say here, of course about classificatory norms — how they interact with empirical content of scientific theories, how they come to be, how they are embedded in scientific practice more generally. My discussion has so far remained rather general (and so it must remain in this context).²⁴ It is premised on an undefended but I think plausible claim: that classificatory practice, like any component of scientific practice, results from a complex interaction between contingent normative commitments about values, aims, and significance-ascriptions and empirical facts (which are in turn partially conditioned by those norms); such complex interactions will involve trade-offs and thus tend to demand a pluralistic outlook in those cases. But there is no similar call to be pluralistic about claims involving the degree of genetic homogeneity we find in certain kinds of biological taxa (so long as one fixes on a particular understanding of the taxa involved).

²³ Note also that even it turned out that species are substantially artifacts of our classificatory whims, the empirical content of these claims would still differ.

²⁴ In other work (Slater MS), I have argued that much of classificatory practice is guided (and not determined) by various classificatory norms that, while responsive to empirical facts, are not fixed by them.

The second contrast with the anti-essentialism case is the way in which essentialism and views like HPC may be applied to biological taxa. Devitt’s “Intrinsic Biological Essentialism”, for example, is the thesis that “Linnaean taxa have essences that are, at least partly, intrinsic underlying properties” (2008, 346). This could be interpreted either as an empirical claim about the genetic structure of already identified (and identifiable) taxa or as a normative constraint on how taxa should be identified (or, if need be, revised). On either interpretation there is a conflict with our best science — with what it takes to be true or how it goes about systematics. HPC, on the other hand, drops the former empirical claim and offers a far more flexible framework for recognizing “natural kind phenomena”. In my view, treating species as HPC kinds need not be taken as constraint upon scientific practice (cf. Boyd 1999b, 162–163). Ereshefsky’s criticism is that HPC takes the wrong orientation to identifying taxa. While he allows that “there may be many HPC kinds in the world” he seems to deny that *any* biological taxa are among them: “HPC’s emphasis on similarity is at odds with phylogenetic approaches to taxonomy. HPC kinds are fundamentally similarity classes, whereas taxa are fundamentally genealogical entities” (2007, 296). But this ignores the possibility that “genealogical entities” may also have members (or parts, if you insist) that share certain clusters of properties in common due to the activity of certain homeostatic mechanisms. The fact that being an HPC kind requires no genealogical qualifications clearly doesn’t mean that things that *do* meet such qualifications cannot be HPC kinds; a metaphysics of species need not be the sole source of factual claims about species — it had *better* not be if we are to remain in generally naturalistic territory.²⁵

6. Conclusions

²⁵ To be fair to Ereshefsky, it sometimes seems that Boyd is using HPC to criticize the monophyly requirement imposed by some schools of systematics. I do not have the space to evaluate this framing of the dispute; the issues here are complex.

Having now considered these three cases, what shall we say about the plausibility of specific naturalistic constraints on metaphysics? First, I believe that the cases illustrate (what seems quite plausible in any case) that not every apparent demand that issues from science should be accorded equal weight. Not all scientific claims are on an epistemic par; nor are all metaphysical claims. We cannot, then, assume that entire disciplines can be neatly stacked in terms of their epistemic superiority. Perhaps some claims that appear to be metaphysical in nature — assuming that this status can be defined independently of their evidential grounds — are in a position of trumping claims extending from the natural sciences. At the level of conflict between particular claims, science *might well* admit of epistemological rivals even if it is unrivaled as an intellectual/epistemic corpus. We cannot reasonably prioritize the scientific over the metaphysical any more than we may reasonably prioritize beliefs formed by, say, vision over those formed via testimony as justificatorily superior.

Second, in evaluating and resolving apparent conflicts, we need to take into account both the relative epistemic standing of the competing claims and the role that potentially silent auxiliary metaphysical theses might play in generating that competition. Third, and relatedly, we should be cognizant of a problem of unconceived alternatives for scientific metaphysics and draw inferences about the correct metaphysics of species (or what have you) cautiously.

As these conclusions function as partial checks on naturalistic constraint, the story so far is largely negative. Is there anything we can say at this point on the positive side about how we ought to naturalize metaphysics? Are there principles for resolving the sorts of apparent conflicts we've examined between scientific and metaphysical claims? How, in general, can we give specific, defensible, and non-vacuous content to the concept of naturalized metaphysics? I'm not confident that we can. This is not to deny that there are many cases of metaphysical overreach where

naturalistic ideals are flagrantly ignored. But we can often diagnose such cases — as illustrated by essentialism about species — as metaphysics failing to take into account well-established facts that bear on the theses in question. I take it, however, that such input can come from any source. Science is generally good at producing claims that deserve to be taken seriously. Many of these obviously bear on philosophical claims. Thus they should be sought out and acknowledged by philosophers and taken into account as appropriate. It's this last imperative that seems to me difficult to assign precise content. My suggestion that we distinguish between scientific practice, norms of classification (or other aspects of scientific practice), and empirical findings, theories, proposals, working hypotheses (and distinguish further within these categories according to their epistemic standing) may be part of the story, but I do not assume that we can map very precise methodological constraints onto such divisions.²⁶

This story might change somewhat depending on how we understand the project of scientific metaphysics (or metaphysics generally). I argued in the previous section that naturalistic constraint should be at best hesitant in contexts in which pluralism may be expected to hold sway (say, in cases where methodological norms or classificatory practice was largely contingent). Even if a particular piece of scientific practice has developed in a uniform way, if it might have legitimately developed differently (with different metaphysical consequences), then the naturalist cannot reasonably insist on grounds of scientific consensus that the corresponding metaphysics must be this or that other way. But suppose we adopted something like Peter Godfrey-Smith's approach to metaphysics according to which "the theoretical constructs developed in systematic metaphysics are best seen as

²⁶ I read McLeod and Parsons as making a similar claim when, in the finale of their critical response to Maclaurin and Dyke's previously discussed criterion for naturalistic acceptability (2012), they write that "it's not possible to determine which theories are [naturalistically acceptable] just by understanding what those theories say. To determine whether a theory is naturalistic, we have to do some philosophy (and possibly some science too)" (2013, 177–178)

models. Metaphysical system-building is model building” (2006, 6). On this view, concerns about pluralism and contingency drop out. A consensus classificatory practice might be as contingent as you like; we can still model it and discuss its ontology in this deflationary sense without any pretense that we are mirroring the fundamental structure of nature.

Your mileage may vary; maybe this doesn't seem like doing *metaphysics*. Fair enough. Yet perhaps we can maintain something of the spirit of Godfrey-Smith's conception while moving in the direction of (traditional) metaphysics naturalized. Here I am reminded of some remarks of Mark Johnston's at the outset of an important essay on how we might formulate a defensible, non-verificationist Pragmatism; he writes:

Let us say that metaphysics in the pejorative sense is a confused conception of what legitimates our practices; confused because metaphysics in this sense is a series of pictures of the world as containing various independent demands for our practices, when the only real legitimation of those practices consists in showing their worthiness to survive on the testing ground of everyday life. Then metaphysics is not just a technical discourse within philosophy to which, since Kant, a technical apparatus of philosophical criticism has been opposed. It is endemic to our culture. So defined, metaphysics is the proper object of that practical criticism which asks whether the apparently legitimating stories which help sustain our practices really do legitimate, and whether the real explanations of our practices allow us to justify them. There then ought to be a critical philosophy which not only corrals the developed manifestations of metaphysics within philosophy but also serves the ends

of practical criticism. Such a critical philosophy would be the content of anything that deserved the name of a progressive Pragmatism. (1993, 85)

Some versions of naturalism can be characterized as strong reactions against Johnston's "metaphysics in the pejorative sense"; I've argued that such naturalists err in asserting too strong an asymmetry in the opposite direction, "corralling" metaphysics too aggressively. I don't claim that naturalized metaphysics should be seen as an outcropping of pragmatism. Rather I see a progressively naturalized metaphysics as a regulative ideal in which competing demands of practice, norms, features of disciplinary discourse (both scientific and metaphysical), and our best theories (both scientific and metaphysical) are weighed against one another in order to develop the best picture of the world that we can. This pragmatic stance seems superior to a discipline-centered approach in its ability to avoid the problems discussed above while making flexible use of our epistemic resources. If more can be said in this direction, we will just have to evaluate such proposals on their merits.

REFERENCES

- Barker, Matthew J. (2010) "Specious Intrinsicism", *Philosophy of Science* 77:73–91.
- Barker, Matthew J., and Robert A. Wilson (2010) "Cohesion, Gene Flow, and the Nature of Species", *Journal of Philosophy* 107 (2):61–79.
- Barnes, Elizabeth (2010) "Arguments Against Metaphysical Indeterminacy and Vagueness", *Philosophy Compass* 5 (11):953–964.
- Boyd, Richard (1999a) "Homeostasis, Species, and Higher Taxa", in Robert A. Wilson (ed.), *Species: New Interdisciplinary Essays*. Cambridge: MIT Press.
- (1999b) "Kinds, Complexity and Multiple Realization", *Philosophical Studies* 95:67–98.
- (2010) "Homeostasis, Higher Taxa, and Monophyly", *Philosophy of Science* 77 (5):686–701.
- Briggs, D., and S. M. Walters (1997) *Plant Variation and Evolution*. 3rd ed. Cambridge: Cambridge University Press.
- Callender, Craig (2011) "Philosophy of Science and Metaphysics", in Steven French and Juha Saatsi (eds.), *The Continuum Companion to the Philosophy of Science*. London: Continuum International Publishing Group.
- Caplan, Arthur L. (1981) "Back to Class: A Note on the Ontology of Species", *Philosophy of Science* 48:130–140.
- Chakravartty, Anjan (2010) "Metaphysics Between the Sciences and Philosophies of Science", in P.D. Magnus and Jacob Busch (eds.), *New Waves in Philosophy of Science*. Basingstoke: Palgrave Macmillan.

- (2013) "On the Prospects of Naturalized Metaphysics", in Don Ross, James Ladyman and Harold Kincaid (eds.), *Scientific Metaphysics*. Oxford: Oxford University Press.
- Coleman, Keith A., and E. O. Wiley (2001) "On Species Individualism: A New Defense of the Species-as-Individuals Hypothesis", *Philosophy of Science* 68:498–517.
- Coyne, Jerry A., and H. Allen Orr (2004) *Speciation*. Sunderland, MA: Sinauer Associates.
- de Queiroz, Kevin (1999) "The General Lineage Concept of Species and the Defining Properties of the Species Category", in Robert Wilson (ed.), *Species: New Interdisciplinary Essays*. Cambridge: M.I.T. Press.
- Devitt, Michael (2008) "Resurrecting Biological Essentialism", *Philosophy of Science* 75 (3):344–382.
- Dorr, Cian (2010) "Review of Ladyman and Ross, Every Thing Must Go", *Notre Dame Philosophical Reviews*.
- Dyke, Heather, and James Maclaurin (2013) "What Shall We Do With Analytic Metaphysics? A Response to McLeod and Parsons", *Australasian Journal of Philosophy* 91 (1):179–182.
- Ereshefsky, Marc (2007) "Foundational Issues Concerning Taxa and Taxon Names", *Systematic Biology* 56 (2):295–301.
- Ereshefsky, Marc, and Mohan Matthen (2005) "Taxonomy, Polymorphism, and History: An Introduction to Population Structure Theory", *Philosophy of Science* 72 (1):1–21.
- Evans, Gareth (1978) "Can There Be Vague Objects?", *Analysis* 38 (4):208.
- French, Steven, and Décio Krause (2003) "Quantum Vagueness", *Erkenntnis* 59:97–124.
- Ghiselin, Michael (1966) "On Psychologism in the Logic of Taxonomic Controversies", *Systematic Zoology* 26:207–215.
- (1974) "A Radical Solution to the Species Problem", *Systematic Zoology* 23:536–544.
- (1987) "Species Concepts, Individuality, and Objectivity", *Biology and Philosophy* 2 (2):127–143.
- Godfrey-Smith, Peter (2006) "Theories and Models in Metaphysics", *The Harvard Review of Philosophy* XIV:4–19.
- Griffiths, Paul E. (1999) "Squaring the Circle: Natural Kinds with Historical Essences", in Robert A. Wilson (ed.), *Species: New Interdisciplinary Essays*. Cambridge: MIT Press.
- Haber, Matthew H. (2013) "Colonies Are Individuals: Revisiting the Superorganism Revival", in Frédéric Bouchard and Philippe Huneman (eds.), *From Groups to Individuals: Perspectives on Biological Associations and Emerging Individuality*. Cambridge: MIT Press.
- (2015) "The Biological and the Mereological: Metaphysical Implications of the Individuality Thesis", in Alexandre Guay and Thomas Pradeu (eds.), *Individuals Across the Sciences*. New York: Oxford University Press.
- Häggqvist, Sören (2005) "Kinds, Projectibility and Explanation", *Croatian Journal of Philosophy* 5 (13):71–87.
- Hawley, Katherine (1998) "Indeterminism and Indeterminacy", *Analysis* 58 (2):101–106.
- Heck, Richard (1998) "That There Might Be Vague Objects", *The Monist* 81 (2):274–296.
- Hull, David L. (1977) "A Logical Empiricist Looks at Biology", *British Journal for the Philosophy of Science* 28 (2):181–189.
- (1978) "A Matter of Individuality", *Philosophy of Science* 45:335–360.
- (1989) *The Metaphysics of Evolution*. Albany: State University of New York Press.
- (1999) "On the Plurality of Species: Questioning the Party Line", in Robert A. Wilson (ed.), *Species: New Interdisciplinary Essays*. Cambridge: MIT Press, 23–48.
- Humphreys, Paul (2013) "Scientific Ontology and Speculative Ontology", in Don Ross, James Ladyman and Harold Kincaid (eds.), *Scientific Metaphysics*. Oxford: Oxford University Press.
- Johnston, Mark (1993) "Objectivity Refigured: Pragmatism Without Verificationism", in John Haldane and Crispin Wright (eds.), *Reality, Representation, and Projection*. New York: Oxford University Press.
- Kitcher, Philip (1989) "Some Puzzles About Species", in Michael Ruse (ed.), *What the Philosophy of Biology Is*. Dordrecht: Kluwer.
- Kornblith, Hilary (1993) *Inductive Inference and its Natural Ground*. Cambridge: MIT Press.

- Koslicki, Kathrin (2003) "The crooked path from vagueness to four-dimensionalism", *Philosophical Studies* 114 (1–2):107–134.
- Kripke, Saul (1980) *Naming and Necessity*. Cambridge: Harvard University Press.
- Ladyman, James, and Donald Ross (2007) *Every Thing Must Go: Metaphysics Naturalized*. Oxford: Oxford University Press.
- LaPorte, Joseph (2004) *Natural Kinds and Conceptual Change*. Cambridge: Cambridge University Press.
- Lewens, Tim (2012a) "Pheneticism Reconsidered", *Biology and Philosophy* 27:159–177.
- (2012b) "Species, Essence and Explanation", *Studies in the History and Philosophy of Biology and Biomedical Sciences* 43:751–757.
- Lewis, David (1986) *On the Plurality of Worlds*. Oxford: Basil Blackwell.
- (1988) "Vague Identity: Evans Misunderstood", *Analysis* 48:128–130.
- Liebman, David, and Matti Eklund (2007) "Sider on Existence", *Noûs* 41 (3):519–528.
- Lowe, E. J. (1994) "Vague Identity and Quantum Indeterminacy", *Analysis* 54 (2):110–114.
- Maclaurin, James, and Heather Dyke (2012) "What is Analytic Metaphysics For?", *Australasian Journal of Philosophy* 90 (2):291–306.
- Maddy, Penelope (2007) *Second Philosophy*. New York: Oxford University Press.
- Mallet, James (1995) "A Species Definition for the Modern Synthesis", *Trends in Ecology and Evolution* 10 (7):294–299.
- Mayr, Ernst (1959) "Darwin and the Evolutionary Theory in Biology", in B.J. Meggars (ed.), *Evolution and Anthropology: A Centennial Appraisal*. Washington, D.C.: Anthropological Society of Washington.
- (1963) *Animal Species and Evolution*. Cambridge: Harvard University Press.
- McLeod, Mike, and Josh Parsons (2013) "Maclaurin and Dyke on Analytic Metaphysics", *Australasian Journal of Philosophy* 91 (1):173–178.
- Melnyk, Andrew (2013) "Can Metaphysics Be Naturalized? And If So, How?", in Don Ross, James Ladyman and Harold Kincaid (eds.), *Scientific Metaphysics*. Oxford: Oxford University Press.
- Moore, Joseph G. (2008) "A Modal Argument Against Vague Objects", *Philosophers' Imprint* 8 (12):1–17.
- Morganti, Matteo (2013) *Combining Science and Metaphysics*. London: Palgrave–Macmillan.
- Noonan, Harold W. (1995) "E. J. Lowe on Vague Identity and Quantum Indeterminacy", *Analysis* 55 (1):14–19.
- Paul, L.A. (2012) "Metaphysics as Modeling: The Handmaiden's Tale", *Philosophical Studies* 160 (1–29).
- Putnam, Hilary (1975a) "The Meaning of 'Meaning'", reprinted in his *Mind, Language and Reality: Philosophical Papers*, Vol. 2. Cambridge: Cambridge University Press.
- (1975b) "Is Semantics Possible?", reprinted in his *Mind, Language and Reality: Philosophical Papers*, Vol. 2. Cambridge: Cambridge University Press.
- Quine, W.V.O. (1953) "On What There Is", in his (ed.), *From a Logical Point of View*, Cambridge: Harvard University Press.
- (1969) "Natural Kinds", reprinted in his *Ontological Relativity and Other Essays*. London: Columbia University Press.
- (1981) "What Price Bivalence", *The Journal of Philosophy* 78 (2):90–95.
- Reydon, Thomas A. C. (2009) "How to Fix Kind Membership: A Problem for HPC Theory and a Solution", *Philosophy of Science* 76 (5):724–736.
- Richards, Richard A. (2010) *The Species Problem*. New York: Cambridge University Press.
- Rieppel, Olivier (2007) "Species: Kinds of Individuals or Individuals of a Kind", *Cladistics* 23:373–384.
- Ritchie, Jack (2008) *Understanding Naturalism*. Durham: Acumen Publishing.
- Ruse, Michael (1987) "Biological Species: Natural Kinds, Individuals, or What?", *British Journal for the Philosophy of Science* 38:225–242.
- Sider, Theodore (2001) *Four-Dimensionalism*. Oxford: Oxford University Press.
- (2003) "Against Vague Existence", *Philosophical Studies* 114:135–146.

- (2009) "Against Vague and Unnatural Existence: Reply to Liebesman and Eklund", *Noûs* 43 (3):557–567.
- Slater, Matthew H. (2013) *Are Species Real?* London: Palgrave–Macmillan.
- (2015) "Natural Kindness", *The British Journal for the Philosophy of Science* 66 (2):375–411.
- (MS) "Pluto and the Platypus: Tale of an Odd Ball and an Odd Duck", *Unpublished*.
- Sober, Elliott (1980) "Evolution, Population Thinking, and Essentialism", *Philosophy of Science* 47:350–383.
- Sokal, R., and P. Sneath (1961) *Principles of Numerical Taxonomy*. San Francisco: Freeman.
- Stanford, P. Kyle (2006) *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives*. New York: Oxford University Press.
- Sterelny, Kim, and James Maclaurin (2008) *What Is Biodiversity?* Chicago: University of Chicago Press.
- van Inwagen, Peter (1988) "How to Reason About Vague Objects", *Philosophical Topics* 16 (1):255–284.
- van Valen, Leigh (1976) "Ecological Species, Multispecies, and Oaks", *Taxon* 25:233–239 [Reprinted in Ereshefsky (1992)].
- Wheeler, Quentin D., and Rudolf Meier (2000) *Species Concepts and Phylogenetic Theory*. New York: Columbia University Press.
- Wilkerson, T. E. (1995) *Natural Kinds*. Brookfield: Ashgate Publishing Company.
- Wilkins, John S. (2009) *Defining Species: A Sourcebook from Antiquity to Today*. New York: Peter Lang.
- Williams, Neil E. (2011) "Putnam's Traditional Neo-Essentialism", *Philosophical Quarterly* 61 (242):151–170.
- Wilson, Robert A., Matthew J. Barker, and Ingo Brigandt (2007) "When Traditional Essentialism Fails: Biological Natural Kinds", *Philosophical Topics* 35 (1/2):189–215.
- Winsor, Mary P. (2003) "Non-Essentialist Methods in pre-Darwinian Taxonomy", *Biology and Philosophy* 18:387–400.
- (2006) "Creation of the Essentialism Story: An Exercises in Metahistory", *History and Philosophy of the Life Sciences* 28 (2):149–174.