No purely epistemic theory can account for the naturalness of kinds

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Abstract

Several philosophers have recently tried to define natural kinds in epistemic terms only. Given the persistent problems with finding a successful metaphysical theory, these philosophers argue that we would do better to describe natural kinds solely in terms of their epistemic usefulness, such as their role in supporting inductive inferences. In this paper, I argue against these epistemology-only theories of natural kinds and in favor of, at least partly, metaphysical theories. I do so in three steps. In the first section of the paper, I propose two desiderata for a theory of natural kinds. In the second section, I discuss one example of a 'general' epistemology-only theory, proposed by Marc Ereshefsky and Thomas Reydon, and argue that theories like theirs fail to provide adequate criteria of natural kinds. In the third section, I focus on one example of a 'specific' epistemology-only theory, proposed by P.D. Magnus, and use it to show why such theories cannot justify the claim that the proposed epistemic criteria account for the naturalness of kinds.

Introduction

Scientists theorize about kinds like *hydrogen* and *gold*, *electron* and *neutron, Canis Lupus* and *Felis Catus*, *stratus cloud* and *cumulus cloud*, *inflation* and *democracy.* Do any of these terms refer to natural kinds, rather than to merely conventional distinctions, and what exactly constitutes the difference? This is the issue at the heart of the natural kinds debate. In fact, there are at least two different questions at stake here, helpfully distinguished by Bird and Tobin (2016). On the one hand, we can ask about the ontology of kinds. What type of things do we refer to when using natural kind terms? Are they sets, universals*, sui generis* entities, or is their existence perhaps reducible to that of particulars (Bird & Tobin 2016)? On the other hand, we can ask what it is that characterizes the naturalness of kind divisions. By using kind terms, we group together things as members of a single kind. When is such a grouping a natural one? The first question asks about

the reality of the entities that we call 'kinds,' and requires a 'deep' ontological answer. The second question asks about the reality of the kind distinctions we make, and requires a different answer. There is more to be said about the naturalness of kind distinctions than that only natural kind predicates refer to a particular type of entity (Khalidi 2013).

The traditional approach to this second question is to look for metaphysical criteria that allow us to characterize natural kinds, like an inexhaustible number of similarities (Mill 1843), natural laws (Collier 1996), or essences (Kripke 1972). Often these criteria are then also used to explain why natural kinds are epistemically fruitful. Perhaps members of natural kinds are objectively similar, which could explain why they support inductive projections, or perhaps kind membership is determined by causal kind essences, which could also account for their explanatory relevance (Platts 1983; Putnam 1975). A typical metaphysical theory of natural kinds aims to account both for their naturalness and their epistemic fruitfulness.

In the last two decades or so, this metaphysical approach to defining natural kinds has become contested. There are at least two reasons for this. First of all, (anti-reductionist) philosophers of the special sciences have objected to the fact that according to several metaphysical theories there are no natural kinds in the special sciences (Khalidi 2013). While some of these philosophers have responded by proposing more relaxed metaphysical criteria that can accommodate the kinds in the special sciences (Boyd 1999a, 1999b; Craver 2009; Griffiths 1999, Khalidi 2013), others have questioned the fundamental assumption that there is a single set of metaphysical criteria that accounts for the naturalness of kinds in all scientific domains, from fundamental physics to sociology (Häggqvist 2005; Magnus 2012, 2014, 2018).[1]

The second reason is the practical turn in the philosophy of science. Philosophers have become more and more interested in the actual scientific practice of grouping things as members of a kind (Brigandt 2009; Lemeire 2016). Consider, for example, the newly edited volume on *Natural Kinds and Classification in Scientific Practice* (Kendig 2016), which aims to study "activities

[1] Most philosophers implicitly accept this monism about natural kinds, for some examples consider the criteria of Wilkerson (1988) and Ellis (2001).

of kinding across a variety of disciplines" and is explicitly described as an application of the practice turn to the philosophy of natural kinds (Kendig 2016, abstract). Instead of aiming to explain the usefulness of kind categories based on their metaphysical characteristics, the authors in this volume examine the many ways in which kind categories are scientifically constructed and used.

Several of the philosophers who have voiced concerns over the traditional metaphysical approach to defining natural kinds have also advanced a positive program; they aim to characterize natural kinds in epistemic terms only (Ereshefsky & Reydon 2015; Häggqvist 2005; Magnus 2012, 2014; McLeod 2010). That is, these authors aim to define natural kinds without saying anything about the metaphysical nature of kinds. Given the problems with finding a single metaphysical theory that accounts for natural kind categories in all scientific disciplines, and given the epistemic fruitfulness of these kind categories within these disciplines, why not just define natural kinds in epistemic terms directly? Why would we even need a metaphysical theory to account for the difference between natural and conventional divisions? Could we not – to give an example that might prove too simple – say that all and only natural kinds support multiple inductive inferences?

In this paper, I argue against these epistemology-only theories of natural kinds and in favor of (at least partly) metaphysical theories. I will do so in three steps. In the first section of the paper I set up the argument. I explain what a successful theory of natural kinds would have to provide by discussing two traditional metaphysical theories. In the second section, I discuss one 'general' theory of natural kinds, that of Marc Ereshefsky and Thomas Reydon (2015), and defend the claim that such theories fail to give adequate criteria of natural kinds. In the third section, I focus on one 'specific' theory, that of P.D. Magnus (2012, 2014), and argue that his theory, and others like it, cannot explain why exactly satisfying the proposed epistemic criteria makes for natural rather than conventional kind divisions.

1. Desiderata for a theory of natural kinds

Before getting into the reasons why purely epistemic theories of natural kinds fail, let me first spell out what a theory of natural kinds has to provide to be successful, taking care not to introduce desiderata that beg the question against these epistemic theories. Needless to say an epistemology-only theory of natural kinds will not provide a metaphysically grounded explanation of the fact that certain kind categories are epistemically useful whereas others are not, but we should not assume that such an explanation is required for a theory of natural kinds to be successful. Instead, I propose two different desiderata.

Here is a first desideratum. Any theory that aims to account for the naturalness of kind distinctions should provide criteria that allow us to distinguish natural from conventional kinds. This is uncontroversial and widely accepted; all current theories of natural kinds aim to provide these (normative) criteria. Essentialists, for example, hold that natural kinds are characterized by real essences (Ellis 2001; Wilkerson 1988). This essential and intrinsic property, or set of properties, is said to constitute the very nature of the kind, making it metaphysically necessary that anything that has this property is a member of the kind. The standard example is H_2O being the microstructural essence of water (Kripke 1972). Irrespective of how we would want to draw the boundaries between chemical substances, anything that is H_2O is necessarily of the same kind, namely water, and nothing is water without having this property. This essentialist criterion distinguishes natural kinds from conventional divisions and is also used to explain why natural kinds are epistemically so useful (Wilkerson 1988).

Yet if one would apply this essentialist criterion to scientific kinds, only the kinds from the most basic sciences would perhaps qualify as natural ones (Collier 1996). The kinds that are theorized about in many of the special sciences, like biology for example, do not have essences in any strict sense (Ereshefsky 2001; Brigandt 2009; Magnus 2011, 2012). Many philosophers therefore consider this essentialist criterion to be unduly restrictive, since the kind categories in the special sciences do not appear to be merely conventional ways of grouping things, and do

support many of the same kind-based epistemic practices as natural kinds in the basic sciences do.

This disagreement raises a fundamental methodological question: how are the criteria proposed by a theory of natural kinds to be justified and evaluated? In other philosophical debates, the necessary and sufficient conditions that one proposes have to accord at least with some fundamental intuitions or pre-theoretical beliefs about the data set shared among researchers in the field. In the debate about natural kinds, however, there are not really any such shared intuitions that can be used to justify a set of criteria. Some philosophers hold that there are only natural kinds in sciences like physics and chemistry (Collier 1996; Ellis 2001), others are happy to accept that the kinds categorized in biology, geology, sociology, and political science are natural kinds as well (Boyd 1999b; Khalidi 2013). This variation in the intuitions about the data set itself is not hard to explain; the notion of a 'natural kind' is a technical philosophical one. There are no intuitions or pre-theoretical beliefs about which kinds constitute natural ones, only theory-based beliefs. As such, a complete theory of natural kinds should not just propose normative criteria to distinguish natural from conventional kind categories, but should also offer a *theoretical justification* of these criteria. This is the second desideratum I propose for any theory of natural kinds to be successful; that it provides a theoretical account of the naturalness of kinds that explains *why* exactly satisfying the proposed criteria makes for natural rather than conventional categories.

Perhaps an analogy with a different philosophical debate can make the distinction between both desiderata clearer. Theories about *scientific explanation* also aim to propose criteria that an explanation must satisfy in order to be successful. According to the Deductive-Nomological model, for example, an explanation must be a deductive argument based on true premises, one of which is a law of nature, that concludes the explanandum. But even given these criteria, one might still ask why satisfying these criteria would constitute an explanatory relation between explanans and explanandum (Strevens, 2008, 10). A *theoretical account* of the nature of the explanatory relation provides that answer. According to Hempel's expectability account of explanation, for example, an

explanation is successful if it shows that "the occurrence of the phenomenon *was to be expected*" (Hempel 1965, 337). This general account of the explanatory relation allows one to understand why deducing a particular event as an instantiation of a law of nature would make for a good explanation; it shows that the event was to be expected. This theoretical account of the nature of explanation could also serve as a justification of one's proposed criteria, although in the debate on scientific explanation this is to some extent unnecessary. Since philosophers broadly agree which explanations one's criteria should capture, these theories are mostly evaluated by how well they accord with the agreed upon data set. In the debate on natural kinds, however, there is so little agreement on which kinds are the natural ones, that it really is necessary for a successful theory to provide both a set of criteria and a theoretical account of the naturalness of kinds to justify these criteria.

In fact, most philosophers who present a set of criteria for natural kinds also defend or at least assume a more general account of the naturalness of kinds that is meant to justify these criteria. Many essentialists, for example, hold that the naturalness of kinds lies in the fact that kind membership is determined completely *mind-independently*, rather than by how we humans think about, perceive, use, or label individuals (Lowe 2008; Sider 2002; Tahko 2015).[2] Defining the reality of kinds as 'mind-independence' allows essentialists to argue for their view that having an essence is the correct criterion for natural kinds; it is only when kind membership is determined by an essential property that kind membership is completely mind-independent, or so they argue.

Similarly, philosophers who defend more relaxed metaphysical criteria, aiming to include many of the kinds of the special sciences, tend to do so based on a quite different understanding of what it is that constitutes the naturalness of kinds. The most popular version of such an account is Boyd's Homeostatic Property Cluster (HPC) theory, which is widely accepted by philosophers of science (Griffiths 1999; Kornblith 1993). According to Boyd's HPC theory (1988, 1991, 1999a,

[2] For doubts about the notion of mind-independence by an essentialist, see Wilkerson (1988). For a broader discussion of the troubles with spelling out naturalness in terms of mind-independence, by a non-essentialist, see Khalidi (2016).

1999b, 2000), natural kinds can metaphysically be characterized as families (F) of properties that tend to co-occur, and for which this co-occurrence "may be metaphorically (sometimes literally) described as *homeostasis*. Either the presence of some of the properties in F tends (under appropriate conditions) to favor the presence of the others, or there are underlying mechanisms or processes that tend to maintain the presence of the properties in F, or both" (Boyd 1999b, 143). Metaphysically characterized as a causally sustained cluster of properties, Boyd's theory explains how these HPC kinds support inductive inferences and causal explanations even when kind members do not share an essential property, both in the basic and in the "Inexact, Messy and Parochial Sciences" (Boyd 1999b, 151).[3]

Boyd's proposed criteria for natural kinds are more relaxed than those proposed by essentialists since he has a very different view of the naturalness of kinds. According to Boyd, kind membership is never determined completely mind-independently. Although the causal structure of the world (HPC's) exists mind-independently, natural kinds do not. Rather, natural kinds only exist in the interaction of our epistemic practices and scientific disciplines with the causal structure of the world. The epistemic practices and interests of the scientific disciplines co-determine kind membership, making it not completely mind-independent. Yet according to Boyd, this epistemic co-determination of kind membership does not undermine the naturalness of a kind, it ensures it. To achieve epistemic accommodation one *must* defer to the causal structure of the world when categorizing particulars. This is the lesson Boyd draws from Goodman's New Riddle of Induction (Boyd 1980). Naturalness, according to Boyd, lies not in the mind-independent determination of kind membership, but rather in the fact that one has *deferred* to the causal structure of the world in order to optimally accommodate the inductive and explanatory demands of a scientific disciplinary matrix (Boyd 1991). Homeostatic Property Clusters are those causal

[3] There is some debate in the literature on whether Boyd holds the view that *all* natural kinds are HPC's. In my view, this just depends on how broadly one defines the notion of an HPC, that is, whether it also includes kinds that do have necessary and sufficient conditions for kind-membership. In any case, nothing important depends on this for the purposes of this paper.

structures that metaphysically support this accommodation, and are therefore characteristic of natural kinds.

Evaluating these metaphysical theories of natural kinds is beyond the scope of this paper, and there are many good reviews of these theories available already (e.g. Khalidi 2013). The rest of this paper will be focused on one particular response to problems with these metaphysical theories, namely to abandon the metaphysical project altogether and to define natural kinds in purely epistemic terms. If every metaphysical theory of natural kinds also aims to explain why kinds are epistemically fruitful, why could this not be used as the defining criterion of natural kinds? Perhaps this epistemic fruitfulness is the only thing that all natural kinds really have in common, grounded in metaphysically very different things (Häggqvist 2005; Magnus 2014, 2018). I call such theories epistemology-only theories of natural kinds. In the rest of this paper, I will argue that these cannot be successful theories of natural kinds.

The goal of this first section was to introduce the two desiderata that I will use to support this argument. A successful theory of natural kinds should provide both criteria of natural kinds and a theoretical account of naturalness that can justify that these criteria account for the difference between natural and conventional categories. Since there are no shared pre-theoretical beliefs about which kind categories should be considered the natural ones, one cannot just hope to justify one's criteria by arguing that they make the intuitively correct distinction between natural and conventional categories.

Note that nothing so far suggests that a purely epistemic theory could not succeed in satisfying both these desiderata. Importantly, the second desideratum defended here is *not* that a set of criteria for natural kinds has to be justified in terms of a *metaphysical account* of naturalness. Any adequate theoretical justification of these criteria would do. Boyd's account of naturalness as 'deference to the world so as to accommodate our inductive and explanatory practices' says nothing about the specific metaphysical nature of kinds either, although he does propose metaphysical *criteria* of natural kinds of course. Nothing suggests from the outset that it is impossible to provide epistemic criteria of natural kinds *and* to explain why satisfying these

criteria makes for natural rather than conventional kind distinctions, without saying anything about the metaphysical nature of natural kinds.[4] Nevertheless, this is what I will argue for in this paper. In the following section, I start by defending the claim that what I call 'general' epistemology-only theories fail to provide successful criteria, under any plausible account of naturalness.

2. General epistemology-only theories

2.1 Ereshefsky and Reydon

I call epistemic theories of natural kinds 'general' when they provide criteria for natural kinds without specifying which epistemic tasks natural kinds exactly support. Instead, these theories start from the observation that some kind categories are particularly fruitful for scientists and then aim to distinguish natural kinds by characterizing this fruitfulness more generally. One could, for example, say that natural kinds are those kinds that successful science cannot do without (Magnus 2012) or the ones that we would end up with at the imagined end of all scientific inquiry. In this section, I will focus on one general epistemology-only theory, that of Marc Ereshefsky and Thomas Reydon's (2015), and argue that this theory fails to provide adequate criteria. Although I will only discuss their view in detail, the argument generalizes to other similar theories of natural kinds (e.g. Dupré 2002).

Ereshefsky and Reydon (2015) object to Boyd's HPC theory, claiming that it is not naturalistic enough. They do so by presenting several cases where there is a mismatch between HPC theory

[4] In this paper I will only discuss *realist* epistemic theories of natural kinds. There are some other epistemic theories of natural kinds that are explicit about not conceiving of 'naturalness' as a realist notion anymore (Franklin-Hall, 2015). These theories abandon the idea that the naturalness of kinds lies somehow in getting something right about the world – which conventional categories do not – but rather conceive of the 'naturalness' of a category as a particular role this category can have in a scientific theory or discipline. McLeod (2010) defends a theory like this, according to which natural kinds allow for many generalizations (criterion), explaining their unitary role for scientific fields (naturalness). If having a unitary role for science is not understood as a 'sign' of naturalness anymore, but as naturalness itself, I don't see why we should still call this *naturalness*. Hence in this paper I only discuss those epistemic theories that explicitly aim to provide a realist but epistemic answer to the problem of natural kinds.

and successful scientific kind categories. Confronted with this mismatch, they ask "[w]ho is right: HPC Theorists or the scientists? We side with the scientists" (2015, 970). In other words, the data set of a thoroughly naturalistic account of natural kinds should not be determined by the theoretical beliefs of philosophers of science, but by the success of science itself.

Let us look at one of their examples of this mismatch; non-causal scientific kinds. These are scientific kinds that are not defined in terms of an underlying mechanism or in terms of causal properties. Although "philosophers are keen on science revealing the causal structure of the world, such enthusiasm for causality is far from universal among scientists" (2015, 974). Consider, they say, the classification of prokaryotic species (Archaea and Bacteria) in microbiology. The problems with defining what species are in eukaryotic taxonomy are well-known among philosophers of biology, but the problems are even bigger for prokaryotes. Several features of prokaryotes, like the fact that they exchange genes horizontally, make the traditional theory-based species concepts ill-suited for use in prokaryotic taxonomy (Ereshefsky 2010). Although some have proposed species concepts that have a clear counterpart in eukaryotic taxonomy, like the recombination species concept (Dykuizen & Green 1991), the ecological species concept (Cohan, 2002), and the phylo-genetic species concept (Stackebrandt 2006), the most popular prokaryotic species concept does not have such a counterpart. This is the *phylo-phenetic* species concept (Rosselló-Mora & Amann 2001).

According to the phylo-phenetic species concept, a prokaryotic "species is `a monophyletic and genomically coherent cluster of individual organisms that shows a high degree of overall similarity with respect to many independent characteristics, and is diagnosable by a discriminative phenotypic property'" (Rosselló-Mora & Amann 2001, 59). This concept results in a 'polyphasic' approach to delineating species, using as many as possible measures of similarity to describe phenotypic and genomic clusters.[5] Generally, one first constructs a phylogeny by

[5] What is described here is one method for species delimitation based on the PPSC. This is not to say, however, that different (ontological) species concepts will always result in incompatible (or even different) ways of delimiting species. See Wiens (2007).

analyzing the divergence of 16S rRNA sequences. A 3% divergence rate is the threshold for delineating a species, although it is also recognized that this RNA molecule is not really informative enough to resolve relations at the level of species (Cohan 2002). Hence in addition to this sequence analysis, DNA-DNA hybridization can be used to measure overall genomic similarity. Here 70% of genomic similarity is the threshold for defining species (Coenye et al. 2005). Both thresholds are not set for theoretical reasons but because it has been shown that genomic similarity above these thresholds correlates well with the results of phenotypic clustering methods (Rosselló-Mora & Amann 2001, 53). The third phase of delineating a prokaryotic species is then to describe as many phenotypic traits as possible that can be used for phenotypic clustering methods and for determining a particular diagnostic trait of the species. According to the PPSC, the combination of these three approaches is the best way to delineate prokaryotic species, understood as a monophyletic group of prokaryotes that is genomically coherent and phenotypically similar.

This phylo-phenetic species concept is currently "the most popular approach to species among microbiologists" (Ereshefsky & Reydon 2015, 974). Its defenders rightfully claim that it is "universally applicable", it is "operational", and it results in "a rather stable, objective and predictable classification system" (Rosselló-Mora & Amann 2001, 59). Nevertheless, the aim of the PPSC is to delineate phenotypic and genomic clusters, not to divide species based on whatever mechanisms cause this clustering. Ereshefsky and Reydon point out that "the PPSC is not intended to capture the causal underpinnings of microbial species, and HPC Theory's emphasis on causality is simply irrelevant for these microbiologists" (2015, 974). Furthermore, the goal of the PPSC is not just to find clusters that support predictions but also to have a stable, operational, and universally applicable classification system. "The root of the problem is that HPC Theory assumes that all scientific classification should capture similarity clusters that can support inferential and explanatory practices. This assumption fails to acknowledge that classification in science can have different aims" (2015, 977-978). Thus, Boyd's HPC theory is said to be flawed. A naturalistic theory of natural kinds should not dismiss the most popular and successful approach to species

taxonomy in microbiology based on what Ereshefsky and Reydon call *a priori* philosophical criteria for natural kinds, like tracing the causal structure of the world or supporting inductive inferences and causal explanations.

Before I respond to this argument, I will briefly lay out Ereshefsky and Reydon's alternative theory of natural kinds. Their aim is to be more naturalistic than Boyd, yet also to present normative criteria that allow us to determine which kinds are the natural ones. The general strategy is to find criteria that determine when scientific kind classifications are 'scientifically successful,' although they do not use this phrase themselves. Inspired by Laudan's normative naturalism and his distinction between cognitive values and methodological rules (Laudan 1987, 1990), Ereshefsky and Reydon introduce a similar distinction between the motivating principles and sorting principles of a classificatory program. A classificatory program results in scientific categories by using sorting principles that are justified by motivating principles.

> Consider the Biological Species Concept […]. Its sorting principles tell us to sort organisms of populations that interbreed into the same species, to sort organisms of populations that do not interbreed into different species, and to sort organisms that reproduce asexually into no species. The motivating principle for the Biological Species Concept is the hypothesis that interbreeding and the existence of relatively closed gene pools cause the existence of stable and distinct evolutionary groups of organisms. (Ereshefsky & Reydon 2015, 979)

With these two notions in place, Ereshefsky and Reydon propose three criteria to determine whether the categories resulting from a classificatory program are natural kinds. First of all, "the sorting principles […] should promote the program's motivating principles" (2015, 980). The motivating principle of the PPSC in microbiology, for example, is to "produce stable and identifiable taxonomic units" (2015, 980), and the polyphasic sorting principles of the PPSC provide such categories. This is called the criterion of *internal coherence*.

The second criterion is that of *empirical testability*. Either the motivating principles or the sorting principles have to be empirically testable. For example, "when microbiologists sort organisms according to a particular genetic marker, they should be able to test whether the information they have gathered is about that genetic marker" (2015, 981). Although Ereshefsky

and Reydon do not provide a theoretical account of naturalness that justifies all three of their criteria, they do provide a justification of this second criterion which shows that they still have a realist notion of naturalness in mind. After all, "[t]his criterion elaborates the longstanding view that natural kinds are at least to some extent grounded in nature—that natural kinds are kinds in the empirical world that are to some degree independent of our classificatory practices (a view that is also embodied in Boyd's accommodation thesis)" (2015, 981).

Finally, a classificatory program should also be progressive rather than degenerative: "a classificatory program is progressive if it provides principles that produce additional classifications or extend existing classifications (relative to competing classificatory programs) and those classifications are empirically successful" (2015, 982). The example of a degenerative classification program they give is the Morphological Species Concept. After all, the Biological Species Concept has proven better at detecting evolutionary units than a classification based on morphological features (2015, 982).

When a classificatory program satisfies all three criteria – internal coherence, empirical testability, and progressiveness – the resulting kind categories should be considered natural kinds. This is what I call a *general epistemology-only* theory of natural kinds, since these criteria do not refer to the specific epistemic operations that natural kinds support, but rather aim to capture their scientific success more generally. In the following section I will argue that general epistemic theories cannot provide successful criteria of natural kinds.

2.2 Scientifically successful kinds, but not natural kinds

An epistemology-only theory of natural kinds that aims to characterize natural kinds without specifying the particular epistemic practices they support, cannot result in adequate criteria for natural kinds. The reason is that kind categories can contribute to the success of science on other grounds than their naturalness, on any plausible theoretical understanding of that notion.

Consider, for example, that many scientific disciplines require kinds to be defined in such a way that they allow for reliable diagnoses of kind members. Especially since the third version of

the Diagnostic and Statistical Manual of Mental Disorders, for instance, much attention has been paid to the fact that psychiatric patients should be classified in the same way by different diagnosticians using the same manual. One way to improve this so-called *inter-rater* reliability is by operationalizing the criteria of psychiatric disorders more strictly. It is, for instance, better to replace a vague criterion like 'the patient has experienced the symptoms for a long time,' with the more specific criterion that 'the patient has experienced the symptoms for over six months.' While this specification might make the description of this label more reliable and hence scientifically more useful, it would not *thereby* make the distinction more valid or natural. *Whatever* specification of the number of months one would have chosen, it would have increased the reliability. Hence reliability is a prerequisite for validity but not a type of validity itself. There is no plausible theoretical account of naturalness according to which a distinction is more natural simply because it is more reliable, since reliability is often increased by completely arbitrary decisions.

This example is meant to get the point across that scientific kind categories can be useful on other grounds than their naturalness. A psychiatric classification system that receives higher inter-rater reliability scores than any other system could thereby satisfy all three of Ereshefsky and Reydon's criteria, and yet it would be wrong to consider the resulting categories natural kinds only based on that fact. Surely the fact that the reliability scores of various classification systems are empirically testable does not automatically count towards the naturalness of the categories, given that these scores are dependent on arbitrary specifications of the diagnostic criteria. Perhaps, however, the example of the DSM is not the best one to use against Ereshefsky and Reydon, since many would argue that the current DSM-5 is not an internally coherent classification system anyway. The goal of the new DSM was to provide a psychiatric classification system that is both reliable and valid, but on both scores there is still much room for improvement (Lemeire 2014; Regier et al. 2013).

Ereshefsky and Reydon would definitely agree, however, that the phylo-phenetic species concept meets their three criteria and yet the same point applies there. Recall that this species

concept is not aimed at capturing the causal structure of the world but rather at referring to predictive clusters that are also stable, universally applicable, and readily identifiable. These practical advantages are also what microbiologists consider the strength of the PPSC: "[a]t the present, many, if not all, prokaryote taxonomists agree that the current species circumscription, although not perfect, is acceptable and pragmatic, and covers the primary goals of taxonomy such as a rapid and reliable identification of strains" (Rosselló-Mora & Amann 2001, 40). Notice, however, that even its proponents call the PPSC a *pragmatic* species concept. That is because they realize that a species concept that is stable, universally applicable and that allows for rapid and reliable identification is practical, but that it does not *thereby* result in the delineation of natural kinds. Other microbiologists are even more pronounced: "[s]trictly speaking, this approach provides an arbitrary and anthropocentric definition of prokaryotic species" (Zhi et al. 2012, 27). And: "[a]lthough this practical approach to the prokaryotic species definition has been the subject of considerable debate and prokaryotic taxonomists have been urged to adopt a more natural species concept […], it has been found acceptable by many. Nevertheless, a more natural species concept would be welcomed […]" (Coenye et al. 2005, 160).

There is a mismatch here between the criteria of natural kinds proposed by Ereshefsky and Reydon and the consensus view among actual scientists on the naturalness of PPSC species. Thus their proposed criteria fail even by Ereshefsky and Reydon's own naturalistic standard. More fundamentally, however, they seem to have provided an interesting answer to the wrong question. If the question were to be which classificatory programs are legitimate and should be pursued or funded, their criteria might very well help to make such decisions.[6] But that is a very different question from providing criteria to distinguish natural from conventional kind categories.

In fact, any general epistemology-only theory will fail to provide adequate criteria to distinguish natural kinds, on any plausible theoretical account of naturalness. Although I have only

[6] In fact, their criteria seem to originate from an earlier text of Ereshefsky where he asks exactly this question (Ereshefsky, 2001). Also telling for the fact that the proposed criteria are an awkward fit for a theory of natural kinds, is that Ereshefsky himself elsewhere calls the phylo-phenetic species concept 'nominalistic' (Ereshefsky, 2010).

discussed Ereshefsky and Reydon's theory in detail, the argument generalizes to other similar theories (e.g. Dupré 2002). There are many aspects of a kind category that co-determine its contribution to scientific success but do not depend on the category being naturally defined. Perhaps the PPSC really is the most useful species concept that can be expected for prokaryote taxonomy, for example, but this fact alone should not make us think that these are natural divisions between prokaryotic species. Being universally applicable, stable, and allowing for rapid and reliable identification are virtues of a classification system that do not depend (solely) on the divisions being natural. I cannot imagine how a *general* epistemology-only theory could provide criteria that include only those types of scientific usefulness that really do require natural categories, which is why I believe such theories are doomed to fail. There is, however, a simple solution to this problem, namely to propose criteria that specify the *epistemic* tasks that require kinds to be naturally defined. These are what I call 'specific' epistemology-only theories of natural kinds, which are discussed in the next section.

3. Specific epistemology-only theories

3.1. P.D. Magnus

Recall that, according to Boyd, natural kinds are, metaphysically speaking, Homeostatic Property Clusters. What makes kind categories *natural*, however, is that we defer to these HPC's when categorizing so as to accommodate the epistemic demands of a scientific discipline. Boyd is specific as well; only the inductive and explanatory demands of a scientific discipline require such deference to the causal structure of the world. Perhaps there are other more practical demands that might influence categorization practices, but these do not make kind categories more natural.

> [I]n defining a kind we should be required to defer to the world just in case and to the extent that reference to the kind in question is to be part of an inductive or explanatory project. In cases in which our concerns are largely with the establishment of workable conventions for non-inductive practice, deference to the world should be largely unnecessary. […] It likewise follows that there should be kinds and categories whose definitions combine naturalistic and conventional features in quite complex ways. (Boyd 1991, 140)

Although Boyd himself provides metaphysical criteria for natural kinds based on his view that inductive and explanatory projects require us to defer to the HPC structure of the world, he also seems to point the way toward a *specific* epistemology-only theory. Given that, as I have aimed to argue, a general epistemology-only theory cannot provide adequate criteria of natural kinds, maybe one should aim to be more specific about which epistemic tasks do require natural kinds. Boyd points the way for such an approach when he says that only inductive and explanatory practices require us to defer our categories to the world. This 'defer-to-accommodate' theory of naturalness seems well-suited for a purely epistemic theory of natural kinds as well. Whereas spelling out the naturalness of kinds in terms of mind-independence invites (essentialist) metaphysical criteria, Boyd's 'deference' theory of naturalness suggests the possibility of purely epistemic criteria as well.

Philosophers who defend a specific epistemology-only theory of natural kinds take up this suggestion. I will only discuss P.D. Magnus' theory in detail, but like before, the counter-argument is meant to generalize to other similar theories (e.g. Häggqvist 2005). Sometimes, Magnus presents his view on natural kinds in a general way, stating that "natural kinds are the categories which are indispensable to successful science for some domain of enquiry" (Magnus 2014, 474). This view combines a restriction clause and a success clause, since natural kinds are said to be those that are *indispensable* for scientific *success*.[7] Stated in this general way, however, this view suffers from the same problem that confronts the proposal of Ereshefsky and Reydon. Some kinds can be indispensable for success even though this is (partly) because of conventional factors, like the fact that they allow for reliable diagnosis. Magnus' restriction clause does not solve this problem either, since it might very well be that these conventional factors are part of what makes a kind category so indispensable. Recall that the PPSC is so popular, and perhaps indispensable, exactly because of its many practical virtues. When adequately specified, however, Magnus' view looks more promising:

[7] For the details on this restriction clause, see Magnus (2012).

> A category *k* is *a natural kind for domain d* if (1) *k* is part of a taxonomy that allows the scientific enquiry into *d* to achieve inductive and explanatory success, and (2) any alternative taxonomy that excluded *k* would not do so. (Magnus 2012, 48)

This formulation retains the success (1) and restriction (2) clause, but now specifies that *only* inductive and explanatory success are what counts for the naturalness of kinds, yet still says nothing about the metaphysical nature of kinds. The specification of the epistemic criteria solves the problem of the previous section. It does seem plausible, moreover, to think that a category would not support our inductive and explanatory practices if it were not somehow in accordance with the world. Magnus is explicit about these criteria being part of a realist theory of natural kinds: "so the distinction between natural and practical kinds breaks down. Natural kinds support inductive and explanatory success in a domain. Insofar as we care about the domain, acknowledging natural kinds will be useful. This makes them no less real. […]. The pragmatic naturalism that I articulated in the previous section maintains that natural kinds, though practical, are real." (Magnus 2012, 103, 120).

Perhaps the application of these criteria could result in the delineation of exactly those categories that several philosophers of science would agree are the natural kinds. In fact, I do not aim to argue that these criteria are mistaken. In the next section, I will argue however that there is no way to justify these epistemic criteria in terms of a more general theory of naturalness, without also saying something about the metaphysical nature of kinds.

3.2. Epistemic criteria of natural kinds cannot be justified

As explained in the first section of this paper, a theory of natural kinds should provide both criteria that distinguish natural from conventional categories *and* a theoretical account of naturalness to justify these criteria. After all, there is as much debate about the criteria of natural kinds as there is about which kind categories should be accounted for. Without a theoretical justification of one's epistemic criteria, there is no way to answer those philosophers with essentialist inclinations who agree that there are many epistemically useful categories in the special sciences, but just do not see why that should make us think of them as *natural* categories

rather than just useful conventions. Magnus does provide a theoretical account of the naturalness of kinds, but it fails to justify his proposed epistemic criteria.

According to Magnus, the naturalness of kind divisions is a matter of the identification of these kinds being *constrained* by the world. His ideas is "that the world condemns a great many taxonomies to failure. Constraint from the world is what makes identifying natural kinds the discovery of structure in the world, rather than merely the imposition of a set of labels onto things that are undifferentiated in nature. Acknowledging this, we can say that a kind is natural to the degree that the world penalizes enquiry conducted using taxonomies that do not acknowledge the kind" (Magnus 2012, 50). For a kind to be natural it cannot be the case that we had a choice whether to identify the kind or not, the world has to make the choice for us, so to speak. Although this notion of naturalness speaks of 'the world' penalizing certain taxonomies – it is after all a realist view – it is an epistemology-only theory of natural kinds since it says nothing about the specific metaphysical nature of kinds.

This 'constraint' theory of the naturalness of kinds should explain why satisfying Magnus' proposed epistemic criteria makes for natural rather than conventional kind categories. At first sight, this seems to be the case. If a category is part of a taxonomy that provides inductive and explanatory success and any alternative taxonomy would not do so, then the identification of that category does appear to be constrained by the world.

And yet, this 'constraint' account of naturalness fails to explain why exactly *inductive and explanatory* indispensability are required for a kind to be natural, and hence fails to justify these specific epistemic criteria. Indispensability for inductive and explanatory success suggests that the world has constrained our categorization, but as I will argue, indispensability for other types of *practical* success that is not reducible to epistemic success results in categories being constrained by the world in much the same way. Hence this 'constraint' theory of naturalness would equally apply to these other types of practical success as well. Yet indispensability for practical success does not constitute a good criterion to distinguish natural from conventional kinds. Therefore, a 'constraint' account of naturalness cannot explain why specifically inductive

and explanatory success would distinguish natural from conventional kinds. Although these more specific epistemic criteria do seem to fare better as criteria of natural kinds than general epistemic ones, a 'constraint' account of naturalness cannot explain *why* that is the case.

Before I turn to some real-world examples that support this argument, consider the following adapted Donnellanian (1983) thought experiment that makes the point very well:

> Assume that we were to make contact with extraterrestrial life, and the result of that contact was our initiation into a larger galactic community like those portrayed on *Star Trek* or *Babylon 5.* Fortunately for us, it turns out that all planets in the community use a gold-based currency, just like we do on Earth. What we call gold, however, is too common to be the galactic basis of a currency, but a particular isotope of the element at atomic number 79 is less common, and only that isotope is considered to be gold in the larger galaxy. Furthermore, this distinction is not an arbitrary one. Only that isotope of gold is stable over the long term in the energy fields of some planets, so it is the only kind of gold that can serve as an intergalactic standard. (Zachar 2002, 220)

In this scenario, the identification of 'gold' as one particular isotope with atomic number 79 is strongly constrained by the world. Not because it would provide some indispensable inductive or explanatory success, but because it is the only isotope that is both not too common and is sufficiently stable across intergalactic energy fields. To achieve practical success in this scenario, that is, to select a useful intergalactic gold-based currency, one cannot arbitrarily choose just any isotope of atomic number 79 one pleases. Given that it has to be sufficiently rare and stable, only one particular isotope will do and it is indispensable that one distinguishes between this isotope of atomic number 79 and other isotopes. And yet, I do not think any plausible theory of natural kinds would consider such an identification of 'gold' *natural* simply because we were constrained by the world and could not have identified a different category to achieve the same *practical* success.[8] It is still conventional to identify 'gold' as one particular isotope simply because that is the only one stable enough to feature as an intergalactic gold-based currency.

[8] Practical success here is meant to refer to that type of success *that is not reducible to epistemic success*. Thus, one particular isotope could perhaps be a natural kind for chemists or physicists based on epistemic success. Furthermore, it could also be a natural kind for economists based on

Although fictional this example is not all too different from one that was mentioned before, namely the PPSC species concept in prokaryotic taxonomy. Recall that microbiologists are unanimous in their view that this species concept provides a useful tool given the many issues with other theory-based species concepts, but also agree that it is not a natural species concept. This contradicts Magnus' constraint theory of naturalness, since in order to achieve species descriptions that are practically useful, as the PPSC concept does, these descriptions are strongly constrained by the world as well. Like the fictional example above, one aspect of this practical usefulness of a species description includes the *stability* of the properties that feature in the description. Although there are many properties of prokaryotes that could and have been studied to delineate different species, current efforts largely focus on information provided by DNA and RNA molecules since these are stable enough in varying environments. After all, unlike "other cell constituents used for chemotaxonomy, only the amounts and not the composition of RNA and chromosomal DNA are affected by growth conditions, and are thus independent of environmental changes" (Rosselló-Mora & Amann 2001, 43). Similarly, the particular usefulness of the 16S rRNA molecule for species delineation is not only due to the amount of phylogenetic information this specific molecule contains, but also because of:

> […] its relative ease of sequence alignment. Alignment is the first critical step of sequence-based phylogenetic analyses. Given that positions with a common ancestry have to be compared for reliable phylogenetic conclusions, homologous position have to be arranged in common columns in correct alignment. All rRNA molecules share a common secondary and higher-order structure. (Rosselló-Mora & Amann 2001, 46)

This structural feature of rRNA molecules makes them particularly useful when aiming to delineate species based on a phylogenetic analysis. Just like the fact that the stability of a species description requires the use of DNA and RNA molecules, so does a phylogenetic analysis require

epistemic considerations. However, the fact that it is practically useful as a currency is the result of it being sufficiently rare and stable, and hence not due to its epistemic credentials. One cannot think of this isotope as a natural kind simply based on the fact that it is sufficiently rare and stable. Practical success in this non-epistemic sense does not make for natural kinds.

molecules that can easily be arranged so that homologous positions correspond. In the same way that a domain of inquiry, like that of microbiology in this instance, can constrain researchers when they aim to identify inductively and explanatorily useful kinds, it can also constrain researchers when they aim to identify kinds with various practical virtues. This point generalizes. Just as one is constrained by the world when kind categories are indispensable for inductive and explanatory success in a particular domain, one is often also constrained by the world when kind categories are indispensable for certain types of practical success. And yet, this constraint by the world only makes for 'pragmatic' rather than natural kinds.

Thus, 'being constrained by the world' cannot really be what constitutes the naturalness of kinds. Even if one would agree that only inductive and explanatory success constitute good epistemic criteria to distinguish natural from conventional kinds, a 'constraint' theory of naturalness cannot explain *why* that is the case. If someone is not convinced by these specific epistemic criteria, as many philosophers with essentialist inclinations would be, one cannot provide a justification of these criteria either. Perhaps indispensability for inductive and explanatory success requires us to be constrained by the world, but so does practical success and that does not seem to make for natural kinds.

Recall, however, that Magnus' constraint notion of naturalness is very similar to that of Boyd, who also said that "in defining a kind we should be required to defer to the world just in case and to the extent that reference to the kind in question is to be part of an inductive or explanatory project"(cfr. supra). With his 'defer-to-accommodate' notion of naturalness, Boyd pointed the way for a purely epistemic theory. He pointed the wrong way, however, since this statement is false. It is not only inductive and explanatory projects that require us to defer our categories to the world. If one aims for a species concept in microbiology that is also practically useful, one has to defer to the world when deciding which properties to use for species delineation just as much.

Does this mean that theoretical accounts of the naturalness of kinds as being 'constrained by the world' or as 'deferring to the world' are doomed to fail? Not necessarily, but they can only work if in addition one also provides a metaphysical picture of natural kinds. After all, what

matters for the naturalness of kinds, I propose, is not just *that* or *how* the world constrains our categorization, but also *what* it is in the world that constrains us. That is to say, it is only when our categories are constrained by those aspects of the world that actually matter for the kindhood of individuals, that this constraint also results in natural rather than conventional *kind* categories. Whereas deferring to the world so as to trace causal similarities shared by individuals in the world could result in natural kinds, deferring to the stability of properties does not. What distinguishes natural kind categories from conventional categories is not just the way in which we relate to the world when categorizing, but also whether this relation results in us tracing those aspects of the world that make individuals belong to the same kind. Specific epistemic criteria of natural kinds cannot be *justified* without also saying something about the nature of kinds.

According to Boyd's HPC proposal, for example, kinds are Homeostatic Properties Clusters, and so it is only when we defer our categorization to optimally trace HPC's that our categories refer to natural kinds. Hence Boyd's metaphysical account of kinds does provide him with the theoretical resources to justify his use of epistemic success as a criterion of natural kinds; only inductive and explanatory projects would require us to defer *to the HPC structure* of the world. Practical success does not result in natural kinds, that is, because even when it would require us to defer to the world as well, it does not require us to defer to HPC's in the world, and these are what constitute the nature of kinds.

This also means that if Magnus' specific epistemology-only criteria would turn out to be successful in distinguishing natural from conventional kinds, it would be because inductive and explanatory success are constrained by just those aspects of the world that matter for kindhood. Without a metaphysical theory of kinds, however, Magnus cannot possibly offer an explanation of why it is that inductive and explanatory success characterizes natural kinds. To explain this fact, and to justify his use of these particular epistemic criteria, Magnus needs to provide at least some notion of the metaphysics of kinds. This point generalizes to other similar specific epistemology-only theories of natural kinds (e.g. Häggqvist 2005).

Note that I have not argued that a *single* metaphysical theory of natural kinds is possible or required. Perhaps the metaphysical nature of kindhood differs sufficiently in different domains so that we would need multiple metaphysical theories. But even if there is no single metaphysical picture that applies to all natural kinds, that does not mean one can, as Magnus suggests (2014, 2018), first define the naturalness of kinds purely in epistemic terms and leave the questions about the particular metaphysical grounds of the epistemic criteria for later. Without a metaphysical theory of kindhood (for a particular domain) in place, there would be no reason to think that only epistemic success, rather than practical success, is constrained by those aspects of the world that determine the kindhood of individuals.

Conclusion

Given the recurring worries that traditional metaphysical theories of natural kinds are too restrictive to understand the naturalness of kinds in the special sciences, purely epistemic theories of natural kinds are likely to become even more popular in the coming years. As more attention is being paid to the uniqueness of the special sciences and to the epistemic uses of kind categories, the move away from metaphysical theories of natural kinds will become even more tempting. Nevertheless, I have argued that this purely epistemological approach to the (realist) problem of natural kinds is fundamentally misguided.

The epistemic approach rests on the assumption that for kind categories to be epistemically useful, either in a general or in a more specific way, these categories have to get something right about the world. Given this fact, could we not just define natural kinds based on their epistemic fruitfulness? Do we really need to specify what it is we get right about the world if we want to account for the naturalness of kind categories?

I have argued that we do. First of all, not all of the ways in which kind categories can contribute to scientific success require us to adjust our categories to the world. Hence 'general' epistemology-only theories will fail to provide adequate criteria. Secondly, even if one would specify that only inductive and explanatory success distinguish natural from conventional kinds,

this specification would remain unjustified if one could not explain why exactly this type of success makes for natural kinds rather than useful conventions. I have argued that even if this epistemic success requires us to be constrained by or defer to the world, this is also the case for various types of practical success, and yet those do not result in natural kinds. To account for the naturalness of kinds, it is necessary that one also specifies which metaphysical features contribute to the kindhood of individuals, and should therefore be traced by our categorization practices.

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