Natural Kind Realism, Taxonomic Pluralism, and Relative Fundamentality

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Abstract

I argue that in order to resolve the debate between taxonomic monism and taxonomic pluralism, we should construe the reality of natural kinds in terms of *relative fundamentality*: a natural kind is real if and only if it is *more fundamental than* its members, whose reality is taken for granted in the context of this debate. Taxonomic monism upholds that there is only one correct way of classifying entities into natural kinds within a given scientific domain; taxonomic pluralism maintains that there are different but *equally* correct ways of classifying entities into natural kinds within a given scientific domain, and these different ways *disagree* with each other. The monism/pluralism debate has reached a stalemate, as the two positions conceive the reality of natural kinds in different ways. While taxonomic monists characterize the distinction between natural and non-natural kinds in *metaphysical* terms, taxonomic pluralists approach this distinction in *epistemic*, *viz*., *naturalist*, terms. According to this approach, natural kinds are groupings that underwrite successful epistemic practices in the sciences, such as explanation and induction. As I argue in this dissertation, however, this naturalist approach alone does not secure taxonomic pluralism. First, it fails to provide the requisite *realist* commitment desired by taxonomic pluralists; second, it fails to ensure that the different ways of classifying entities in a given domain into natural kinds genuinely *disagree* with each other, as proclaimed by pluralists. In the face of these two problems, I argue that taxonomic pluralists should give up their anti-metaphysical stance and formulate the reality of natural kinds in terms of *relative fundamentality*. This formulation reorients the monism/pluralism debate so that different classifications of entities into natural kinds can be ranked by their degree of relative fundamentality, according to which

membership in a kind posited by a more fundamental classification would *account for* membership in a kind posited by a less fundamental classification.

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Introduction

Taxonomic pluralism is the view that there are multiple correct ways to classify individual entities within a given scientific domain. For instance, a taxonomic pluralist may maintain that, although evolution is central to biological study, a phenetic taxonomy, which classifies living organisms according to their morphological similarities, is as correct as a phylogenetic taxonomy, which groups organisms into species according to their common ancestry. Likewise, a taxonomic pluralist may insist that classifying chemical nuclides according to their mass number is no less correct than classifying them according to their atomic number, despite the fact that the latter classification forms the periodic table.

I challenge this view in this dissertation. Yet, I do not follow in the footsteps of the traditional taxonomic monists. Traditionally, taxonomic monists argue that the world comes with a *natural-kind* structure to which *the* correct classification must correspond. And they formulate this natural-kind structure by appealing to various metaphysical principles. An example of these metaphysical principles is essentialism, which upholds that every entity at issue possesses a set of intrinsic properties that are both necessary and sufficient for its identity. If essentialism is correct, a monist conclusion is imminent: *the* correct classification should be the one that captures the real essences of things that constitute the natural-kind structure of the world, instead of the nominal essences we conventionally attribute to things.

On the contrary, in challenging taxonomic pluralism, I espouse the naturalist stance endorsed by its proponents. The naturalist stance prioritizes empirical findings over metaphysical considerations in thinking about natural kinds. Accordingly, natural kinds are understood as groupings that underwrite successful scientific practices such as explanation, induction, and prediction. Taxonomic pluralists argue that scientists' diverse epistemic concerns in different investigative contexts are likely to call for different ways of classifying the individual entities in a given domain. Emphasizing the epistemic roles of natural kinds in scientific investigations, taxonomic pluralists further indicate that, since those metaphysical principles employed by their monist opponents fail to concur with empirical evidence and scientific theories, we should reject taxonomic monism. For example, the aforementioned doctrine of essentialism, when being applied to the classification of living organisms, is at odds with evolutionary theory. If we postulate that each individual organism possesses a set of intrinsic properties essential to its identity and that this set of properties also determines what species a given organism belongs to, then it seems that we also have to accept that species have essences. Yet, according to evolutionary theory, species evolve, i.e., their members do not possess a set of unchanging intrinsic properties.

Of course, one may reject taxonomic pluralism by dismissing the scientific categories that do not abide by the metaphysical principles in question, or by revising these metaphysical principles. For example, one may argue that species are not natural kinds, since they do not possess unchanging essences. Or one may argue that we should adopt a broader notion of essence that is not confined to an entity's intrinsic properties, but includes also its historical origin. Nevertheless, in this dissertation, I adopt none of these monist strategies in formulating my critiques against taxonomic pluralism. For my goal in this dissertation is not to vindicate taxonomic monism. Rather, in embracing the naturalist stance towards natural kinds, I identify three different commitments that are necessary for taxonomic pluralism, and question whether they can be adequately fulfilled by the naturalist stance alone. In exposing the inadequacy of the naturalist stance, I attempt to demonstrate that any additional presuppositions that are required to fulfill these three commitments do not always favor a pluralist conclusion.

Since the critiques of taxonomic pluralism I am going to present all revolve around the naturalist stance towards *natural kinds*, it is important for me to first explain why I choose to employ the term "natural kinds". Many taxonomic pluralists in the current literature advocate that we should give up the term "natural kinds". This is not just because the term is obscure, as different philosophers have different interpretations of it, but also because the term is loaded with unwanted (metaphysical) presuppositions. This suggestion is indeed not novel, for example, J. S. Mill preferred the term "real kinds" to "natural kinds". Nelson Goodman (1978) speaks of "relevant kinds" instead of "natural kinds". Haslam (2002) proposed a more refined "taxonomy of kinds" that is not confined to the dichotomy of natural and artificial kinds, based on the classification of psychiatric categories. Ian Hacking (2007) proclaimed more than ten years ago that the study of natural kinds is merely a "scholastic twilight". More recently, Haslanger (2015) suggests that we should replace the notion of natural kinds by the notion of *explanatory kinds*, which focuses specifically on the explanatory purposes we try to achieve with our classification.

Nevertheless, in adopting the naturalist approach, *natural kinds* in this dissertation are understood as categories that occupy crucial epistemic roles in successful scientific practices, such as explanation, induction, and prediction. In other words, the term "natural kinds" in this dissertation is free of any metaphysical connotation. Rather than replacing it with a more refined notion such as explanatory kinds or real kinds, the long philosophical tradition associated with the term "natural kinds" would remind us of the various presumptions made by taxonomic monists and taxonomic pluralists in their debate. Only by apprehending these presumptions can we understand why the current debate has reached a deadlock. More importantly, I aim to provide a way in which the deadlock can be resolved.

In Chapter 1, I indicate that, as a non-trivial alternative to taxonomic monism, taxonomic pluralism must distinguish itself from conventionalism about natural kinds. Not only that taxonomic pluralism, epistemologically speaking, is not an "anything goes" position, but also, metaphysically speaking, it has to be a *realist* position. This amounts to the *realist commitment* of taxonomic pluralism, i.e., *natural kinds are real*. To understand the realist commitment of taxonomic pluralism, we need to separate the question of *naturalness* from the question of *reality*. The question of naturalness asks in what sense is a kind *natural*, while the question of reality asks in what sense is a kind *real*. I argue that, in identifying natural kinds as scientific categories, the naturalist stance has at most answer to the question of naturalness, but not the question of reality. I contend that the naturalist stance towards natural kinds endorsed by taxonomic pluralists requires us to give up the traditional conception of reality, which construes what is real as being what is mind-independent. However, the naturalist stance fails to supplant any appropriate substitution for this traditional conception of reality. To illustrate my argument, I discuss the shortcomings of two prominent pluralists accounts in the current literature, namely the modest pluralist account of Khalidi and the radical pluralist account of Boyd.

In Chapter 2, I formulate my answer to the question of *reality*. I suggest that the reality of natural kinds should be construed in terms of *irreducibility*. Nevertheless, the reduction involved is neither conservative nor eliminative, as it is typically understood. I elucidate why the Quinean notion of ontological commitment, understood in terms of existential quantification, does not comply with the realist commitment of taxonomic pluralism, as entailed by the naturalist stance towards natural kinds. As a result, reduction here, being an ontological notion, should be construed in terms of *relative fundamentality*. I argue that natural kinds are real if and only if they are more fundamental than their members, but not vice versa. And natural kinds are more fundamental than their members because the membership in a given natural kind *metaphysically explains* the clustering of properties typically found among members of that kind.

In Chapter 3, I present a puzzle to taxonomic pluralism by examining its second commitment, namely the *disagreement commitment*. I argue that in order for taxonomic pluralism to be true in a given scientific domain, the different classifications involved must *disagree* with each other. And this disagreement requires groupings posited by these different classifications to *crosscut*, i.e., partially overlap with each other. Nevertheless, the naturalist stance towards natural kinds may threaten genuine disagreement between classifications, as it also supports individuality pluralism. Individuality pluralism maintains that there is more than one correct way in dividing the world into individuals. If individuality pluralism is true in a given scientific domain, the different classifications that are supposed to confirm taxonomic pluralism may not classify the same set of individuals. But in order for different classifications to disagree about *how* a given set of individual entities should be classified, they must agree about *what* individual entities are there to be classified, otherwise, there would be no genuine crosscutting between groupings posited by these classifications. Therefore, the naturalist stance is in tension with taxonomic pluralism. I illustrate this puzzle by examining the classification of living organisms in biology.

Finally, in Chapter 4, I further develop the notion of reduction I outline in Chapter 2, and explain how it can be applied to adjudicate the monism/pluralism debate in cases where both the realist commitment and the disagreement commitment are fulfilled. Purportedly, the monism/pluralism debate can be formulated in terms of reduction. With regard to the various classifications scientists employ to categorize individual entities in a given domain, taxonomic pluralists assert that they cannot be reduced to each other. On the contrary, taxonomic monists maintain that these different classifications can be ultimately reduced to a single, correct classification. Given that the reality of natural kinds is now spelled out in terms of irreducibility and the reduction involved is construed in terms of *relative fundamentality* instead of elimination, we should revise the notion of reduction involved in this monism/pluralism debate accordingly. This leads us to the last commitment of taxonomic pluralism, namely the *equality commitment*. This commitment suggests that, in order for taxonomic pluralism to be true, the different classifications in question must not only be irreducible to each other (understood in an eliminative sense), but must also be *equally* fundamental. By inspecting the different classifications of chemical nuclides, I explicate in what sense one classification is more fundamental than another in terms of *explanatory asymmetry*.

Chapter 1: The Realism of Taxonomic Pluralism

I. Introduction

In this chapter, I will examine the *realist commitment* of taxonomic pluralism. Taxonomic pluralism is the view that there is more than one correct way to classify things into natural kinds in a given scientific domain. According to its proponents, taxonomic pluralism offers a *realist* alternative to taxonomic monism (Boyd 1999b, 2010b, forthcoming; Brigandt 2009; Chakravartty 2011; Kellert, et. al. 2006; Khalidi 2013, Longino 2002). Thus apart from rejecting taxonomic monism, taxonomic pluralists also has to distance their own position from conventionalism. Taxonomic pluralists must uphold that scientific taxonomies are not merely arbitrary, conventional ways of classifying things; instead, scientific taxonomies have *realist* import, i.e., they inform us how the reality is subdivided into kinds of things. This *realist commitment*, I believe, requires taxonomic pluralists to endorse the reality of the groupings posited by scientific taxonomies, rather than just the reality of the world, the reality of the things being classified, or the reality of the properties according to which things are being classified into kinds. This is because a conventionalist about classification could commit to the reality of all these without at the same time committing to the reality of any grouping posited by a given scientific taxonomy. Of course, taxonomic pluralism is not an "anything goes" position, it suggests that some, but not all, taxonomies have realist import. Let us call the groupings posited by taxonomies that are presumed to have realist import "natural kinds". Thus like taxonomic monism, taxonomic pluralism is *realist* about *natural kinds*. Yet, different from taxonomic monism, taxonomic pluralism maintains that these natural kinds cannot all be subsumed under a single classification.

However, closer examination suggests that, contrary to taxonomic monism, taxonomic pluralism may not only be an alternative worldview concerning the natural-kind structure of the reality, but a worldview that is based on an alternative conception of reality about natural kinds. If this is the case, then its disagreement with taxonomic monism may merely be verbal. For instance, taxonomic monists (e.g., Wilkerson 1995; Ellis 2001, 2014) typically believe that natural kinds are *real* in the sense that they correspond to the *mind-independent* divisions of the world. In contrast, most taxonomic pluralists (e.g., Boyd 1999b, 2010b, forthcoming; Brigandt 2011a; Chakravartty 2011; Ereshefsky 2001; Khalidi 2013; Kitcher 1984)[1] maintain that natural kinds are *real* even though they are not completely independent of us. If both sides of the debate understand the reality of natural kinds differently, then the monist conviction, i.e., "there is only one correct natural kind classification", and the pluralist conviction, i.e., "there is more than one correct natural kind classification", may come out to be true at the same time, even though literally they contradict each other.

To ensure that taxonomic monists and taxonomic pluralists are not talking past each other, we must first figure out the nature of their disagreement. Is it a disagreement about how reality is divided into natural kinds, or is it a disagreement that stems from how we should understand the very conception of reality concerning natural kinds? The goal of this chapter is therefore to investigate how we should

[1] I do not include here some of the prominent critics of monism who carry a strong antirealist overtone in their accounts, even though they push for a pluralist conception along the same lines. The reason is that the monism/pluralism debate I am dealing with in this dissertation is one *within* natural kind realism. For example, Stanford (1995), pace Kitcher (1984), argues that since "the legitimate interests of biologists *constitute* those divisions recognized as species", such divisions fail to supervene on the external world independent of our minds, which is necessary for their reality. On the other hand, although Waters (2017) maintains that his account is "realist", he rejects the very reality of the groupings in biological investigations as capturing anything fundamental about the world, since such groupings are essentially conditioned on the interests and practices of scientists. Dupré (1993) present an intriguing case of "pluralism" that I will return to later in this chapter. Also notice that although Ereshefsky (1998, 2001) is antirealist or eliminativist about the species concept (given its pluralism), he is realist about groupings generally referred as species in biology.

understand *natural kind realism*, which is supposed to be the common ground shared by both taxonomic monists and taxonomic pluralists.

Natural kind realism is the view that *natural kinds are real*. Two different questions are involved in this claim. First, in what sense is a grouping *natural*? Second, in what sense is such a grouping, being natural, *real*? To answer the first question, I will clarify what I mean by *natural* kinds in section II. I will then elucidate why the *naturalist* reading of naturalness I adopt will motivate taxonomic pluralism in section III. To answer the second question, *viz.*, in what sense a grouping is *real*, I will first explain why, in understanding the reality of natural kinds, the *naturalist* reading of naturalness requires us to give up the traditional construal of reality in terms of mind-independence. Next, in section IV, I will explain why the traditional understanding of reality, which construes what is real in terms of being mind-independent, fails to accommodate natural kind realism in the monism/pluralism debate. I will inspect the substitute conceptions of reality respectively embodied in the modest pluralist account by Khalidi (sections V and VI) and the radical pluralist account by Boyd (sections VII and VIII); I will argue that they are not robust enough to substantiate natural kind realism. In section IX, by further dissecting the flaw of Boyd's account, I will explain what natural kind realists should be realist about. My goal in this chapter is *explicative* instead of argumentative, I content myself with clarifying crucial concepts in the debate without necessarily endorsing any of them. I will argue in the next chapter that the reality of natural kinds should be understood in terms of *irreducibility*.

II. What are natural kinds?

Let me first clarify what I mean by "natural kinds". Philosophers understand the term differently. For example, they have diverse views with regard to what such kinds *are*—are they sets (Quine 1969), types (Mumford 2005), universals (Bird 2018, Hawley & Bird 2011), or *sui generis* entities such as substantial universals (Lowe 2006)? This is the question of *kindhood* (Bird 2018). Nevertheless, taxonomic pluralists generally dismiss this *metaphysical* question, as they believe that answering it would not bring us any closer to how the world is divided into kinds.[2] For example, Brigandt remarks, "a purely metaphysical construal of what a natural kind is is of very limited use" (Brigandt 2011a, 175). More specifically, Khalidi complains that a metaphysical conception of natural kind "does not give us a way of distinguishing natural from nonnatural kinds" (Khalidi 2013, 10).[3]

[2] Another reason for dismissing the question of *kindhood* at the outset is that the question goes beyond the scope of this dissertation. For example, in arguing whether natural kinds are universals or particulars, the question of natural kind realism eventually falls back to the traditional debate between realists and nominalists about universals. Thus whether or not natural kinds are real is ultimately determined by whether there are universals, together with whether we can come up with a satisfactory account of identifying natural kinds as universals. Recent examples of developing an account of natural kinds by first looking at the metaphysical question of *kindhood* are Bird (2018) and Bird & Hawley (2015). They argue that natural kinds, being real, are complex universals, instead of particulars or *sui generis* entities.

[3] We should be aware that in rejecting the metaphysical question of *kindhood*, as well as the metaphysical reading of *naturalness*, I do not thereby reject all the metaphysical issues related to natural kinds. In fact, the concern of this chapter, namely the *realist claim* of taxonomic pluralism, is a metaphysical issue itself. I completely agree that dwelling on the metaphysical nature of natural kinds will not allow us to discover which groupings are genuinely natural. Clearly, figuring out whether natural kinds are universals or particulars will not help us to determine whether classifying organisms into species according to their evolutionary lineages or according to their morphological similarities is more natural. Yet, this question of *naturalness* is completely distinct from the question of reality, namely in what sense natural kinds are *real*. Once we have neatly separated these two questions and adopted a naturalistic reading to the question of *naturalness*, the question of *reality*, as I will show below, becomes an urgent philosophical issue to taxonomic pluralists. Therefore, the naturalists' complaint of the uselessness of the metaphysical accounts is justified only when the metaphysical question of reality and the epistemological question of naturalness are mixed up, i.e., only when a metaphysical reading of *naturalness* is being adopted. However, the two questions should not be mixed up in the first place. Therefore, the *metaphysical* question of *reality* should not be dismissed at hand simply because it does not shed light on the *epistemological* question of *naturalness*.

Khalidi's complaint raises another question concerning the nature of natural kinds, which appears to be independent from the question of *kindhood*. Instead of asking what are natural *kinds*, the question is concerned with what are *natural* kinds. For things can be grouped into kinds in many ways, and it seems that some ways are more "natural" than the others.[4] This is the question of *naturalness* and it is the focus of this section.

So what does it mean by being "natural"? I propose that we should adopt a *naturalist* reading of naturalness in answering this question. This *naturalist* reading is to be contrasted with a metaphysical reading on the one hand, and a socio-psychological reading on the other hand. As we will see in the next section, this naturalist reading serves as a key motivation for taxonomic pluralism. To a certain extent, it explains why taxonomic pluralism stands on a middle ground between taxonomic monism and conventionalism.

In contending that there is no "natural" kind because no genuine distinction can be drawn between "natural" and "unnatural" properties, Nanay (2010) takes "naturalness" to be an objective feature of the world, understood in terms of David Lewis' conception of sparseness. Similarly, Bird formulates the question of *naturalness* as "[i]s the world such that there are genuinely *natural* divisions and distinctions, i.e., that there are natural differences and similarities between things" (Bird 2018, 1398, italic original)? In both Nanay's and Bird's accounts, being "natural" is construed *metaphysically*.

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[4] More specific issues related to the question of *naturalness* include: is it necessary for the properties shared by members of a *natural* grouping to be sustained by a homeostatic mechanism (Boyd 1999a, 2010a), or is it sufficient if the clustering of these common properties exhibits a certain degree of cliquishness or stability (Slater 2015)? Is a grouping *natural* if it is sanctioned by a particular scientific discipline, or are *natural* kinds to be found at the convergence of different scientific disciplines—as "categorical bottleneck" (Franklin-Hall 2015)? I will eventually pick up these issues at the later stage of my dissertation.

The metaphysical construal is unsatisfactory. As I indicated earlier, *natural kind realism* involves two questions. First, the question of *naturalness:* in what sense a kind is *natural*? Second, the question of *reality*: in what sense are natural kinds *real*? These two questions deserve separate treatments. However, in identifying naturalness as a feature of reality, the metaphysical reading of *naturalness* does not differentiate between them.[5]

On the contrary, when Quine (1969) and Hacking (1993, 2007) suggest that "natural kinds" would eventually be replaced by theoretical kinds, they are referring to pre-theoretical, folk groupings. Quine (1969) argues that although the notion of similarity and the notion of kind are not inter-definable, they are inseparable. Quine believes that our standard of similarity is innate; therefore, some ways of classifying things seem to stand out from the others "naturally". For example, given that color is a striking feature in our visual field, it seems more "natural" to classify flowers in the wild according to their colors, instead of according to the ways they pollinate (which are far from obvious to a common observer). Quine calls the groupings we employ in accordance with the innate standard of similarity "intuitive kinds" or "natural kinds". Initially, these "natural" groupings enhance our chance of survival, as they allow

[5] One may argue that the metaphysical reading of naturalness similarly separates two different questions. In understanding naturalness as an objective feature of the world, the metaphysical reading first deals with a metaphysical question, namely in what sense a kind is *real*. It then goes on to examine an epistemological question, namely whether groupings posited by our classificatory practices correspond to the *real*, i.e., *natural* kinds. Nevertheless, according to taxonomic pluralists, this is to put the metaphysical cart before the epistemological horse. Actually, many theorists who maintain a pluralist stance advocate that we should replace the metaphysically loaded term "natural kinds" with a more epistemologically oriented idiom, such as "relevant kinds" (Goodman 1978), "investigative kinds" (Brigandt 2003), "theoretical kinds" (Hacking 2007), "explanatory kinds" (Haslanger 2014), or "nonarbitrary kinds" (Ludwig 2018). This is because being "natural" is traditionally associated with being mind-independent. As we will see later in this chapter, mind-independence is not an adequate notion to understand the reality of groupings involved in the monism/pluralism debate. Moreover, a metaphysical reading of naturalness requires us to subsequently understand the reality of natural kinds in terms of existential quantification. For once we have identified naturalness as an objective feature of the world, the only meaningful way to understand the claim "natural kinds are real" is that there *exist* kinds that possess this objective feature of naturalness. As we are going to see in Chapter 2, existential quantification fails to capture correctly the ontological commitment of a realist position.

predictions at the level of food gatherings. Nevertheless, Quine believes that as science progresses, sophisticated theoretical groupings that provide more reliable predications have superseded these intuitive groupings, or "natural" kinds.[6] These intuitive groupings are "natural" from a *socio-psychological* point of view: we customarily or instinctively classify things according to them, given our "natural", i.e., inborn, propensity.

Likewise, J. S. Mill indicates that in our initial attempt to classify things, we are inclined to focus on properties that are "simple, easily conceived, and perceptible on a first view, without previous process of thought" (Mill 1858, 433). Mill discusses two different types of classification. The first type is derived from our use of names. By giving a collection of things a general name, we aim to express some common properties of that collection of things; yet, such way of classifying things, which relies on the "simple, easily conceived, and perceptible" properties, is arbitrary (Mill 1858, 433). In contrast, the second type of classification is prior to naming. It aims at "the best possible ordering of the ideas of objects in our minds" (Mill 1958, 433). While Quine's unreflective, intuitive way of classifying things does not seem to be in conflict with the first type of classification, it is unlikely to fulfill the goal of the second type of classification according to Mill. Mill maintains that the second type of classification, which allows us to ascertain the laws among different kinds of things, is "global": it refers to the "general division of the whole nature" (Mill 1858, 433).

Mill's second type of classification beholds two criteria. First, the groupings it posits should round up as many common properties as possible. Grouping things

[6] Interestingly, these theoretical or scientific groupings may appear to be "unnatural" compared to the "intuitive" or "natural" kinds. For example, classifying whales together with other terrestrial animals as *mammals* seems to be an "unnatural" choice compared to classifying them with other aquatic animals as *fishes*. The same goes for classifying marsupial mice with kangaroos and opossums, rather than with ordinary mice, into a single kind. As Quine argues, according to our "primitive standards", marsupial mouse appears to be more similar to ordinary mouse than to kangaroo or opossum (Quine 1969, 128).

according to their surface colors fails to meet this standard: red birds, red insects, red flowers, and red liquid may not share anything in common apart from *being red in color*. The second criterion requires that these groupings should identify properties that are the causes of those common properties. Again, even if apart from *being red*, red birds, red insects, red flowers, and red liquid also share other common properties, it is unlikely that *being red* is the cause of those common properties. The resulting classification, which satisfies these two criterion, is a scientific, or "Natural" classification, rather than "technical", or "artificial" (Mill 1858, 434).

So Quine and Mill share the view that there are two different stages or types of classification. On the one hand, at the initial stage of classification, there is the untutored type that answers to our psychological propensity; on the other hand, at a more advance stage, there is the more refined, scientifically sanctioned type that is the fruit of relentless empirical studies. Where Quine and Mill differ is how they refer to these two different stages or types of classification. For Quine, the initial stage is "natural", given that it is motivated by our "natural" inclination. For Mill, the more advance stage is "natural" in the sense that it is *naturalistic*. As Hacking puts it, in the *naturalist* tradition, it is empirical science, instead of metaphysics, that determines which kinds are *natural* (Hacking 2007, 218).

Let me put aside issues concerning the relation between these two stages or types of classification, such as whether the theoretical kinds are modifications of intuitive groupings, or whether between these two stages there exists a sharp distinction or merely a gradation. Since the monism/pluralism debate is concerned with whether there is one or more than one *correct* way of classifying things in a given scientific domain, I will follow Mill's usage of "natural" instead of Quine's usage. Although Mill does not directly call those groupings "natural kinds", he does refer to such classification as "natural". Should *natural kinds* inform us about how the reality is divided into kinds of things, it seems more reasonable for us to put our bets on Mill's "natural kinds" rather than Quine's "natural kinds". As Quine points out, it is doubtful why "our innate subjective spacing of qualities [would] accord so well with the functionally relevant groupings in nature" (Quine 1969, 126). Henceforth, by "natural kinds", I refer to theoretical kinds or scientific kinds, i.e., kinds or groupings that are posited by our mature scientific theories. Examples of these natural kinds are fundamental particles in physics (e.g., *quarks*), chemical elements (e.g., *helium*), and biological species (e.g., *Homo sapiens*).

The *naturalistic* reading thus construes *naturalness* as an epistemic feature.[7] Accordingly, natural kinds are identified as groupings that play crucial epistemic roles in scientific investigations; they underwrite successful practices of explanation, prediction, and induction. These groupings are "natural" because they are sanctioned by the *natural sciences*. In this regard, being *natural* is contrasted with being *arbitrary* or *gerrymandered*, rather than with being artificial or unreal, as it would be under a metaphysical interpretation.[8]

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[7] As it will become clear, my notion of "natural kinds" is more liberal than it appears. Given that naturalness comes in degrees, I am in principle open to natural kinds recognized outside of our mature scientific theories, albeit they are likely to be less "natural" than those I am dealing with in this dissertation.

[8] It should be clear that the *naturalist* reading of naturalness I propose here is not a metaphysical doctrine. Metaphysical (or ontological) naturalism suggests that natural kinds must be definable in physicalist term or constituted by physical entities. As I stated earlier, this metaphysical issue, which pertains to the question of *kindhood*, is not my concern in this dissertation. On the contrary, following Brigandt, the *naturalist* reading I propose here is a methodological position. Methodological naturalism emphasizes that philosophical study of natural kinds should be directed by scientific methods and empirical evidence, instead of any *a priori* or metaphysical assumption. This methodological conviction, I believe, has two further interpretations. On the one hand, it can be understood as stating what *methodology* should be employed in studying natural kinds. In this case, it is the methodology of natural science. On the other hand, it can be viewed as suggesting that an adequate account of natural kinds must take into consideration the *methodological roles* they play in natural science. For example, natural kinds underwrite different scientific practices, such as induction, explanation, and prediction. As we will see, these two interpretations, which are not mutually exclusive, loosely correspond to the

Once we have settled the question of *naturalness*, namely in what sense a kind is *natural*, and decided that we are going to focus on theoretical groupings in the natural sciences, the monism/pluralism debate becomes a debate about whether these groupings can all be integrated into a single system of classification. Assuming that both monists and pluralists agree upon what criteria govern successful integration, their debate could then be confined to the *reality* of those conflicting theoretical kinds, i.e., kinds that cannot be successfully integrated into a single classificatory framework. Here is the logic: for taxonomic monists, since there is only one correct classification, these conflicting kinds cannot all be *real*, as they do not fit into a single classification; for taxonomic pluralists, since these conflicting kinds are all sanctioned by the natural sciences, they must be *real*; if they do not fit into a single classification, then they must belong to different classifications. Of course, the underlying assumption is that both taxonomic monism and taxonomic pluralism are realist positions about natural kinds. Before I turn to the question of *reality*, let me first elucidate in what sense this *naturalist* answer to the question of *naturalness* may provide a *prima facie* reason for accepting taxonomic pluralism.

III. The naturalist reading and taxonomic pluralism

As we have seen, the metaphysical reading of *naturalness* is unsatisfactory because it fails to distinguish the question of *naturalness* from the question of *reality*. In construing *being natural* as an objective feature of the world, the metaphysical interpretation of *naturalness* usually appeals to different metaphysical assumptions in qualifying natural kinds. Many of these metaphysical assumptions have a strong monist leaning. Thus in rejecting the metaphysical reading, the *naturalist* reading of

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two different pluralist accounts I am going to discuss below, namely Khalidi's (2013) modest account and the more radical account put forward by Boyd (1999, 2010b, forthcoming).

naturalness I depict in the previous section dismisses at the same time these monist metaphysical assumptions and opens the door to taxonomic pluralism.

As we have seen, the *naturalist* reading of *naturalness* understands the difference between natural and non-natural groupings in *epistemic* terms. Natural kinds are privileged because of the epistemic roles they play in scientific investigations, namely they underwrite successful practices such as explanation, prediction, and induction. As Boyd suggests, natural kinds are "solutions to problems… about how to sort things so as to facilitate reliable induction and explanation" (Boyd 1999b, 72). Brigandt in particular points out that a naturalist account of natural kinds has to answer to *epistemological* questions. These questions include: "what inferential and explanatory aims scientists pursue with the study of a certain natural kind", and "how well a grouping of objects into a kind meets such inferential and explanatory aims" (Brigandt 2011a, 173-174). Reydon indicates that a naturalist approach is more promising than the traditional, metaphysical approach since "we do not have direct access to the natural kind structure of the world (if there is such a structure)" (Reydon 2010, 185); he describes the switch from the traditional, metaphysical approach to a naturalist approach as an "epistemological turn" in the study of natural kinds.

Once we shift our focus from the metaphysical question "what *are* natural kinds" to the epistemological question "what *are* natural kinds *for*", taxonomic pluralism emerges as a plausible alternative to taxonomic monism. As Brigandt points out, answering this epistemological question requires us to study natural kinds "based on the empirical details pertaining to each kind… [which] may differ from case to case" (Brigandt 2011a, 175). These empirical details depend on the specific epistemic goals a given classification is supposed to fulfill in a given investigative context. Pluralists justify their position in this way: since scientists target different epistemic goals in different investigative contexts, it is unreasonable to expect a single classification to successfully meet all these different epistemic goals at the same time.

Apart from serving as the premise for taxonomic pluralism, the *naturalist* reading of naturalness also supplants empirical evidence that is inconsistent with those metaphysical assumptions that back taxonomic monism. One such metaphysical assumption is the *hierarchy assumption*. Taxonomic monists who adhere to this assumption argue that the world has a *mind-independent* natural-kind structure, and this natural-kind structure has the form of a nested hierarchy. More specifically, the hierarchy assumption maintains that no two genuine natural kinds overlap unless they overlap completely, i.e., all the members of one kind are at the same time members of another kind.[9] Following the naturalist reading, taxonomic pluralists argue that many groupings posited in scientific investigations actually violate this metaphysical assumption: they *crosscut* each other.[10] For instance, they argue that a monist picture in biological classification is untenable because empirical evidence shows that

[9] Taxonomic monists such as Ellis (2001, 2014) argue that crosscutting kinds are problematic because they disobey the requirement that divisions between genuine kinds have to be categorically distinct. Ellis maintains that two kinds are categorically distinct when there is no gradual transition from one kind to another such that it is indeterminate to which kind a thing belongs. If there were kinds that do not have categorically distinct boundaries, then, in Ellis' view, any distinction between such kinds would be drawn by us, rather than by nature.

[10] Some of these cases in biology are: the crosscutting of the phylogenetic categories with the Linnaean taxa (Hennig 1999: 5); the crosscutting of the kinds *enzyme* and *protein* revealed by Thomas C. Cech's discovery of RNA's (ribonucleic acid) catalyzing power in the 80s (Tobin 2010). Taxonomic pluralists also cite cases of crosscutting in other scientific disciplines as evidence against the hierarchy assumption. For example, Hacking points out that crosscutting is common in chemistry. A case in point is the crosscutting between the kind *rubidium* and the kind *boson*. Neither of them is a species of the other: it is not the case that all members of the kind *rubidium* are members of the kind *boson*; nor is it the case that all members of the kind *boson* are members of the kind *rubidium*. Nonetheless, *rubidium-87* is a species of both of them (Hacking 2007: 214).

biological kinds do not form a single hierarchy: many well-founded biological taxa *crosscut* each other.

I will say more about the hierarchy assumption in Chapter 3. At this stage, it is crucial to see how an assumption like it, which is supposed to govern how natural kinds should be integrated into a classification, plays out in the monism/pluralism debate. Since taxonomic monism and taxonomic pluralism disagree about whether there is one or more than one correct way of classifying things into natural kinds in a given scientific domain, the groupings in question are therefore those that cannot be subsumed into a single classificatory framework. Under the hierarchy assumption, this single classificatory framework should have a nested hierarchical structure. Taxonomic pluralists reject this assumption by appealing to the fact that many scientific groupings that are well founded on empirical evidence actually crosscut each other. Taxonomic monists respond to this challenge by claiming that those crosscutting kinds, for instance, biological kinds, are not genuine natural kinds, precisely because they violate the hierarchy assumption.[11]

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[11] Quite often, in addition to the hierarchy assumption, taxonomic monists also appeal to the *essentialist* assumption in dismissing the crosscutting kinds as unreal. According to the essentialist assumption, members of a given natural kind share a set of (unchanging) intrinsic properties that is both necessary and sufficient for membership in that kind. For example, Ghiselin (1987), in supporting his view that biological species are not classes/kinds but individuals, adds that this is because species evolve but classes/kinds do not change, along with the difficulties in constructing a nested hierarchy for species. Likewise, in arguing that biological kinds are not natural kinds, Ellis (2001) suggests that biological species, apart from violating the hierarchy assumption, also run afoul of the essentialist assumption. Indeed, the hierarchy assumption is partly implicated in the essentialist assumption. For those who uphold the essentialist assumption usually further commit themselves to the view that each individual entity has a set of essential attributes (i.e., its essence) that makes it the type of entity it is; and a classification that mirrors the structure of reality should be constructed according to the essences of things. Accordingly, an entity should not belong to more than one kind when those kinds are not hierarchically ordered. For this would mean that we do not know what that entity is, given that membership of a kind stands for an entity's identity.

So, with regard to groupings that cannot be integrated into a single classificatory framework, there is no dispute about whether they really violate the hierarchy assumption or whether these kinds have any important epistemic roles to play in scientific investigations. Taxonomic monists and taxonomic pluralists concur with each other in these two issues. What they disagree about is which of these two issues, i.e., the metaphysical assumption made by monists or the epistemic roles emphasized by pluralists, should be decisive in settling the question of whether a grouping is real. On the one hand, pluralists maintain that these crosscutting kinds are *real* because they are indispensable to empirical investigations; on the other hand, monists believe that these crosscutting kinds should be dismissed as *unreal*, since they violate the metaphysical assumption at issue, e.g., the hierarchy assumption. The dispute turns into a conceptual one concerning the very criterion of *reality*. In order to resolve this dispute, we need to first ensure that both taxonomic monists and taxonomic pluralists appeal to the same criterion of reality.

IV. Natural kind realism and mind-independence

Let me now turn to the question of *reality*, namely in what sense are natural kinds *real* (if they are *real* at all)? Unlike the question of naturalness, which is *epistemic* in nature, the question of reality is a *metaphysical* question. It is concerned with whether natural kinds, i.e., groupings posited by scientific classifications, inform us how the reality is divided into kinds of things.

My goal in this and the next two sections is to explicate how *natural kind realism*, i.e., the claim that *natural kinds are real*, should be understood, rather than vindicating it. In this section, I will first argue why we should give up the traditional understanding of reality, which construes what is real in terms of being mind-independent. I will contend that this traditional understanding does not fully capture the crux of the debate between realists and antirealists about natural kinds. I will also explain why giving up the traditional understanding of reality will open the door to taxonomic pluralism. In the next four sections, I will examine respectively the conceptions of reality endorsed by two different pluralist accounts. They are: the modest pluralist account proposed by Khalidi and the radical pluralist account put forward by Boyd. I will argue that the naturalist approach to natural kinds endorsed by these pluralist accounts succeeds in exposing the shortcoming of the metaphysical approach, yet it fails to substitute the traditional conception of reality with a defensible one.

Traditionally, the standard doctrine of realism construes the reality of the external world in terms of its mind-independence. What is real, therefore, "is not made up of 'ideas' or 'sense data' and does not depend for its existence and nature on the cognitive activities and capacities of our minds" (Devitt 2005, 768). Natural kind theorists who embrace this understanding of realism assume that if natural kinds are real, then they must be part of an objective, mind-independent reality.[12] In other words, the world comes with its own natural-kind structure(s), independent of our classification. Call this the traditional reading of natural kind realism. Among the supporters of this view we find Devitt (2011), Franklin-Hall (2015), Lowe (2014), Tahko (2012, 2015), Sorenson (2011), etc.

Devitt writes (2011, 158),

[12] In general, philosophers who construe *naturalness* as a mind-independent feature of the world, like Bird (2018) and Nanay (2010), understand the reality of natural kinds in terms of mind-independence, since they do not separate the question of naturalness from the question of reality. Of course, it does not mean that those who understand the reality of natural kinds in terms of mind-independence must adopt at the same time a metaphysical answer to the question of *naturalness*.

> The background issue that is most relevant is often known as "realism about the external world," concerned initially with the observable entities of common sense but spreading to scientific entities, both observable and unobservable. Let us attend only to scientific entities. What is realism about these entities? It has two dimensions, one committed to the *existence* of entities, the other to their *mind-independence*. We can capture the doctrine well enough as follows: *Realism*: Entities of most scientific kinds exist mind-independently.

More specifically, Lowe (2014, 20) writes,

> Now, what *grounds* the distinctive existence and identity conditions governing the members of different kinds—where, by such conditions, I mean those necessary and sufficient for the *existence* of any given member of the kind and for the *identity or distinctness* of any two such members? Here we are immediately confronted with the fact that *natural* kinds—such as the kinds *monkey* and *mountain*—are presumably wholly *mind-independent*, whereas artefact kinds are plausibly not.

Franklin-Hall (2015, 926) also writes,

> On the realist approach, as traditionally construed, the universe possesses a mind-independent natural-kind structure, such that whether a category or classification ''carves at the joints'' has nothing to do with its place ''in human languages, conceptual schemes, biology, or anything like that''(Sider 2012, p. 5). Thus we do well or badly, classification-wise, to the extent that our partitions track the kinds embedded in nature itself, and the pathological categories are those that in no way—even but through a glass darkly—match the world's own.

Yet, the traditional reading of realism is too narrow to accommodate many genuine theoretical groupings in scientific disciplines such as biology, psychology, and sociology. Although these groupings are not strictly mind-independent, philosophers and scientists still want to maintain a realist stance towards them, given the epistemic roles they play in their disciplines. For example, the 24 synthetic elements that can be found on the periodic table would not have existed on our planet without being created artificially in the laboratory; psychiatric kinds such as mental disorders that are by nature mind-dependent (Beebee & Sabbarton-Leary 2010; Haslam 2002; Tsou

2016); and social categories such as race and gender, which are uniquely human (Bach 2012; 2016; Haslanger 2000; Sveinsdóttir 2011).[13]

Indeed, it is often the reality of these groupings (i.e., groupings that are not strictly independent from human conceptualizations and practices) that are in dispute in the monism/pluralism debate. Therefore, it is unfair to adopt a criterion that rules out reality of these groupings at the outset. Since both taxonomic monists and taxonomic pluralists are supposed to be realists about natural kinds, an adequate realist account should house both of them. Franklin-Hall does not overlook the importance of such inclusiveness when she claims that, in adopting the mind-independence criterion, her reading of realism is permissive to both positions. As she puts it, her conception of realism leaves open "questions on which particular realist theories may differently commit" (Franklin-Hall 2015, 929). However, she draws the line in the wrong place. As we are going to see shortly, in adopting a naturalist reading of *naturalness*, most pluralist accounts in the current literature prioritize empirical findings over metaphysical assumptions in studying natural kinds. As a result, many of the conflicting scientific classifications they refer to in supporting their pluralist worldview posit groupings that are not completely mind-independent. If we stick to the traditional reading of realism, these accounts, which pose the biggest challenges to monism, will no longer be counted as pluralistic. This is because they will not be counted as genuine realist accounts about natural kinds at all. Hence, in order to capture the crux of the current debate between taxonomic monists and

[13] The types of mind-dependence involved in these examples are different. For example, synthetic elements are regarded as occupying the empty slots on the periodic table. In this case, the existence of the kinds themselves does not depend on us, but the existence of their members is causally dependent on us, at least on the planet Earth. On the contrary, our conceptualization seems to take a *constitutive* role in the formation of social kinds such as race and gender. Khalidi (2016) discerns four different categories of dependence/independence; Franklin-Hall (2015) also distinguishes two different types of mind-dependence in terms of *content* and *status*. Since mind-independence is irrelevant to the reality of natural kinds (as I will soon show), I will not get into the details of these distinctions.

taxonomic pluralists, we should give up the traditional reading of realism, which construes what is real in terms of being mind-independent.

Furthermore, I believe that the mind-independence criterion does not align with our philosophical conception of reality. Consider the debate in the philosophy of mind concerning the ontological status of mental states. Dualists and physicalists disagree on different grounds about whether mental states are genuinely real. But they never dispute that mental states are mind-dependent: physicalists would not argue that mental states are unreal because they are mind-dependent.[14] Instead, physicalists argue over issues such as whether mental states are identical to physical brain states (as in J. J. C. Smart's identity theory) or whether mental states can be reductively explained away (as reductive physicalists hold). Hence, our philosophical understanding of reality does not strictly hinge on whether an entity is mind-independent. In short, being mind-independent is not necessary for being real.

Being mind-independent is also not sufficient for being real. Consider Merricks' (2001) argument against the reality of ordinary material objects, such as rocks. Although the existence of these objects is not mind-dependent, Merricks contends that they are not real because they can be *reduced* to the particles that compose them. For

[14] By drawing a distinction between essential dependence versus modal dependence, Cameron (2008) argues that Devitt's formulation of realism, which identifies what is real with what is mind-independent, does not automatically rule out the reality of mental activities. This is because even realism about mental activities has to accept that mental activities necessarily depend on the mind, it denies that mental activities *essentially* depend on the mind. So, even we accept the mind-independence reading of reality, realism about mental activities is not *trivially* false. While Cameron's suggestion seems to resolve the conflict between the mind-independence reading of reality and the realism about mental activities, it would not safeguard the mind-independence reading in our current debate. First, if Khalidi is correct that a distinction between necessary and contingent dependence fails to distinguish real kinds (e.g., *oxygen*, *Homo sapiens*) from unreal kinds (e.g., *phlogiston*, *fairies*), then it is even more difficult to see how a further distinction between essential and modal dependence could do the job. As Khalidi rightly points out, *unreal* kinds are contingently mind-dependent, they are neither necessarily nor essentially mind-dependent. More importantly, given their anti-metaphysical stance, it is unlikely that taxonomic pluralists would accept a metaphysically-loaded distinction between necessary dependence and essential dependence. In fact, most taxonomic pluralists are anti-essentialists.

instance, according to Merricks, everything that can be explained by referring to an individual "rock" can in principle be completely explained by referring to the fundamental particles that compose the rock in question. If this is true, then it seems that the individual "rock" has no causal role to play in the world, and is therefore not real. Setting aside whether or not Merricks' argument is sound, the point to take home is that, from a philosophical point of view, being mind-independent may not be sufficient for an entity to be considered real.[15]

As we have seen, the traditional reading of realism dismisses from the outset a significant portion of theoretical groupings in various scientific disciplines as unreal, even though these groupings are no less sanctioned by empirical evidence than other alleged mind-independent groupings. Dissatisfied with this result, many theorists, in particular those who adopt a naturalist reading of naturalness, call for a realist account that emphasizes on the epistemological aspects of natural kinds. Instead of requiring real kinds to be strictly mind-independent, they argue that natural kind realism should endorse a conception of reality that would allow us to account for the epistemic roles of natural kinds in scientific investigations, i.e., the fact that natural kinds underwrite successful practices such as explanation, induction, and generalization (Boyd 1999b, 2010b, forthcoming; Brigandt 2009, 2011; Haslanger 2016; Khalidi 2013, 2016; Koslicki 2008; Magnus 2012).

Thus Boyd (1999b, 7, italic mine) writes,

> Here's an important sense in which natural kinds and their naturalness are not independent of human purposes, interests, aims and practices. If we

[15] Of course, in retrospect, one may retort, why is there a "rock" that gives us the illusion that it exists, if such a "rock" is not mind-dependent? However, this "mind-depending" feature is universal to any concept of an alleged object, whether the concept applies to something real or not. The concept of "rock" is as mind-dependent as the concept of fundamental particles the "rock" is composed of. Indeed, Merricks' argument has nothing to do with the connection between the reducible, unreal objects and the fact that they are mind-dependent. Again, the distinction is between concepts that capture something real compared to concepts that do not, rather than between something that is non-conceptual/mind-independent and something that is conceptual/mind-dependent.

> adopt the standard realist and naturalist conception of natural kinds as vehicles for the identification of projectible generalizations, *then practice dependence is entailed*.

And Khalidi (2013, xiv) writes,

> It is common for philosophers to express realism about kinds in terms of the claim that kinds are human- or mind-independent, but I reject this way of grounding realism since it threatens to rule out all psychological and social kinds. More importantly, to be real, a kind need not be independent of human beings or their minds; it must simply be manifested in the world (a world that includes the human mind). The surest way to ensure that our categories identify real kinds is to pursue a scientific method that serves epistemic purposes.

Nevertheless, there is a clear tension between being mind-dependent and being real. Obviously, many mind-dependent objects, such as the dagger Macbeth hallucinated, are not real. So how should theorists who embrace a naturalist answer to the question of naturalness revise their understanding of natural kind realism so as to preserve the reality of natural kinds that are mind-dependent?

One attempt to ease the tension between the mind-dependent character of many natural kinds and their alleged reality is to limit the application of the mind-independence criterion. For example, we may confine the mind-independence criterion to non-mental entities only, thus allowing mental kinds such as psychiatric kinds and emotions to be real even they are mind-dependent. As Khalidi points out, apart from being unparsimonious, this strategy still excludes artificial groupings such as synthetic biological and chemical kinds, as well as many groupings in the social sciences. Further granting exception to these entities can at best be *ad hoc* (Khalidi 2016, 225). Another attempt to refine the mind-dependence criterion is to separate the harmless types of mind-dependence from the problematic types. For example, Boyd (1991) argues that only constitutive dependence, as opposed to causal dependence, is incompatible with the reality of natural kinds. Yet, as Khalidi (2013) indicates, Boyd's explication of the difference between constitutive and causal dependence, and other similar proposals, are far from satisfactory.[16] I agree with Khalidi that the strategy of bracketing certain harmless types of mind-dependence is doomed to fail. In particular, this strategy fails to explain why there are two different standards of reality, despite the fact that there is only one world. Does applying a different standard of reality to entities that are mind-dependent imply that these entities are less *real* than entities that are mind-independent? If yes, then in what sense are these mind-dependent entities *really* real? But if the answer is no, then the need to introduce a separate standard of reality for mind-dependent entities only suggests that a more fundamental standard of reality is in play. This more fundamental standard would construe the reality of both mind-independent and mind-dependent items in the same way, rather than positing one criterion of reality for the former and a different criterion of reality for the latter. As Haslanger argues, even the mind-independence requirement "provides good reason for rejecting one conception of 'objective reality,' this does not force us into either skepticism or idealism, for there are other ways of conceiving what it means to be real and other ways of conceiving an 'independent' reality" (Haslanger 1995, 97).[17] I thereby side with Khalidi (2016) in seeing that mind-independence is merely a "red-herring" in the discussion of natural kind realism.

[16] For a detailed assessment of different versions of "mind-dependence", see Khalidi (2016).

[17] Even though Haslanger does not describe the "mind-independence" as a "red-herring", she points out "the reason why the previous models of justice, knowledge, and reality have gone so wrong is that they ignore the force of *social construction*" (Haslanger 1995, 95). Thus, in discerning different types of social construction, such as that of causal, constitutive and pragmatic, her goal is different from Boyd and Ereshefsky. Instead of trying to redraw the distinction between what is real and what is not real, she aims to figure out the relevant aspects of "social construction" or "mind-dependence" of our classifications in different discourses.

V. Khalidi: projectibility and nodes in causal networks

Let me now turn to Khalidi's account and inspect what notion of reality it endorses in spelling out natural kind realism. In dismissing the notion of mind-dependence as a red-herring, Khalidi attempts to draw the line between real kinds and unreal kinds elsewhere. He proposes that we should look at the very purpose a given classification aims to serve in evaluating the reality of the groupings it posits. Khalidi concurs with Dupré (1993) that there does not exist any "pure" classification, i.e., classification that is completely independent of our interests. Yet, he distances himself from Dupré's all-too-liberal "promiscuous realism". [18] For Khalidi, not all (practically useful) classifications have realist import. While Dupré believes that the very intention behind a classification has no bearing on the ontological status of its groupings, Khalidi maintains that only classifications that are introduced primarily to serve *epistemic* purposes would capture the real structures of the world.[19] As Khalidi argues, "[t]he surest way to ensure that our categories identify real kinds is to pursue a scientific method that serves epistemic purposes" (Khalidi 2013, xiv). And "what distinguishes epistemic purposes from other purposes is that our best epistemic practices aim to uncover the divisions that exist in nature" (Khalidi 2013, 63).

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[18] Dupré argues that theoretical groupings in science are no more "natural" than prescientific or vernacular groupings found in ordinary discourse. By referring to classificatory practices in biology, Dupré contends that it is inaccurate to say that prescientific classifications are partial and subjective, while scientific classifications are impartial and objective. According to Dupré, the latter are no less anthropocentric than the former because there exists inescapable "uncertainty about what constitutes the distinct existence of similar but related species" (Dupré 1993, 35-36). Thus any standard or criterion we use to classify things into kinds can only be chosen for practical reasons, such as human application (Dupré, 1993, 36). Moreover, due to the multifarious interests of scientists, scientific classifications fare no better than prescientific classifications in presenting a systematic, orderly picture of the world.

[19] It should be clear that the distinction between epistemic and non-epistemic is concerned with the question of *naturalness*. Thus in my account, Dupré's "promiscuous realism" is not a genuine realist position. This is not because he denies that natural kinds are real, but because he does not posit any distinction between groupings that are natural and groupings that are not. In other words, Dupré does not provide a positive answer to the question of *naturalness*. In contrast, natural kind realism commits to the claim that some ways of classifying things to kinds are natural (while the others are not), and the groupings posited by these natural classifications, accordingly, are real.

So in what sense natural kinds reveal the "divisions that exist in nature"? According to Khalidi, "[o]ur best guides to nature's divisions are those categories that enable us to explain and predict natural occurrences by tracking causal patterns. Hence, categories that serve this epistemic purpose denote natural kinds" (Khalidi 2013, 160). Assuming that Khalidi is correct that genuine epistemic practices should aim at uncovering the causal structures of the world, then natural kinds have realist import precisely because they track the causal structures of the world. For Khalidi, the reason for believing that natural kinds successfully track the causal structures of the world is that natural kinds are *projectible.*[20] Khalidi believes that this epistemic feature is not only "the most widely agreed upon characteristic of natural kinds" (Khalidi 2013, 18), but also, more importantly, it leads "naturally to a metaphysical account in terms of causality" (Khalidi, 2013, xii). As Khalidi sums up,

> Science aims to identify projectible properties, particularly clusters of properties that point reliably to yet other property clusters. The fact that these properties are projectibly clustered indicates that there are causal links between them. Hence projectibility is the epistemic marker for the metaphysical relation of causality" (Khalidi 2013, 80).

[20] While projectibility is usually understood as a property of predicates, here projectibility designates an epistemic feature of natural kinds. The notion of projectibility is originally used by Goodman to contrast conflicting hypotheses, i.e., hypotheses that give predictions that at most one can be true at the same time. Goodman suggests that a hypothesis is "projectible" "if and only if it is supported, unviolated, and unexhausted, and all such hypotheses that conflict with it are overridden" (Goodman 1983, 108). The crucial notion here is of course, "overridden". The first three criteria (i.e., being supported by existing evidence, not being violated by counterevidence, and being unexhausted in the sense that it has instances yet to be determined) are basic criteria for any amplitative inference or induction. According to Goodman, a hypothesis *H* (e.g., "All Ps are Qs") is said to "override" *H'* "if the two conflict and if *H* is the better entrenched and conflicts with no still better entrenched hypothesis" (Goodman 1983, 101). Although projectibility goes hand in hand with entrenchment, for Goodman, the latter is specifically a property of a predicate. A predicate (i.e., P, as well as Q, in our example) is better entrenched if it has been used more frequently for projection than another (e.g., P', or Q'), i.e., that it has been used more frequently in a projected hypothesis. In other words, a better entrenched predicate has a "more impressive biography" (Goodman 1983, 94). Here I will adopt Quine's use of the term projectibility in describing kinds, and not just hypothesis or prediction involving natural kinds. According to Quine, projectible predicates, e.g., P and Q, are predicates "whose shared instances all do count… towards confirmation of [All P are Q]" (Quine 1969, 115). Moreover, the choice of "projectibility" over "entrenchment" is to emphasize the *propensity* of natural kinds in delivering correct prediction instead of just its *past record*.

Behind this realism about natural kinds is a more general realist stance towards science. As Khalidi claims, "once we adopt a realist stance towards science… we thereby accept that the categories that science devises in order to understand nature provide the best insight into the kinds that really exist" (Khalidi 2013, 65-66). Now the crucial question is: what justifies this "realist stance towards science", which allows Khalidi to proceed from the *projectibility* of natural kinds to their reality?

Khalidi does not explicitly put forward any concrete argument in justifying this "realist stance towards science". I think that the "No Miracle" argument that is traditionally used to support scientific realism is most relevant here. The "No Miracle" argument, as Psillos summarizes, contends that "the impressive predictive and explanatory successes of scientific theories would remain unaccounted for, unless we accept that the entities, processes and causal mechanisms they posit to operate behind the phenomena are real" (Psillos, 2005, xx). Natural kinds in Khalidi's account are exactly these entities posited by different scientific theories: they "enter into new generalizations, are explanatorily fertile, and generate novel predictions" (Khalidi 2013, 44).

Providing a thorough assessment of the "No Miracle" argument is beyond the scope of this dissertation. Yet, even if the "No Miracle" argument is cogent, it is unclear whether Khalidi can directly apply it to justify his realist stance towards natural kinds. According to the "No Miracle" argument, the unobservable theoretical entities postulated by our best scientific theories cannot merely be our subjective projections onto the world. They must correspond to an objective, mind-independent reality; for otherwise it would be impossible to explain the extraordinary success of our explanatory, inferential and predictive practices made possible by these theoretical posits. Therefore, the "No Miracle" argument is founded on a distinction between a mind-independent, objective reality and our subjective projections. But once we allow kinds the depend on our own conceptualizations to be counted as real, as Khalidi does, we can no longer rely on this very distinction to account for the reality of natural kinds. So Khalidi owe us an argument as to why the epistemic feature of *projectibility* is sufficient to vindicate the reality of natural kinds.

VI. Problems of Khalidi's natural kind realism

In fact, the problem of Khalidi's realist account about natural kinds goes deeper. Apart from the issue of justification I pointed out in the previous section, there are two other conceptual issues. These two conceptual issues are concerned with the notion of epistemic purposes, which is supposes to distinguish natural, real kinds from unreal kinds in Khalidi's account. First, it is unclear how we can clearly separate the epistemic from the non-epistemic (Longino 1996; Kitcher 2001). Second, even if we can stipulate such a distinction, confining epistemic purposes to tracking causal patterns does not seem to provide a complete picture of how classificatory practices are deployed in actual scientific investigations.

Consider an investigation that aspires to discover the causal structure of the chemical world. While this is a legitimate epistemic purpose according to Khalidi, it is doubtful how this investigation, in order to accomplish its goal, can be isolated from other types of research in chemistry that are more practice-oriented, e.g., pharmaceutical or medical research. If "epistemic purposes" are understood as those that "aim to uncover the divisions that exist in nature", then quite often fulfilling these "epistemic purposes" is not an end but a means for us to advance other "non-epistemic" goals, such as administering more efficient strategies or achieving better control over the external world. Indeed, research in chemistry is strongly driven by all sorts of application-related considerations. Furthermore, even securing such a "purely epistemic" goal often requires practices that are embedded in a broader investigative context that is not free of non-epistemic or practical interests. For instance, Baetu indicates that the discovery of biological mechanisms "involves the piecing together of experimental results from interventions" (Baetu 2016, 3311). While such interventions target multiple variables in a controlled fashion, they are chosen for their practical benefits instead of any abstract, purely epistemic purpose. Reydon (2016) also points out that classifications in actual scientific practices are multifarious, and it is the investigators who ultimately decide which criteria are adopted. Their decisions rely on the given investigative agenda, which is not necessarily "epistemic" in the narrow sense described by Khalidi. For example, in the classification of gene types, scientists shift between different criteria such as sameness of locus in the genome, lineage of descent, or functional similarity, depending on what goal they try to achieve (Reydon 2016). These epistemic and non-epistemic agendas intertwine in scientific investigations, whether the investigators ultimately aim to fulfill epistemic or non-epistemic purposes. Thus it is unclear precisely where we should draw the line between the epistemic and the non-epistemic. More importantly, it is questionable which of these goals are prior to the other.

And even if we can distinguish epistemic purposes from non-epistemic purposes, Khalidi's notion of epistemic purposes does not seem to fully capture the actual classificatory practices in the natural sciences. In addition to the claim that natural kinds are discoverable by science, Khalidi further asserts that *all* scientific categories correspond to natural, real kinds (Khalidi 2013, 43). This more "contentious" claim, according to Khalidi, suggests that we should not divide scientific categories into natural kinds on the one hand, and epistemic or investigative kinds on the other hand, as proposed by Brigandt (2003), for example. Khalidi finds such division arbitrary as it implies that some scientific categories fall short of providing us "insight into kinds that exist in nature" (Khalidi 2013, 43). In contrast, he believes that being *projectibe* is both necessary and sufficient for a given category "to be accepted as part of our established scientific theories" (Khalidi 2013, 43).

Khalidi admits that the above claim is "contentious" because realists in general are not likely to accept the view that all scientific categories align with natural kinds (Khalidi 2013, 43). However, I think that this claim is "contentious" for the opposite reason. "Scientific categories" understood as *projectible* kinds or causal nodes according to Khalidi do not exhaust all scientific categories in actual scientific investigations. Khalidi's notion of scientific categories is too narrow as it confines genuine scientific categories to groupings that serve a particular type of epistemic purpose, i.e., uncovering the causal structure of the world. For example, in criticizing Laudan's (1984, 2004) distinction between epistemic and cognitive values or virtues, Reiss & Sprenger argues that "[n]eat distinctions between strictly truth-conducive and purely cognitive scientific values are hard to come by (Reiss & Sprenger 2014).[21] Some philosophers, such as McMullin (1983, 1996, 2014), similarly advocate a

[21] Two other trends in the philosophy of science with regard to the distinction of epistemic and non-epistemic are also relevant here. On the one hand, there are those who dismiss such distinction. For example, Longino (2002) argues that objectivity of science should take into consideration values that are traditionally regarded as "non-epistemic". And in rejecting the value-free ideal of natural science, Douglas claims that the distinction between epistemic and non-epistemic values presupposed by the value-free ideal is unviable. As she suggests, social, non-epistemic values "are influencing science through epistemic values" because they "shape the instantiation of epistemic values" (Douglas 2009, 91). On the other hand, there are those who defend such distinction yet argue that non-epistemic values are no less legitimate than epistemic values in evaluating scientific theories. Non-epistemic values are more than just "tiebreakers" when two hypotheses or models are equally well supported by epistemic considerations. For example, Brigant (2013) contends that social values (i.e., non-epistemic values) have a significant impact on determining the conditions of adequacy of a scientific theory, and such impact is not affected by the increase of empirical evidences. Elliott & McKaughan argues that, "[s]cientists need not always maximize the fit between a model and the world; rather, the purposes of the users determine what sort of fit with the world and therefore what balance between epistemic and nonepistemic considerations is needed in particular contexts" (Elliott & McKaughan 2014, 5). While assessing these different arguments would go beyond the concern of this dissertation, it should be clear that these two trends are incompatible with Khalidi's account of natural kind realism, which privileges epistemic purposes over non-epistemic purposes.

broader notion of "epistemic values" based on the assumption that truth is not always the only goal of scientific enquiry; other goals, such as creating understanding, are also important. Likewise, Waters (2017) contends that scientists normally do not aim at answering questions such as "what is a biological individual", "what is a gene"—questions that are, according to Khalidi, "purely epistemic" and "aim to secure knowledge of real features of the universe" (Khalidi 2013, 216).

While Waters' argument is mainly derived from the individuating practices of genes in biology, Ereshefsky & Reydon provide a more relevant example here.[22] As they put it, "though many philosophers are keen on science revealing the causal structure of the world, such enthusiasm for causality is far from universal among scientists" (Ereshefsky & Reydon 2015, 974). They point out that many of the empirical parameters scientists employ to group microbes together according to the Phylo-Phenetic Species Concept (PPSC) are not purely epistemic (in Khalidi' sense). These parameters, such as phenotypic and genotypic similarities, are not chosen for the sake of tracing the causal mechanisms underlying bacterial species. Instead, they are chosen because they allow ready and stable identification, which is crucial for scientific investigations. Interestingly, this is far from exception, if we take into the consideration the fact that not only PPSC is the most widely accepted species concept in microbiology, but also most of life is microbial. Furthermore, groupings that serve the purpose of inference may only correlate with each other, without the correlations being underwritten by causal relations, as it is required to satisfy Khalidi's "pure epistemic purposes".

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[22] Yet, Waters' "no general structure thesis" is clearly relevant here, as he argues (Waters 2017, 83, italic original),

> I expand my conclusion about genetics to motivate the *no general structure thesis:* the world lacks a general, overall structure that spans scales. It does not have a "*the* causal structure" that Salmon (1984) claims all explanations should fit into, and it lacks the kind of overall "definite and mind independent structure" that Psillos (1999) claims is an essential tenet of realism.

It is therefore hard to see how Khalidi can establish his natural kind realism by appealing to his notion of "epistemic purposes". The problem is not only that it is difficult for us to separate on the *epistemic* level classifications that are motivated by *bona fide* epistemic purposes from classifications that are not. The more fundamental problem resides on the *conceptual* level. If according to Khalidi, natural kinds are discovered by natural science and at the same time genuine natural science is confined to inquiries motivated by "epistemic purposes", then many of the scientific categories that we accept as natural kinds will be undesirably excluded. In fact, there does not appear to exist any unified "epistemic purpose" or over-arching goal behind all genuine scientific classifications, whether classification is taken as a means to achieve some other goals or as an end in itself. As Khalidi admits, "there is still a great deal more to be said about what epistemic purposes are and what it is to be guided by them" (Khalidi 2013, 222). All in all, the prospect is dim. As we have seen, in order to establish the reality of scientific categories on their naturalness, Khalidi appeals to the fact that these categories are *projectible*. Yet, projectibility can be found in categories posited by classifications that are driven by reality-revealing, "epistemic" purposes, as well as classifications driven by conventional, "non-epistemic" purposes. To say that classifications aiming to discover "the real causal structure" will result in projectible categories is to put the metaphysical cart before the epistemic horse, instead of the other way round, as Khalidi believes.

So, although Khalidi is correct to point out that any attempt to safeguard the traditional notion of reality by reformulating the mind-independence/mind-dependence distinction is futile, his own alternative proposal fares no better. As we have seen, Khalidi's does not explain how the realist stance towards scientific categories can be justified. Clearly, this realist stance is not automatically vindicated by the notion of "epistemic purposes" he formulates to discern real kinds from non-real kinds. Indeed, Khalidi definition of "epistemic purposes" is unsatisfactory. In confining "epistemic purposes" to classifications that set sight on tracking the causal patterns of the world, Khalidi fails to pay attention to the multifarious goals that motivate classificatory practices in actual scientific investigations. In other words, Khalidi's notion of *natural* kinds is too narrow. In the coming section, I am going to scrutinize to what extent a broader or a more radical notion of *naturalness* could offer a defensible account of natural kind realism.

VII. Boyd: "accommodationism" and disciplinary matrix

Here is an interesting observation: the objections against Khalidi's account of natural kind realism I presented at the end of the previous section do not come from the monist camp; instead, they are made by his pluralist allies. In identifying natural kinds as "nodes in causal networks" (Khalidi 2013, 207), Khalidi deduces his pluralist conclusion by alluding to the fact that "there is no single causal template that fits all instances of natural kinds or relates natural kinds to their associated properties" (Khalidi 2013, 80). Since different sciences seek explanations on different levels, it is unlikely that groupings posited by these different special sciences would all fit harmoniously into a single classification.[23] Yet, as we have seen, scientists do not always aim at tracking causal patterns with their classifications. Thus there are in fact more natural kinds than groupings that stand for "nodes in causal networks", as Khalidi believes. In other words, Khalidi's notion of *natural* kinds is too restrictive.

[23] I will scrutinize this pluralist claim in Chapter 4.

Given his partial understanding of "epistemic purposes", Khalidi fails to acknowledge many genuine scientific grouping as natural kinds. So, if we accept that Khalidi is at least correct in drawing a pluralist conclusion from his restrictive notion of natural kinds, expanding the notion of natural kinds to include categories posited by classifications that do not aim at tracking causal patterns will probably result in a more radical version of taxonomic pluralism.

This radical version of taxonomic pluralism faces an immediate issue. Although it is unclear where we should draw the line between the epistemic and the non-epistemic, it is inadvisable for taxonomic pluralists to give up such distinction completely. As I mentioned before, taxonomic pluralism is not an "anything goes" position. As an alternative not only to taxonomic monism but also to conventionalism, taxonomic pluralism upholds the conviction that some classifications are more *natural* than the other. Without any distinction between the epistemic and the non-epistemic (or a distinction of a similar sort), it is hard to see how we can provide an adequate answer to the question of *naturalness*. As Kellert et al. (2006) also point out, an extreme pluralist position such as Dupré's "promiscuous realism", which treats scientific and non-scientific classifications as equally correct, is basically indistinguishable from a relativist position.

So, how can this more radical version of taxonomic pluralism be made viable, i.e., how can we reformulate the distinction between the epistemic and the non-epistemic without running into the same troubles of Khalidi's account on the one hand, and without relegating into complete conventionalism on the other hand? Perhaps the only way out is not to assume a hard and fast line between the epistemic and the non-epistemic (or any distinction of a similar sort) in the first place, and embrace a more flexible notion of *naturalness*. To examine the feasibility of this strategy, in the following, I will turn to a leading example of this more radical version of taxonomic pluralism, namely the "accommodationist" account of natural kinds Boyd has pursued and developed in the past 30 years.

To begin with, recall that according to Khalidi, the distinction Boyd makes between causal and constitutive mind-dependence fails to safeguard the traditional reading of realism, which construes what is real in terms of being mind-independent. While I believe that Khalidi's criticism is right on track, I also want to point out that in his more mature writings, Boyd no longer focuses on this distinction in explicating the reality of natural kinds (even though he has never explicitly rejected the distinction).[24] Instead, he develops the notion of "accommodation" to do the job. According to his "accommodationist" thesis, not only doesn't mind-dependence conflict with the realism of natural kinds, but also, more importantly, "natural kinds and their definitions are discipline-or-practice relative and are thus not 'mind independent'" (Boyd, forthcoming). As He indicates (Boyd 1999b),

> The lesson we should draw from the accommodation[ist] thesis is that the theory of natural kinds *just is* (nothing but) the theory of how accommodation is (sometimes) achieved between our linguistic, classificatory and inferential practices and the causal structure of the world. A natural kind *just is* the implementation, in language and in conceptual, experimental and inferential practice, of a (component of) a way of satisfying the accommodation demands of a disciplinary matrix. Natural kinds are features, not of the world outside our practice, but of the ways in which that practice engages with the rest of the world.

So in what sense are natural kinds, being mind-dependent (or practice-dependent), still real in Boyd's account? Boyd argues that although natural kinds are the

[24] In his early writings, i.e., before he formulates clearly the accommodationist thesis of natural kinds, Boyd argues that a realists should embrace the *no noncausal contribution doctrine*, which holds that "the adoption of theories, frameworks, paradigms, projects, intellectual or practical interests, and so forth, makes no non-causal contribution to the causal structure of the world scientists study" (Boyd 1990, 183; see also Boyd 1991).

"workmenship of women and men", it is incorrect to say that the boundaries between them "are as Men, not as Nature makes them" (Locke 1690). On the contrary, Boyd suggests that "accommodation" is "bicameralist" in nature, as "the (causal structure of the) world [also] plays a heavy legislative role" in the definition of natural kinds. Now the question is: what exactly is this relation of "accommodation", which is "intended to capture the basic realist element in the naturalist realist conception of natural kinds" (Boyd 1999b)?

Similar to Khalidi's, Boyd's realist account of natural kinds is anchored on a naturalist answer to the question of *naturalness*, which equally capitalizes on the epistemic reliability of natural kinds in delivering successful scientific practices such as explanation, induction, and prediction. While Khalidi, as we have seen, does not fully elucidate his "realist stance" towards natural kinds, Boyd introduces the notion of "accommodation" to account for why the epistemic reliability of natural kinds serves as a hallmark for their reality. According to Boyd, the naturalness of natural kinds "consists in a certain accommodation between the relevant conceptual and classificatory practices and independently existing causal structures" (Boyd 1999b). Yet, for Boyd, "accommodation" is not just a relation between natural kinds and the causal structure of the world. Instead, "accommodation" designates a relation between the "inferential architecture" of a given "disciplinary matrix" and the relevant causal structures of the world.[25] "Inferential architecture" encompasses a wide range of

[25] The term "disciplinary matrix" is originated from Kuhn (2012). According to Kuhn, the term "disciplinary matrix" is chosen to replace the term "paradigm" in his original account of scientific revolution. By "disciplinary matrix" Kuhn means something more encompassing than a paradigm, which is now used to refer specifically "concrete problem-solutions that students encounter from the start of their scientific education" (Kuhn 2012, 186). Apart from these "exemplars", a disciplinary matrix of a given scientific community also includes: (1) "symbolic generalization", i.e., "formal or readily formalizable components of the disciplinary matrix" (Kuhn 2012, 182); (2) "the metaphysical parts of paradigm", i.e., "beliefs in particular model", which supplies the community with "preferred or

activities in a scientific community such as perceptual, cognitive, behavioral, classificatory, and referencing practices. Moreover, an "inferential architecture" is embedded in a "disciplinary matrix", which comprises "a family of inductive and explanatory aims and practices, together with the conceptual resources and vocabulary within which they are implemented" (Boyd 1999b, 7).

While it is difficult to neatly list out all the different components that constitute the inferential architecture of a given disciplinary matrix,[26] the epistemic aims of a given disciplinary matrix play a key role in successful accommodation. As Brigandt contends (Brigandt 2011a, 175),[27]

> Most importantly, the empirical considerations pertaining to a kind include not only empirical properties of the kind's members, but also the *epistemic-scientific aims* that can be met by using the kind in scientific theorizing. These features matter for scientific practice and theory, so that a naturalistic approach must take them into account.

In acknowledging that the classificatory practices in a disciplinary matrix are tied to the specific epistemic-scientific aims they may serve, Boyd's accommodationism at

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permissible analogies and metaphors" as well as determines "what will be accepted as an explanation and as a puzzle-solution" (Kuhn 2012, 183); and (3) "values" such as accuracy, consistency, margin of permissible error (Kuhn 2012, 183). While one may easily infer a pluralist picture based on the fact that different disciplinary matrixes are likely to endorse different classifications, what is of particular importance here is the component "values" in a disciplinary matrix. As Kuhn suggests (Kuhn 2012, 185),

> though values are widely shared by scientists and though commitment to them is both deep and constitutive of science, the application of values is sometimes considerably affected by the features of individual personality and biography that differentiate the members of the group.

[26] In fact, Boyd also admits, "accommodation covers cases of the epistemically valuable hunches and trained methodological judgments that you go to graduate school to acquire (the inexplicit parts of what Kuhn calls 'paradigms')" (Boyd, forthcoming).

[27] Along the same lines, Brigandt develops an account that separates three different components of a scientific concept (or theoretical entity), namely, its reference, its inferential role as well as the epistemic goal pursued with the concept's use (Brigandt 2011a). According to Brigandt, "the epistemic goal pursued by a scientific concept's use is the type of knowledge (certain kinds of inferences, explanations, discoveries) the concept is *intended* to deliver, given its usage by a research community" (Brigandt 2011a, 177, italic original). And one important reason for introducing the notion of "epistemic goal" to the traditional idea of a concept is to account for the rationality of concept change in a scientific investigation. "Epistemic goals" is therefore a much broader concept than Khalidi's "pure epistemic purposes"—it includes purposes that may not be aiming to reveal the causal structure of the world. For instance, groupings that serve the purpose of inference may only correlate with each other, without the correlations being underwritten by causal relations, as it is required to satisfy Khalidi's "pure epistemic purposes".

the same time abandons the view that these practices always aim to achieve a universal, "pure" epistemic goal, such as tracking the causal patterns of the world, as Khalidi maintains. While a particular scientific-epistemic goal may determine what classification scientists would adopt in a given disciplinary matrix, what scientific-epistemic goals may deem reasonable or realistic also depends on what inferential and inductive resources are available in the disciplinary matrix in question. And of course, these resources are provided by the background theories that make up the disciplinary matrix.

As a result, Boyd adds a contextual spin to the notion of naturalness. In the "accomodationist" framework, the *naturalness* of a kind is no longer determined exclusively by any single epistemic parameter such as *projectibility*. On the contrary, the naturalness of a given kind is "relative to the role reference to it [i.e., a given natural kind] plays in a disciplinary matrix" (Boyd 1999b)[28]. Thus although Boyd still refers to the *projectibility* of natural kinds in explicating their epistemic reliability in a given disciplinary matrix, his accommodationist framework actually entails a more radical pluralist picture than Khalidi's account. Even if *projectibility* may be regarded as a crucial feature of scientific categories in general, different disciplinary matrices are going to posit different sets of scientific categories. This is because, given their unique epistemic agendas, different disciplinary matrices are likely to endorse different standards of *projectibility*. Hence, natural kinds are disciplinary-oriented. As

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[28] It is worth quoting the passage in length (Boyd 1999b, emphasis original),

> Thus the fundamental notion in the theory of theoretical natural kinds is not the notion of such a kind, *simpliciter*, but instead the notion of a kind's being natural with respect to a particular *inferential architecture*. When we talk simply of a natural kind, or of natural kinds generally, there is either tacit reference to some inferential architecture or tacit quantification over some domain of them. At least in the case of natural kinds in the sciences, that inferential architecture can best be thought of as being provided by a *disciplinary matrix*: a family of inductive and explanatory aims and practices, together with the conceptual resources and vocabulary within which they are implemented. The naturalness of a scientific natural kind is relative to the role reference to it plays in a disciplinary matrix.

Boyd puts it, "[n]atural kinds in chemistry need not be natural kinds in geography" (Boyd forthcoming).

Boyd's accommodationist account of natural kinds therefore provides a blueprint for the more radical version of taxonomic pluralism I mentioned above. This is because, taking into consideration the multifarious epistemic aims of scientific classifications, it embraces a more flexible reading concerning the epistemic role of natural kinds. Indeed, this contextual reading of the *naturalness* of natural kinds also offers a more accurate and realistic picture of classificatory practices in natural science than the modest pluralist account described by Khalidi. But the question is: does it yield a *realist* account of natural kinds?[29]

VIII. Problems of Boyd's natural kind realism

Recall that according to Boyd's "accommodationism", scientific categories are part of a more encompassing "inferential architecture". Moreover, "inferential architecture" is disciplinary-oriented. Under these two presumptions, the realism of natural kinds should no longer be construed in terms of a direct relation between natural kinds and the divisions of the world (or "nodes in causal networks", as suggested by Khalidi). On the contrary, according to Boyd's "accommodationism", the relation between our scientific categories and the world is mediated by the "disciplinary matrix" these categories belong to. As Boyd puts it, "questions about the reality of (alleged) natural kinds should always be understood as questions about the suitability of those kinds for induction and explanation *in particular disciplinary matrices*" (Boyd 2010b, 222, italic original). Whether our scientific categories "fit" the relevant causal structures of the world ultimately depends on various factors of a given disciplinary matrix, such as

[29] One issue is whether the multiple classifications posited by different disciplinary matrix are truly commensurable and genuinely disagree with each other. I will examine this issue in Chapter 3.

the scientific-epistemic aims of the disciplinary matrix, the inferential resources available, and the methodological standards adopted by the scientific community in question. As Boyd repeatedly points out, the judgment of *projectibility* of our scientific categories, or more generally, the degree of accommodation between an "inferential architecture" and the relevant causal structures of the world, irretrievably depends on the background theories that make up the disciplinary matrix in question (Boyd, 1989, 1990, 2010, forthcoming).

With the mediation of a disciplinary matrix, it seems that successful "accommodation" of its inferential architecture to the relevant causal structures does not require scientific categories to correctly represent the causal structures in question. In other words, "accommodation" may not be truth-indicative. For instance, although direct correspondence between the scientific categories of a given disciplinary matrix and the divisions of the world is likely to increase the degree of accommodation of its inferential architecture to the relevant causal structures, it is by no means necessary for successful accommodation. As Boyd suggests,[30]

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[30] It should be noted that Boyd, in his earlier works (e.g., Boyd 1988, 1990, 1999) espouses a correspondence theory of truth in spelling out natural kinds realism (or scientific realism in general). For instance, he points out that "[t]ruth (about natural kinds, causal relations and the other fundamental subjects of science) is *correspondence* truth—socially constructed truth won't do" (Boyd 1999b, 4, emphasis original). More importantly, as Boyd affirms, "the achievement of knowledge of approximate correspondence truths is central to that accommodation" (Boyd 1999b). However, in the same work, he also writes,

> As I propose to use the term [i.e., "accommodation"], there may be basically successful disciplinary matrices not all of whose accommodation demands can be satisfied: for some of the explanatory or inductive aims of such a disciplinary matrix there might not exist in the world the sorts of causal structures which could sustain the sought after generalizations or explanations (Boyd 1999b).

In fact, in his later works (e.g., Boyd 2010b, forthcoming), Boyd does not only point out repeatedly that that truth, successful reference, and even partial denotation of kind terms are mere special cases, he also, more importantly, suggests that "comparable insights and contributions to accommodation have been made by the methodological role of beliefs and statements whose contribution cannot be explained in terms of a Tarski-style conception of reference and of truth or approximate truth" (Boyd, forthcoming). As we are going to see below, this shift away from correspondence truth in his account of natural kinds is not only consistent with its contextualist tone, but also, to a certain extent, inevitable within the "accommodationist" framework. Indeed, in his earlier writing, Boyd briefly suggests that failure of reference is acceptable in science. Nevertheless, he was yet to put forward his "accommodationist thesis" at that time. For more, please see footnote 35 below.

> The key idea is that successful reference to natural kinds is a special case of epistemically fruitful alignment or *accommodation* between perceptual, instrumental, cognitive, and representational practices, on the one hand, and inductively, practically or explanatorily relevant causal features of the world.

Boyd uses the German "idealist morphology" as a historical example. According to Boyd, although many details of the German "idealist morphology" (e.g., the Hegelian idealist conceptions of beings) were "laden with non-referring idealist terms and concepts, and so lacked truth conditions altogether", the theory still contributed to the development of evolutionary thought, which is essentially naturalistic and materialistic (Boyd, forthcoming). This is because in their own disciplinary matrix, these "non-referring idealist terms and concepts" enable the accommodation of the scientists' inferential practices to biologically important causal phenomena.

It is understandable why successful "accommodation" would tolerate non-referring terms and concepts.[31] In fact, such tolerance is necessary. As we have seen, Boyd's "accommodationism" recognizes a more liberal notion of epistemic aims than Khalidi's account. For Boyd, such epistemic aims are not confined to tracking the causal patterns of the world, as Khalidi believes. There is no reason to assume that accomplishing aims that fall under this more liberal notion of epistemic aims always requires our scientific categories to represent the causal structures of the world, or stand for "nodes in causal networks". Indeed, what "structures of the world" are we

[31] Apart from the shift away from correspondence truth, another change in Boyd's "accommodationist" thesis of natural kinds is that he focuses more on the semantic aspect of accommodation in his later writings. Thus rather than taking "accommodationism" as a realist theory of natural kinds, he refers to it as a "semantics for scientific realism" (Boyd, forthcoming). Instead of discussing whether our scientific categories accommodate to the structure of the world, he emphasizes more how natural kind terms refer to natural kinds. Yet, Boyd also points out, "[a] natural kind is nothing (much) over and above a natural kind term together with its use in the satisfaction of accommodation demands" (Boyd 1999b, 16). Therefore, in the following, I will use "natural kinds" and "natural kind terms" interchangeably when I refer to scientific categories within the "accommodationist" framework.

talking about here, when there is no particular structure these classifications aim to capture?

Let us consider the Phylo-Phenetic Species Concept (PPSC) in microbiology as an example again. As Ereshefsky & Reydon suggests, classification according to the PPSC is not intended to trace the causal mechanisms of the world. It is chosen because it allows ready and stable identification of microbiological species. Of course, this is not to say that the causal mechanisms of the world are not involved to make possible such ready and stable identification, as Boyd (forthcoming) points out in his defense against the criticism of Ereshefsky & Reydon (2015)[32]. Yet, if Ereshefsky & Reydon are correct about the motivation of the PPSC classification, then it would be mistaken to regard the PPSC categories as standing for the relevant causal structures. Indeed, once we allow the epistemic agendas that do not aim at uncovering the causal structures of the world to bear on the *naturalness* of scientific categories, "accommodation" or "fit" can at most signify the *world-dependence* aspect of these categories. Therefore, Boyd contends that we need a "broader conception of accommodation and representation". Moreover, Boyd believes that this "broader conception of accommodation and representation", in treating "reference, partial denotation and truth as special cases of accommodation[,] affords realists a more promising option" (Boyd, forthcoming).

However, it is unclear whether this "broader conception of accommodation and representation" really affords a *realist* option for taxonomic pluralism. If successful "accommodation" does not require kind terms to have a reference—worse

[32] Ereshefsky & Reydon (2015)'s criticism is not about the realism of natural kinds in the accommodationist framework. Instead, it is directed at Boyd's account of biological species, understood as Homeostatic Property Cluster (HPC) kinds. According to the HPC theorists (e.g., Boyd, Brigandt, Wilson), natural kinds, in particular *species*, are groups of individuals that share similarities stabilized by homeostatic causal mechanisms. Ereshefsky & Reydon question whether a singular causal relation is necessary for the homeostatic mechanism. More will be said about the HPC kinds in the next chapter.

still, if successful "accommodation" sometimes even depend on empty kind terms, then it seems that many superseded or obsolete scientific frameworks, such as the phlogiston theory, the caloric theory of heat, and the luminiferous aether theory of light, can be regarded as examples of successful accommodation. Despite the fact that kind terms like "phlogiston", "caloric", and "aether" do not refer, they do contribute significantly to successful inferential practices in their own contexts of inquiries or disciplinary matrices. Yet, no natural kind realist would be happy to admit that kinds like *phlogiston*, *caloric*, and *aether* are *real* kinds. If, according to "accommodationism", scientific categories that contribute to the accommodation of the inferential architecture to the relevant causal structures of the world are not necessarily real kinds, then "accommodationism" fails to provide a defensible distinction between real kinds and non-real kinds. However, this distinction is crucial to natural kind realism.[33]

My doubt here is not whether the epistemic reliability of our scientific groupings is sufficient to vindicate that they "accommodate" to the relevant causal structures of the world. Nor am I complaining that a sort of "relativism" may creep in when the door is opened to multifarious epistemic aims and interests. These *epistemic* issues are of course important. The issue I am concerned with is whether the scientific categories of an inferential architecture that "accommodates" to the relevant causal structures of the world would reliably inform us anything about such structures, given that such structures are supposed to play a "heavy legislative role" in a relation of

[33] One cannot simply reply my objection here by saying that I cast too wide a net for "epistemic aims" that scientific classifications are supposed to fulfill. Not only because it is difficult to clearly delimit the epistemic from the non-epistemic, but also, as we will see in Chapters 3 and 4, a liberal understanding of epistemic aims is central to many prominent accounts of taxonomic pluralism in the current literature of natural kinds.

"accommodation", as the "bicameralist" thesis suggests.[34] Again, as is behind my objections to Khalidi's pluralist account, my more deep-seated worry about Boyd's "accommodationism" is a *conceptual* one: does "accommodation" secure a tenable distinction between realism and antirealism about natural kinds?

IX. What should a natural kind realist be realist about?

In order to understand why Boyd's "accommodationism" fails to provide a defensible distinction between realism and antirealism about natural kinds, first we need to be clear about what a natural kind realist should be *realist* about.[35] Consider the debate between moral realists and expressivists. The former believe that there are moral facts that govern whether moral judgments (e.g., *killing is wrong*) are true or not. The latter, being antirealist about moral facts, contend that moral judgments only express the attitudes of moral agents. Nevertheless, even though expressivists deny that there are moral facts that determine the truth-value of moral judgments, they can still maintain that there are facts of the matter concerning the moral attitude conveyed in a given moral judgment. For example, moral expressivists can debate whether someone, in claiming that "killing is wrong", *really* holds a disapproving stance towards the act of killing (Fine 2001, 23). But this by no means implies that moral expressivists are

[34] It should also be noted that my complaint here does not assume that the reality of scientific categories can only be vindicated when they directly correspond to the natural-kinds structures of the world. In dismissing mind-independence as a criterion for the reality of natural kinds, we should not appeal to direct correspondence as a reference for their reality. Indeed, as Boyd indicates, one advantage of the accommodationist framework is that it avoids the "idealist (or theistic) metaphysics involving something like a pre-established harmony between human thought and other features of the world" (Boyd, forthcoming), which is commonly assumed in the mind-independence reading of reality.

[35] Interestingly, in an earlier writing on scientific realism entitled "Realism, Conventionality and 'Realism about'"(before he formulated his "accommodationist" account of natural kinds), Boyd has already proposed a "realist" account that is very tolerant to reference failure, as he writes,

> the realist, in portraying methodologically central theories as relevantly approximately true, need not treat all of their constituent terms as (even partially) referring. What she must do is to portray them as being approximately true in respects suitable to explain the reliability of the methods they underwrite. (Boyd 1990, 187)

realists about moral facts, such as *killing is wrong*. Similar situation can be found in the debate concerning the ontological status of mental properties such as phenomenal qualia. Although physicalists would admit that there are facts of one's neurophysiological states in determining whether one is in a particular mental state or not, it is mistaken to conclude that they are as realist as property dualists about phenomenal qualia.

It is therefore unclear to what extent Boyd's account is truly realist about *natural kinds* when he writes the following in explaining why natural kinds and their definitions are real, despite being construed as discipline-or-practice relative,

> In theories about the cognitive, perceptual and behavioral ecology of other species, like Belding's ground squirrels and vervet monkeys, no departure from realism is implicated by reference to the cognitive, perceptual or motivational states of the organisms in question. The organisms and their cognitive, perceptual or motivational states are real and causally efficacious so there's nothing anti-realist about referring to them. The same is true for any theory about the human case…

Brigandt also espoused a similar "realist" stance in his account of biological species taxa, which is based on Boyd's Homeostatic Property Cluster (HPC) theory of natural kinds (Brigandt 2009, 86),

> [T]he HPC… view agree[s] in endorsing some sort of realism about taxa, at least species taxa. Natural kinds are assemblies of objects that are grouped according to properties that actually exist in nature, so that the boundary and unity of a natural kind are not conventional.

Nevertheless, even if it is true that our "cognitive, perceptual or motivational states are real" and things are "grouped according to properties that actually exist in nature", it still does not mean that the categories posited by the classification in question are real. Otherwise, we have to admit that kinds like *phlogiston*, *caloric* and *aether* are real as well, for many of the observational features that are believed to confirm the reality of these unreal kinds, e.g., combustion, weight, temperature, as well as propagation of light, are *real* as well. Therefore, if this is what realism is all about in Boyd's "accommodationism", then successful "accommodation" at most signifies that the groupings in question are world-dependent. Yet, the feature of world-dependence is by no means sufficient to differentiate real kinds from unreal kinds.

Why? This is because an antirealist would not concede that since theoretical groupings are not real, they are all based on "unreal" properties or properties that do not exist in nature. An antirealist can agree, on the one hand, that some groupings are more natural than the others, and on the other hand, that these natural groupings are based on real properties, without at the same time assigning any privileged ontological status to these groupings. Having individuals grouped together according to properties they actually possess is one thing; how these properties are selected in constructing a classification is another thing. Thus, one can be a realist about properties without being a realist about *kinds*, just as one can be a realist about particulars or tropes, without at the same time being a realist about universals.

The realist commitment captured by Boyd's "accommodationism" is therefore trivial: the world has certain causal structures, and our theoretical groupings are successful because they "accommodate" to the relevant causal structures. Yet just by saying that there exists such a relation between our scientific classifications and the relevant causal structures of the world, without explicating what this relation is, Boyd's "accommodationism" is not robust enough to capture a realist position about *natural kinds*. As Chakravartty points out, a "significantly more robust (and resultantly, more plausible) understanding of realism" requires more than just "Ramsey-sentence realism" (Chakravartty 2011, 167).[36]

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[36] Take the Ramsey sentence of a theory T as: $\exists x_1 \ldots x_n\, T\,[x_1 \ldots x_n]$. The Ramsey sentence suggests that the given theory T is actually realized by the n-tuple of entities denoted by the theoretical terms it postulates. Thus we can rewrite T as $T\,[\tau_1 \ldots \tau_n]$, with the occurrence of the theoretical terms (i.e., $\tau_1 \ldots \tau_n$) posited by the theorey in question. Accordingly, Ramsey-sentence realism merely states that the

Indeed, natural kind realism is not only concerned with whether our natural classifications have a real basis, but what that basis *is*. Being realist about a kind *K* is more than just being realist about the individual members of *K* or being realist about the properties according to which these individuals are being grouped together into *K*. What is crucial is whether there is a *real* basis for selecting a particular set of properties that determines the membership in *K*. Whether we are going to agree with the details of Mill's account of natural kinds, his formulation is illuminating here. As Mill points out, apart from encompassing a wide range of common features, a natural kind should also identify properties that are the causes of these common features, i.e., properties in virtue of which members of that kind regularly possess these common features. Consider the kind *white cubic thing*. Even it groups things according to "properties that actually exist in nature", namely *being white in color* and *being cubic in shape*, we are not going to admit that the grouping *white cubic thing* is a real kind. This is because there does not seem to exist any real linkage between the two properties in question, namely *being white in color* and *being cubic in shape*.[37]

All in all, realists about natural kinds are not engaging in a debate with idealists. The philosophical debate between realists and anti-realists about natural kinds would still arise even among theorists who concur on the reality of the world, or the reality of its causal structures. Indeed, in the context of this discussion, the reality of the world as well as its causal structure(s), are taken for granted, as Boyd

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theoretical entities postulated by *T* are real, without specifying what they are. But it is precisely with the question of what these theoretical entities are that *natural kind realism* is concerned.

[37] I want to clarify two points here. First, although it is unlikely that the grouping *white cubic thing* would turn out to be a natural kind, it is still a kind. This is because it represents a collection of things being grouped together according to its two associated properties, namely *being white in color* and *being cubic in shape.* Second, what I try to show here is not that the grouping *white cubic thing* is not a natural kind or that it is not a real kind. Instead, the point I am driving at is that even though its two associated properties (again, *being white in color* and *being cubic in shape*) are real, they are still not *sufficient* to vindicate the reality of the *kind* itself.

repeatedly points out, accommodation is a relation between our inferential architecture and the "independently existing causal structures". The debate between realists and anti-realists about natural kinds should be concerned with a more specific question, namely whether or not *natural kinds*, understood as categories that underwrite successful scientific practices, are real. Thus to be realist about natural kinds is not just to uphold that natural kinds are world-dependent, but also to uphold that *natural kinds* are *real*. In this regard, Boyd's "accommodationism" fails to provide an adequate characterization of this realist commitment.

Perhaps this failure is an inevitable outcome when we adopt a naturalist answer to the question of *naturalness*, which identifies natural kinds as groupings that underwrite successful scientific practices such as explanation, induction, and prediction. Yet, taking into consideration the manifold epistemic aims scientists try to accomplish in different investigative contexts, pluralists have to adopt a liberal notion of natural kinds. It seems that, as a consequence, pluralists also have to adopt a liberal notion of reality. As we have seen, such a liberal notion of reality is not robust enough to distinguish real kinds from unreal kinds. If this turns out to be the case, however, then pluralists should embrace anti-realism about natural kinds, instead of a deflationary notion of realism. For such a deflationary notion of realism fails to provide a meaningful distinction between realism and antirealism about natural kinds. But this is not the case—I am going to elucidate how the realist commitment of natural kinds realism should be construed in the next chapter.

X. Conclusion

In this chapter, I put forward a pluralist-friendly schema of natural kinds. This schema adopts a naturalist answer to the question of *naturalness*. Natural kinds are *natural* because they are sanctioned by natural science. Therefore, natural kinds are identified as categories that underwrite successful scientific practices such as explanation, induction, and prediction. Given that taxonomic pluralism is supposed to be a *realist* alternative to taxonomic monism, I also demonstrated why this pluralist-friendly schema should dismiss mind-independence as a relevant criterion in evaluating the reality of natural kinds. I looked for a substitute for this traditional construal of reality in two different accounts of taxonomic pluralism, namely the modest pluralist account of Khalidi and the more radical pluralist account of Boyd. I contended that both accounts fall short of offering a defensible notion of reality for the realist commitment of natural kind realism. In the case of Khalid, the concept of *naturalness* is too narrow as it is based on a dubious distinction between the epistemic and the non-epistemic. In the case of Boyd, the concept of *naturalness* is too broad to offer a defensible distinction between real kinds and unreal kinds.

Chapter 2: Natural Kind Realism, Irreducibility and Relative Fundamentality

I. Introduction

In the previous chapter I argued that we should reject the traditional reading of realism, which construes what is real in terms of being mind-independent. This is because, when being applied to natural kinds, this traditional reading of realism would dismiss as unreal a lot of mind-dependent scientific categories that taxonomic pluralists use as empirical evidence to support their position. As a result, a realist account of natural kinds that construes their reality in terms of mind-independence fails to fully represent taxonomic pluralism, which is supposed to be a realist position about natural kinds as well. In order to give taxonomic pluralists the benefit of the doubt, I think that not only we should abandon the mind-independence criterion of reality, but we should also adopt a substitution that complies with taxonomic pluralists' *naturalist* stance towards natural kinds (or more precisely, their naturalist answer to the question of *naturalness*). According to this naturalist stance, natural kinds are groupings that underwrite successful scientific practices such as explanation, induction, and prediction. Apparently, such epistemic roles of natural kinds provide us reason to believe that natural kinds are real. Yet, as I demonstrated, the modest pluralist account of Khalidi and the radical pluralist account of Boyd both fail to offer a defensible substitution for the mind-independence criterion, precisely because of their naturalist stance towards natural kinds.

In this chapter, I am going to undertake this unfinished business and put forward a substitution for the mind-independence criterion of reality. Paying attention to the limitation of the mind-independence criterion, as well as the mistakes of Khalidi's and Boyd's accounts, I suggest that we should construe the reality of natural kinds in terms of *irreducibility*. In order to understand in what sense natural kinds are *irreducible*, hence *real*, we have to answer two questions. First, *to what* are natural kinds irreducible, therefore real? In other words, what are the *relata* of this relation of reduction that we appeal to in spelling out the reality of natural kinds? Second, what exactly is this *relation* of reduction?

Before I put forward my answers to these two questions, in the coming section, I will present the *prima facie* reasons why we should formulate the reality of natural kinds in terms of *irreducibility*. Since the notion of reduction is notoriously ambiguous, in order to narrow down my focus, I will first contrast different notions of reduction. So in section III, I will compare the notion of reduction embodied in natural kind realism and the notion of reduction embodied in taxonomic pluralism. Then in section IV, I will compare the *eliminative* reading and the *conservative* reading of (ontological) reduction, and explain why the *eliminative* reading is more appropriate to our task, i.e., the task of formulating natural kind realism. In section V, I will present the inherent paradox of *eliminative* reduction. And in section VI, I will examine how Quine's notion of ontological commitment may resolve this paradox. In section VII, I will explain why Quine's (or the neo-Quinean) conception of ontological commitment fails to capture the realist commitment of natural kind realism. Next, in section VIII, I will pursue a non-Quinean approach, which emphasizes the idea of *relative fundamentality* in construing the notion of ontological reduction. I will go on to expound the details of this non-Quinean approach in section IX and elucidate how we should revise the notion of eliminative reduction accordingly in section X. Finally, I will answer a potential difficulty this non-Quinean approach may face.

II. Irreducibility: the *prima facie* reasons

Understanding the reality of natural kinds in terms of irreducibility is not an *ad hoc* proposal. When I indicated in the previous chapter that the mind-independence criterion fails to align with our philosophical understanding of reality, I have already alluded to the notion of *irreducibility*. Let me recap the two examples here. In the debate about the reality of mental entities, such as phenomenal qualia, both dualists and physicalists agree that these mental entities are mind-dependent. Yet, dualists and physicalists disagree about whether mental entities can be *reduced* to physical entities. Therefore, being mind-independent is not necessary for being real. Similarly, in the debate about the reality of ordinary material objects such as a rock, its alleged mind-independence is not sufficient to vindicate its reality. Philosophers like van Inwagen and Merricks argue that ordinary material objects are not real since they have no causal role to play in the physical world. This is because everything that can be explained by referring to a given material object can be completely explained by referring to the particles that compose it. In other words, according to van Inwagen and Merricks, ordinary material objects are not real because they can be *reduced* to the particles that compose them. These two examples show that what is crucial in the philosophical debate concerning the reality of an entity *X* is not whether *X* is mind-independent. Instead, a more general criterion is in play, namely *irreducibility*.

Apart from providing a more encompassing and general notion of reality than mind-independence, *irreducibility* also spells out the notion of reality in more basic terms than mind-independence. An explanation of *X*'s reality based on the fact that *X* is mind-independent can in principle be replaced by an explanation appealing to the fact that *X* is *irreducible*. But the reverse does not hold: being mind-independent does not explain why something is irreducible.

In fact, the notion of *irreducibility* is central to the debate between taxonomic monism and taxonomic pluralism. On the one hand, in arguing that different ways of classifying things into natural kinds are needed to satisfy the multifarious epistemic pursuits of the scientists in a given scientific domain, taxonomic pluralists maintain that these classifications are *irreducible*, i.e., they cannot be reduced to a single classification. On the other hand, taxonomic monists contend that there is a single correct classification that all these different classification can be *reduced* to. We should notice that this is not simply a debate among natural kind realists concerning whether a set of natural kinds constitutes a single or multiple classificatory frameworks. Deep down, it is a debate concerning the *reality* of those kinds that cannot be subsumed or integrated into a single classification. For taxonomic monists, these kinds cannot all be real for there is only one correct classification. Thus some of them just appear to be real, they can actually be *reduced* away. For taxonomic pluralists, these kinds are all *really* real: they cannot be *reduced* away. Thus it seems that the notion of *irreducibility* has already been adopted by taxonomic monists and taxonomic pluralists (who are both natural kind realists) in their debate, which is a debate concerning the reality of natural kinds.

III. Irreducibility: natural kind realism vs. taxonomic pluralism

However, this notion of irreducibility, as espoused by taxonomic pluralists, should not be confused with the one that is required to establish natural kind realism. Taxonomic pluralists argue that multiple classifications are needed to fulfill the diverse epistemic aims scientists have in different investigative contexts. Thus, natural kinds posited by these multiple classifications are irreducible in the sense that they are necessary for successful scientific practices such as explanation, induction, and prediction. For instance, Magnus (2012) contends that natural kinds are not just groupings that support successful science. To this "success clause", he adds a "restrictive clause": natural kinds are *indispensable* to the success of science. Irreducibility so understood becomes an *epistemic* notion. It specifies the *naturalness* of natural kinds. In other words, it tells us which groupings are natural kinds, namely those that are indispensable for successful scientific practices.

What we are concerned with, however, is the question of *reality*: in what sense are natural kinds, understood as groupings that (indispensably) underwrite successful scientific practices, *real*? Although the epistemic significance of natural kinds provides us a good reason to believe that they are real, it does not tell us how the reality of natural kinds should be construed. Both Khalidi's account, which focuses on "pure epistemic purposes", and Boyd's account, which focuses on the relation of "accommodation", fail to provide a satisfactory answer to this ontological question, despite the fact that they both emphasize on the epistemic reliability of natural kinds. In fact, without an appropriate conception of reality, it is impossible for us even to draw out the ontological implications from the fact that natural kinds are epistemically irreducible. Therefore, the notion of reduction we should be concerned with is *ontological reduction* rather than epistemic reduction: natural kinds are real if and only if they are *ontologically irreducible*.[1]

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[1] While it may appear that epistemic (or intertheoretic reduction) is prior to ontological reduction, whether epistemic reduction has any ontological import is a largely controversial issue. As one may rightly argue, the items being linked up in epistemic reduction are our representations of reality; whether reduction holds between these representations is one question, and whether reduction holds between what they represent in the world is another question. After all, answer to one does not guarantee the answer to the other. A case in point would be the debate regarding the status of the special sciences. One may concede that, for example, the subject matter of psychology (e.g., mental states and intentionality) is part of, or can ultimately be reduced to the subject matter of chemistry or biochemistry (e.g., neurotransmission), without at the same time agreeing that psychological theories can be reduced to physical theories. This is because one may maintain that theoretical terms in psychology have an explanatory role that cannot be performed by theoretical terms in physics. If this is the case, then epistemic irreducibility does not serve as a reliable indicator for the ontological status of the entities in question. More radically, one would argue that we should jeopardize the notion of

Furthermore, even if we construe the claim of irreducibility made by taxonomic pluralists in ontological terms, the relata involved would still be different from the relata involved in the claim of irreducibility that is required to illustrate natural kind realism. In asserting that *groupings* posited by one classification cannot be replaced by *groupings* posited by another classification, what taxonomic pluralists have in mind is kind-kind reduction. While taxonomic monists argue that only those groupings belonging to *the* correct classification are real, taxonomic pluralists contend that kinds posited by these different classifications are all real. The debate between taxonomic monists and taxonomic pluralists thus revolves around *which* set(s) of groupings is (are) real. On the contrary, the debate between realists and anti-realists about natural kinds is not about whether any particular set of natural kinds is real, or whether one set of natural kinds can be replaced by another set of natural kinds. It deals with a more general question, namely whether or not natural kinds *themselves* are real. So, for natural kind realists, what is at stake is not kind-kind reduction, but kind-*non-kind* reduction. If natural *kinds* can be completely reduced to something of a

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ontological reduction in natural science. One may believe that whether and how a theoretical model should be simplified and revised through epistemic reduction is a pragmatic matter depending on the given epistemic task; and ontological issues will only distract us from these important questions. For example, Bickle (2003) contends that we should simply replace the "murky" notion of ontological reduction with scientific, i.e., intertheoretic reduction. McIntyre (2007) argues that reduction and emergence between chemistry and physics should be taken as purely epistemic, since an ontological interpretation is often misleading. Similarly, Hendry (2010) argues that the failure of epistemological or intertheoretic reduction is independent of any metaphysical relation between the theories in question. On the contrary, Le Poidevin (2005) argues that ontological reduction of chemical properties to the more fundamental physical properties can be defended independently of epistemological reduction. Hence, we should separate ontological reduction from epistemic reduction, since the latter does not contribute to the former. Similarly, in philosophy of biology, the debate concerning the reduction of classical genetics to molecular genetics is predominantly epistemic; the ontological question, namely whether the Mendelian gene can be reduced to DNA, is considered to be irrelevant. Wimsatt (2006) also argues that the traditional metaphysical conception of reduction does not align with the actual practice of science. Wimsatt indicates that reduction in actual practice of science is methodological in nature; it aims to provide heuristics for problem solving. Kaiser (2015, 57) points out that the majority of authors in the philosophy of biology do not address the ontological issues in their debates of reduction(ism). Likewise, in replying to those who believe that reductionism is in principle possible in biology, Brigandt (2011b), Brigandt and Love (2017) argue that this ontological question is not relevant to a philosophical understanding of biology in practice. We must not lose sight of the fact that these views all count towards the recent "turn to practice" campaign in the philosophy of science, which maintains that speculative metaphysical principle has no role to play in actual scientific research (Kellert *et. al* 2006). I will say more about the "turn to practice" approach in the next two chapters.

different nature, then it seems that we have good reason to claim that natural kinds are not real. But what does it mean by saying that natural kinds cannot be so reduced?

IV. Ontological reduction: eliminative vs. conservative

In proposing that natural kind are real because they are *irreducible*, it seems that I am appealing to a notion of ontological reduction that has an antirealist import, or an "eliminative" reading of ontological reduction, as it is commonly called. According to the eliminative reading, if an entity *X* can be ontologically reduced to another entity *Y*, then *X* can be replaced by *Y* and be eliminated from our ontology. For example, in saying that *polywater* is being reduced to *ordinary water containing impurities from improperly washed glassware* (Railton 1993, 161), we eliminate *polywater* from our ontology. Paul Churchland (1981, 1985) and Patricia Churchland (1986) rely on this eliminative reading of reduction in arguing that neuroscience eliminates psychology, a project that Bickle (2003) continues to pursue in his "new wave reductionism".

However, there is also a diametrically opposite reading of ontological reduction, namely the "conservative" reading (sometimes also called "vindicative" or "retentive" reading). Instead of being eliminated or gotten rid of, the conservative reading suggests that the reality of an entity is conserved by being ontologically reduced to its (physical) base.[2] Accordingly, the fact that *heat* is "reduced" to *mean molecular kinetic energy* proves exactly that *heat* is real, in the sense that there is nothing fictitious or mysterious it: *heat* ***is*** *mean molecular kinetic energy*. On the contrary, the reality of *caloric fluid* cannot be vindicated precisely because it cannot be so "reduced": there is no such (physical) thing as *caloric fluid* that flows from

[2] In the following, when I refer to the item being reduced in a reduction, i.e., *X* as in *X is ontologically reduced to Y*. I will switch freely between terms like "the reduced entity", "the old item", etc., leaving open as to whether *X* is an object, a property or an event. Likewise, apart from calling *Y* "the reduction base", "new item", etc. depending on the context, I will also call it the "reducing entity", without suggesting that it is *Y* that *actually* carries out the reduction.

hotter bodies to colder bodies. This conservative reading is endorsed by non-reductive physicalism (Sklar 1967, van Gulick 2001), as well as the accounts of Esfeld & Sachse (2011) and van Riel (2014). While the two readings appear to be incompatible with each other, the conservative reading seems to be more popular in the recent literature of the philosophy of science and the philosophy of mind, partly due to the demise of eliminative materialism. As Kim also points out, "a *more central form of reduction* is "conservative" (or "preservative", "retentive") reduction whereby the reduced phenomena *survive as legitimate entities of the world*" (Kim 2007, 94-95, emphasis mine).

So why do I prefer the eliminative form to the "more central", conservative form of reduction in formulating natural kind realism? In order to answer this question, it is necessary for us to see in what sense both forms are "reductive". As Kim summarizes (1999, 15 emphasis original),

> Central to the concept of reduction evidently is the idea that what has been reduced need not be countenanced as an *independent* existent beyond the entities in the reduction base—that if *X* has been reduced to *Y*, *X* is not something "over and above" *Y*. From an ontological point of view, reduction must mean *reduction*—it must result in a simpler, leaner ontology.

Apparently, reduction is "eliminative" (in the ordinary sense) in nature. I take the second, italicized "reduction" in the sentence "reduction must mean *reduction*" above as suggesting that a successful reduction must "eliminate" what is previously mistaken as being "over and above" the reduction base. Thus reduction leads to a "simpler, leaner ontology". However, Kim goes on to point out that such "simplification" of our ontology can be accomplished by reduction that is either "conservative" or "eliminative". As he continues (Kim 1999, 15, italic mine),

> Reduction is not necessarily elimination: reduction of *X* to *Y* need not do away with *X*, for *X* may be conserved as *Y* (or as part of *Y*). Thus, we can speak of "conservative" reduction (some call this "retentive" reduction), reduction that conserves the reduced entities, as distinguished from "eliminative" reduction, which rids our ontology of reduced entities. *Either way we end up with a leaner ontology*.

To put it somewhat paradoxically, the "elimination" of entities that are not really "over and above" the reduction base can be achieved by "non-eliminative" reduction.

Thus apart from the contrast between eliminative reduction and conservative reduction when "reduction" is construed as a technical, philosophical notion, there is also another subtle contrast when "reduction" is understood in the ordinary sense, as in the phase "reduction in size". To avoid confusion, I will refer to this *reduction in the ordinary sense* as "elimination" (given that elimination always results in a reduction in the size of our ontology), saving the term "reduction" for the technical notion we are dealing with here.

Thus two different types of elimination might be involved in ontological reduction. On the one hand, there is elimination in the *trivial* sense, i.e., something in our ontology is being eliminated regardless of how the elimination is brought about. This elimination is trivial in the sense that it is a consequence shared by both conservative reduction and eliminative reduction. As Kim puts it, "either way we end up with a leaner ontology". On the other hand, there is elimination in the *robust* sense. It signifies that the (trivial) elimination, or the reduction in the size of our ontology, is brought about by an *eliminative reduction*. Let me put it in another way, in order to avoid the apparent circularity: in a robust or eliminative reduction, the reduced entity is being eliminated via the reduction. And it is this eliminative reduction, understood as robust elimination, that the *non-reductive* physicalists have in mind when they say that their physicalism (despite relying on reduction) is *non-reductive* (i.e., non-eliminative).

The contrast between the two senses of elimination, namely the trivial one and the robust one, allows us to clarify the difference between conservative reduction and eliminative reduction. Although both kinds of reduction are eliminative in the trivial sense, only eliminative reduction is eliminative in the robust sense. A trivial elimination does not require the reduced item to be identical with the eliminated item.[3] For example, when *heat* is reduced to *mean molecular kinetic energy* in a conservative reduction, the reduced item *heat* is not being eliminated. Instead, the reality of *heat* is conserved or vindicated by the reduction. On the contrary, when *polywater* is reduced to *ordinary water containing impurities from improperly washed glassware* in an eliminative reduction, the reduced entity *polywater* is being eliminated at the same time—the elimination is robust. So in what sense is something being eliminated (or there is *a reduction in size*) in a conservative reduction? For instance, what is being eliminated in the reduction of *heat* to *mean molecular kinetic energy*, if *heat* (i.e., the reduced) is not being eliminated?

A possible answer: in identifying *heat* with *mean molecular kinetic energy*, this conservative reduction eliminates what has been falsely identified with *heat*, such as *caloric fluid*. However, if this is the case, it seems that trivial elimination eliminates too much: is *phlogiston*, which was previously mistaken as the substance of *heat* before *caloric fluid*, also eliminated in this conservative reduction? If yes, what about *heat*, understood as *fire* by the Pre-Socratics? Trivial elimination so

[3] Of course, these two senses of elimination do not strictly parallel the two types of ontological reduction. While robust elimination entails trivial elimination (but not vice versa), there exists no similar entailment between eliminative reduction and conservative reduction.

understood is not selective enough to tell us which bogus item should be discarded from our ontology. Therefore, we need a better guidance. Yet, we should bear in mind that this problem is a problem of trivial elimination, not conservative reduction *per se*, although trivial elimination is all we have in a conservative reduction. I will briefly explain in the next section how conservative reduction gets around this problem. We will also see that robust elimination faces a similar problem, albeit for a different reason.

Another possible answer is: conservative reduction eliminates what is "over and above" the reduction base of the entity in question. Thus, in reducing *heat* to *mean molecular kinetic energy*, we eliminate everything "over and above" *heat* by tying *heat* down to the (physical) reduction base, namely *mean molecular kinetic energy*. Thus through conservative reduction, we eliminate everything that is spooky, or non-physical. Nevertheless, our concern here is not about whether or not the spooky, or non-physical elements associated with a given entity *X* should be eliminated. Given our naturalistic conviction, this is beyond question. What is at stake, however, is the ontological status of *X*, i.e., whether or not *X* is spooky. If conservative reduction is all about trimming off the spooky elements of a given entity, then it fails to provide any philosophical insight to the ontological status of that entity. Wimsatt (2006) therefore refers to the reductivist position that relies on this kind of reduction (i.e., trivial elimination) as "vulgar reduction(ism)" or "nothing-but-ism". Kaiser (2015), in employing the label "nothing-but-ism", also indicates that it merely stands for a trivial ontological claim that is basically taken for granted by the majority of philosophers of biology, whether or not they admit emergent properties between levels of reduction.

Now it should be clear why conservative reduction does not serve our purpose. The question of natural kind realism is whether or not natural kinds are real, i.e., whether natural kinds should be eliminated from our ontology. The elimination here should not be understood in the trivial sense. For if it is, then question of natural kind realism will become whether or not natural kinds are spooky, non-physical (hence should be eliminated). Nevertheless, our *naturalist* answer to the question of *naturalness* has already excluded this possibility at the outset, given that this *naturalist* answer identifies natural kinds with groupings sanctioned by natural science. In fact, we mean something stronger when we put forward the claim that natural kinds are irreducible (hence real), namely natural kinds cannot be eliminated from our ontology. Trivial elimination, in suggesting that reduction will result in a leaner ontology, fails to specify this claim. Since conservative reduction is eliminative only in the trivial sense, therefore, it is not sufficient to formulate the notion of reality for natural kinds. What we need is a more robust sense of elimination, which is offered by eliminative reduction. And it is to this option I turn in the next section. Henceforth, when I talk about eliminative reduction, I refer to elimination in the robust sense, unless stated otherwise.

<u>V. The paradox of eliminative (ontological) reduction</u>

Is eliminative reduction really a tenable reading for *ontological* reduction? In other words, is it correct to say that, if *X* is ontologically reducible to *Y,* then *X should* be eliminated (by *Y*)? In the following I will further analyze the notion of ontological reduction and display the inherent paradox of the eliminative reading.

We have already come across the idea that both eliminative and conservative readings are *eliminative in the trivial sense*: reduction results in a "simpler, leaner ontology". Suppose a given ontology *O* is being reduced to another ontology *O'*, given that *O'* represents a "simpler, leaner ontology" than *O*, the reductive relation is *asymmetric*.[4] Of course, what does it mean by a "simpler, leaner ontology" is debatable, but it should be clear that if *O* is reducible to *O'*, *O'* is not reducible to *O*.

If we agree with Kim that "conservative reduction requires identities" (Kim 1999, 15), then it seems that the conservative reading is not an option for genuine reduction anymore. Identity clearly violates the requirement of asymmetry. If *X* is identical to *Y*, then *Y* is of course identical to *X*. Yet, according to those who regard the relation of ontological reduction as an identity, when *water* is reduced to H_2O, it is being identified with H_2O at the same time; still, they would not admit that H_2O could be reduced to *water*, even though *water* is identical to H_2O. How is this possible?

Van Riel (2013) attempts to resolve this paradox by appealing to the Fregean distinction between sense and reference. According to van Riel, who adopts the conservative reading, an adequate philosophical analysis of reduction has to reconcile "diversity" and "directionality" with "unity". "Directionality" refers to the asymmetry of reduction.[5] Van Riel suggests that, for instance, in the reduction of *water* to H_2O, the terms "water" and "H_2O", despite having the same reference, actually carry

[4] Here I refer to the reduction of one *ontology* to another *ontology*. As we see very soon, the notion of asymmetry become more complicated when we move on to the reduction of one *entity* to another *entity*. I assume that reducing a given ontology *X* to another ontology *Y* requires, to a certain extent, the reduction of *X*'s entities to *Y*'s entities. Therefore, if the reduction of a given ontology to another one is asymmetric, then the reduction of one entity to another entity should also be asymmetric. The crucial issue is how we should construe this notion of asymmetry in the reduction of one entity to another entity.

[5] Van Riel prefers the non-technical term "directionality" to "asymmetry". This is because according to him, reduction of an entity *X* to another entity *Y* requires the identity of *X* and *Y*, and identity is symmetric. "Directionality" aims to capture the fact that "if an appropriate instance of '*X* reduces to *Y*' express a truth, then the corresponding instance of '*Y* is reduced to *X*' expresses a falsehood"—despite the fact that there exists no asymmetry between *X* and *Y* (van Riel 2014, 1, fn. 1). In this regard, *directionality* is a weaker requirement than *asymmetry*, for the latter entails the former, but not vice versa. Since my account of reduction is *eliminative* (while van Riel's account is *conservative*), I will follow the common practice in the literature and stick to the use of "asymmetry".

different senses. In other words, they are different modes of presentation.[6] If this is the case, then "diversity" is obtained because two *different* modes of presentation are involved in the reduction. "Unity" is also achieved because these two modes of presentation have exactly the same reference. Lastly, "directionality" is secured by the hyper-intensional context a reduction statement (i.e., "*X* is reduced to *Y*") generates. The hyper-intensional context blocks substitution *salva veritate* of co-intensional expressions. Since without substitution *salva veritate,* we cannot interchange *X* and *Y* in the above reduction statement (despite the fact that they are co-intensional) without changing its truth-value.[7]

One may complain that van Riel's account of reduction is not truly ontological—for the asymmetry is no longer characterized in terms of any ontological difference between the reduced and the reduction base (of which there is none). On the contrary, it seems that the asymmetry is spelt out in representational terms, namely modes of presentation. Since we will soon see that eliminative reduction also has a difficult time with a similar complaint, I will simply provide a quick reply here. The reduction is ontological at its core because reduction exists between two terms

[6] Van Riel maintains that a mode of presentation has to be understood in terms of "property structure", namely "structures of properties under which an expression's meaning or conceptual content presents us with the object the expression designates or signifies" (van Riel 2014, 1). For instance, the property structure of "water" is given by our folk theory of water, while the property structure of "H_2O" is associated with our mature chemical theory.

[7] As van Riel points out, in his model of reduction, the difference between the reduced and the reducing in a reducing statement "*X* is reduced to *Y*" is a difference in "modes of presentation" or "conceptual contents", rather than a difference in *objects*. Consider the statement "*water* is reduced to H_2O". According to van Riel, while *water* is identical with H_2O, there is a mode of presentation m_1 (for example, our folk understanding associated with the term "water") and there is a mode of presentation m_2 (for example, one that states the molecular structure of *water* as two parts hydrogen and one part oxygen) such that when *water* is presented under m_1 is reduced to *water* when presented under m_2. Since the two terms involved in such a reduction statement (e.g. "*water*" and "H_2O") are *co-intensional* (or *necessarily*, *water* is identical to H_2O), a *hyper-intensional* context is needed to block the substitution *salva veritate* of the two terms (van Riel 2014, 100).

only if they both refer to the same entity. Identity, as an ontological notion, serves as the condition of possibility for the reduction on the level of representation.

Let us now turn to the eliminative reading. Intuitively, an eliminative reduction is necessarily asymmetric, as it is supposed to reduce the unreal *by* the real. However, if we characterize the asymmetry in terms of the reality of the given pair of entities, then eliminative reduction immediately runs into trouble. In a non-eliminative reading, the reality of the reduced is conserved by the reducing base. However, in an eliminative reading, the reduced is being *eliminated*. But what *is* actually being reduced? When we say that *polywater* is being *eliminated* through reduction, in what sense *polywater* is really being reduced and eliminated? After all, *polywater* does not exist. And it is exactly because it does not exist therefore it can be *eliminated*. However, if *polywater* does not exist, how can it enter a reductive relation in the first place?

The problem here is not concerned with how we should deal with empty terms. For if it is, perhaps we can simply follow van Riel's strategy and formulate the reduction in terms of modes of presentation associated with the reduced "entity" rather than the reduced "entity" itself, given that a meaningful mode of presentation does not necessarily require a reference. But this will create another problem. Once we shift our focus to modes of presentation, the *asymmetry* of the reduction will be understood in representational terms. In the case of conservative reduction, whether a given mode of presentation can be reduced by another mode of presentation depends on whether they really refer to the same entity. Only a mode of presentation that designates the same entity as the reducing base (which is a mode of presentation itself) will be reduced by it. However, in the case of eliminative reduction, we do not have such ontological linkage to explain why a particular mode of presentation, instead of another, is being reduced in a given reduction. For instance, although "caloric fluid" and "polywater" are intuitively two expressions associated with two different modes of presentations, the ontological status of their purported designations, namely *caloric fluid* and *polywater*, are basically the same, for both of them are not legitimate entities in our ontology. If the reducing base is the *mean kinetic energy*, then ontologically speaking, its relation with *caloric fluid* on the one hand, and its relation with *polywater* on the other hand, should be the same. Yet, we do not want to admit that since both the modes of presentation "caloric fluid" and "polywater" do not have a reference, they can both be reduced and eliminated by the mode of presentation "mean molecular kinetic energy". Again, when reduction is understood as identity, we can formulate the reduction in terms of modes of presentation of the entity in question. This is because the given modes of presentation are supposed to refer to the same entity. When reduction is understood as elimination, no such co-referring relation is possible, given that the reduced term does not have any reference. In other words, unlike identity, the relation of elimination itself (as an ontological relation) does not distinguish items that can be reduced by a given reduction base from those that cannot.

Therefore, in the case of eliminative reduction, we have to introduce additional criteria to govern which bogus, unreal entity can be reduced and eliminated by a given reduction base. Gottlieb (1976) suggests that reduction should preserve the *epistemic role* of the sentences in the reduced theory. By the same token, though with a focus on conservative reduction, Railton (1993) and Mizzoni (2017) indicate that it is the preservation of the same *explanatory role* that allows a reduced entity to be identified with (hence conserved by) the reducing entity. Brigandt (2010, 2012) develops a model of theory succession by drawing a tripartite distinction between reference, *inferential role* (of a concept) and *epistemic goal*. However, the crucial issue is not just to tell the different bogus entities apart, but more importantly, how the differentiation is made. Apparently, these proposed criteria all attempt to make the differentiation by focusing on the epistemic aspects of the reduction. This strategy does not create any problem in the case of conservative reduction, as the reductive relation is still anchored in a relation of identity. Yet, as we have seen, the same strategy would not work in the case of eliminative reduction, since there is no unique ontological linkage between an eliminable, i.e., non-existing entity, and a real one. Without such a linkage, it is doubtful in what sense eliminative reduction would still be genuinely *ontological*, as the reduction may become a purely epistemic one.

Van Riel states in jest that, in an eliminative reduction, "unity, diversity and directionality come very cheap" (van Riel 2014, 2). According to him, "unity" is simply given by the fact that there is just one real entity in an eliminative reduction, *viz.*, the reducing entity. Nevertheless, in addition to it, there is the "alleged existing entity", *viz*., the reduced. Hence, we still find diversity. We also have "directionality" since our "ontologically appropriate talk" (i.e., mode of presentation we attach to the expression allegedly picking out the real, reducing entity) is more basic than our "ontologically misguided talk" (i.e., mode of presentation we attach to the expression allegedly picking out the unreal "entity"). However, if my analysis is correct, eliminative *ontological* reduction actually comes very expensive. The "directionality" or asymmetry of an *ontological* reduction has to go beyond the representational level.

Thus what we want is not just a way to reconcile these three components in our account of reduction, but a way to reconcile them on an ontological footing.

Before offering my answer to this challenge, I will take a look at Quine's classical account of ontological reduction in the next section. By appealing to the notion of ontological commitment, Quine's account seems to successfully resolve the aforementioned challenge within an eliminativist framework.[8] I will argue that the Quinean notion of ontological commitment, defined in terms of existential quantification, is incompatible with our understanding of natural kind realism.

VI. Quine: ontological reduction and ontological commitment

In "Ontological Reduction and the World of Numbers", Quine (1964) sketches the condition for a theory *T* to be reduced to another theory *T*', in an *ontological* sense. The condition is,

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[8] Whether Quine's account is "eliminative", in the robust sense I propose, is indeed controversial. In general, Quine is explicit that reduction, or what he sometimes called "explication", is eliminative, in the sense that it "banishes a problem it does so by showing it to be in an important sense unreal; viz., in the sense of proceeding from needless usages" (Quine 1960, 260). However, he also suggests in various occasions (even in the same book) that the alleged distinction between an eliminative reading and a non-eliminative reading can be viewed as fictitious—they are just different ways of "phasing matters" (1960, 261). This leads some critics (e.g., Gustaffson 2006) to conclude that Quine is neither an eliminativist nor identity theorist—but more like a pragmatist that treats the idiom of reduction or explication as a philosophical tool. Likewise, Gottlieb (1976) indicates that Quine has never argued for the claim that his notion of ontological reduction is eliminative, and he believes that any attempt to construct the missing argument will be futile. According to Gottlieb, Quine's strategy of ontological reduction relies ultimately on his own model of inter-theoretic reduction as well as the thesis of ontological relativity; however, an eliminative reading is incompatible with them. Nevertheless, I believe that adopting an eliminative reading for Quine's notion of reduction is justified, for two *prima facie* reasons. First, his wavering between a strict eliminative stance and a pragmatic stance most frequently happens in his discussion of physicalism. On the contrary, his stance is consistently eliminative in his discussion of the reality of numbers and pairs, which are in connection with the concept of "ontological commitment" that I am going to explore in the coming section. Second, given that Quine also acknowledges the aforementioned paradox with regard to the ontological status of the entity being reduced, he is aware of the robust sense of elimination in his own account. Since my concern here is the feasibility of eliminative reduction instead of Quine's scholarship, in the following exegesis, I will allow myself the freedom to construct and present Quine's account of reduction as eliminative, so far it does not contradict with the core idea of his philosophy.

(C) There is an effective mapping of the primitive predicates of *T* on open sentences of *T′* such that the result of replacing all primitive predicates in a sentence of *T* with the correlated open sentences of *T′* has the same truth value as the original.[9]

This condition is made up of two components. First, the reduction has to be truth-preserving (as suggested by "has the same truth value as the original"). Second, the reduction has to be structure-preserving (as suggested by "mapping of the primitive predicates").

Before having a closer look at these two components, let me first explain in what sense this account of reduction is ontological instead of epistemic, notwithstanding taking theories as the relata. The answer hinges on Quine's notion of *ontological commitment*. Roughly speaking, the ontological commitments of a given theory are the entities (or kinds of entities) that must *exist* in order for that theory to be true. According to Quine, two steps are involved in figuring out the ontological commitment of a given theoretical framework. First, we need to determine the ontological commitment of each theory in that framework by paraphrasing each theory in that framework into first-order logic notations. Quine calls this process "regimentation". Second, we need to determine whether the ontological commitment of each theory is necessary. This second process is handled by the inter-theoretic (ontological) reduction described by the condition (C) above. Let me examine these two steps in turn.

According to Quine, the ontological question—"what there is"—is a question about what a given theory or "form of discourse" is committed to (Quine 1948, 33).

[9] I adopt this paraphrase from Gottlieb (1976). Similar paraphrase can be found in Kroon (1992).

What a given theory or form of discourse is committed to, as Quine elucidates, are "those and only those entities to which the bound variables of the theory must be capable of referring in order that the affirmations made in the theory be true" (Quine 1948, 33). If we take a theory as a set of statements, then what this theory is committed to are what has to exist in order for all these statements to be true. For instance, if a theory contains the statement $\exists x$ Electron (x), then the theory is committed to the existence of electron. Likewise, if a theory contains the statement $\exists x$ (x is red), then the theory is committed to the existence of at least one red object. Notice that in the second example, the theory is not committed to the existence of the property or attribute of *redness*, since the statement does not ***quantify over*** *redness*.

The appeal to bound variables in the above formulation of ontological commitment is motivated by the fact that the ontological commitment of our ordinary discourse, or the common people's ontology, is often obscure and misleading. A case in point is the second example in the previous paragraph. In our ordinary discourse, one may refer to the property *redness* as what a cherry, a chili pepper, and a ripe strawberry all have in common. It thus appears that there exists the property *redness*, i.e., that we are committed to the existence of the property *redness*. However, once we paraphrase our ordinary discourse into first-order logic notations, it will be clear that this commitment to the property *redness* is merely an illusion. This is because the "regimented" language does not require us to quantify over the property *redness*. This paraphrasing, or regimentation, is a prerequisite for the subsequent inter-theoretic reduction, since it clarifies the ontological commitment of a theory. More specifically, it shows how "an apparent commitment to a certain object or kind of object need not be taken as a real commitment" (Hylton 2007, 246). In this regard, regimentation itself can be seen as a form of reduction, albeit preliminary. It allows us to get down to the "primitive predicates" of a theory, as specified in the above condition (C) of reduction. As Quine puts it, "it is only our somewhat regimented and sophisticated language of science that has evolved in such a way as really to raise ontological questions" (Quine 2008, 276).

So, the reduced theory *T* in the above condition (C) of ontological reduction is supposed to be a regimented theory. The question of ontological reduction then becomes whether the ontological commitment of theory *T* is really necessary. The answer is yes, if the regimented theory *T* cannot be reduced to another theory *T'* to yield an overall leaner ontology. Therefore, despite being inter-theoretic, the reduction is ontological at its core, for the reduction of a theory *T* to another theory *T'* is at the same time a reduction of the ontological commitment of *T*'s to the ontological commitment of *T*'.

Let us now return to the condition (C) and examine its truth-preserving and structure-preserving components. For Quine, when we reduce a theory *T* to another theory *T'*, we translate the old predicates of *T* to the new predicates of *T'*. So how do we ensure that the reduction is legitimate? Since the new theory is expected to perform the same job of the old theory, preserving the truth of the old theory is of course, a basic requirement.[10] Different from Carnap, Quine argues that we cannot rely on co-extensiveness to ensure that our reduction is truth-preserving. As Quine illustrates, the different ways of construing natural number formulated by Frege, von Neumann, and Zermelo are equally good in reducing natural numbers into sets.

[10] In fact, how this truth-preserving criterion should be understood is far from straightforward. Nevertheless, the details are irrelevant to our present concern. For a discussion, see Kroon (1992) and Loeffler (2005). Kroon in particular argues that Quinean eliminative reduction cannot be "literally truth-preserving" (Kroon 1992, 55). This is because such a reduction, which aims to replace one ontology with another, "[denies] a foothold to terms purporting to refer to items in the rejected ontology" (Kroon 1992, 55), therefore, sentences containing such terms are false or truth-valueless. I will say more about the relation between the ontologies of the reduced and the reducing theories below.

Nevertheless, their accounts are not co-extensive. On the contrary, they are all legitimate reduction of natural numbers because they are all structure-preserving models of natural numbers. Each of these models provides an "effective mapping" between the old predicates (i.e., natural numbers) and the new predicates (i.e., the Fregean sets, the von Neumann's sets, and the Zermelo's sets) (Quine 1960, 210).

However, even with the addition of the structure-preserving requirement, Quine is still afraid that the above condition of reduction (C) can be easily trivialized. What Quine has in mind here is the wholesale "Pythagoreanism" implied by the Löwenheim-Skolem theorem. According to the Löwenheim-Skolem theorem, any first-order consistent theory (i.e., theory that has a true interpretation) has a model of countable domain. Given that the elements of such a model are countable, one can construct a structure-preserving model of it in natural numbers. The consequence is that "theories of any sort can, when true, be reduced to theories of natural numbers" (hence the name "Pythagoreanism") (Quine 1960, 211).

In response to the threat of trivialization by the Löwenheim-Skolem theorem, Quine further delimits the condition (C) of ontological reduction by adding another criterion: a genuine reduction should be able to specify a *proxy function* that systematically maps the entities in the domain of the reduced theory *T* to entities in the domain of the reducing theory *T'*. Quine believes that we would not be able to specify such a function for a numerical or "Pythagorean" reduction; hence, the threat of trivialization is solved.

Without going into the technical details of how the proxy function works, I want to point out how this additional criterion leads to a problem similar to the paradox of eliminative reduction we discussed in the previous section. The paradox is that while we need to specify the reduced item in ontological terms, such specification is made impossible in an eliminative reduction given that the reduced item is supposed to be a non-existent. Quine indicates that the proxy function is an item belonging either to the new theory or a meta-theory (in contrast to the two object theories in the reduction). Since a proxy function is a *function* that takes entities in the ontology of the old theory as values, it has to range over the domain of the old ontology. Otherwise, these reduced entities cannot be taken as values by the proxy function. However, given that what being quantified over exists, if the proxy function belongs to the new theory or a meta-theory and it ranges over the domain of the old ontology, it means that those reduced entities (mistakenly) postulated by the old ontology also exist, at least from the standpoint of the new theory, or the meta-theory, which is supposed to have a leaner ontology. So we face the same paradox that we left unanswered in the previous section. And Quine is not unaware of it, as he writes (Quine 1969, 58),

> … we cannot declare our new ontological economies without having recourse to the uneconomical old ontology.
> This sounds, perhaps, like a predicament: as if no ontological economy is justifiable unless it is a false economy and the repudiated objects really exist after all.

To put it the other way round, in order for there to be a genuine reductive relation between the old theory and the new theory so that entities mistakenly postulated in the old theory can be reduced and eliminated, these entities must also exist in the new theory. If this is the case, then there is no elimination in the robust sense; instead, it seems that inter-theoretic reduction, according to Quine, is merely an incorporation of the "reduced" theory into a background theory (be it the new theory or the meta-theory) with a broader universe.

Nevertheless, Quine immediately provides a solution, as he continues,

> there is no more cause for worry here than there is in *reductio ad absurdum*, where we assume a falsehood that we are out to disprove. If what we want to show is that the universe *U* [i.e. the ontology of the reduced theory *T*] is excessive and that only a part exists, or need exist, then we are quite within our rights to assume all of *U* for the space of the argument. We show thereby that if all of *U* were needed then not all of *U* would be needed; and so our ontological reduction is sealed by *reductio ad absurdum*. (Quine 1969, 58)

By appealing to *reductio ad absurdum*, the proxy function is able to simulate the talk of the old entities in the new theory, without really committing to them. Suppose this method works, it resembles van Riel's strategy: the distinction between genuine commitment and bogus commitment foreshadows a shift from the actual entity to how the entity is represented in a reductive relation. Our earlier attempt to incorporate this strategy into eliminative reduction fails, because we cannot secure the linkage between the ontological and the epistemic. However, since in Quine's picture, "ontology is a function of theory assent" (Loeffler 2005, 162), the linkage is secured. Although we are operating with the representation or simulated talk of an alleged existing entity, the result is no less ontological: a reduction of a given theory is simultaneously a reduction of its ontology to the ontology of the new theory.

If the *reductio ad absurdum* works, then Quine's model of reduction may allow us to explicate the reality of natural kinds by eliminative reduction. As we have seen, Quine's stance towards reduction is eliminative, as it aims to replace the ontology of the old theory by the more economical ontology of the new theory. More importantly, it is eliminative in the *robust* sense—the old predicates are eliminated by the new predicates through the systematic mapping of a proxy function. Also, Quine's approach is *naturalistic*. Ontological reduction is not directed by *a priori* or metaphysical speculation. "What there is" is determined by our scientific theories. They are the ontological commitments of our mature scientific theories.

Of course, Quine's account of ontological reduction is not free of its own problems. First, the actual details, objective, adequacy, as well as importance of Quine's notion of regimentation remains highly controversial. Rayo (2007) indicates that the standard semantics for (singular) first-order logic, as employed by Quine, fails to encompass all kinds of ontological commitment we find in scientific discourse. Rayo therefore contends that we need either a second-order predicate logic or a plural first-order language to accommodate cases in which a collective reading of pluralized count noun is required (rather than a distributive reading). Rayo (2007) calls this plethological commitment, in addition to ontological commitment.

Second, as many critics argue, Quine's picture of ontological reduction is inconsistent. For example, Loeffler (2005) contends that given Quine's notion of reference and ontology, (inter-theoretic) reduction is impossible in his framework. Kroon (1992) points out that there is a "change of heart" in Quine's later writings with respect to the notion of ontological reduction. There is an increasing emphasis on the preservation of "verbal behavior", to the extent that ontological reduction between two different theories becomes nothing more than translation between two different languages. Moreover, some critics, such as Bonevac (1982) and Iwan (2000), suggest that the proxy function requirement lacks both philosophical as well as technical motivations. On the one hand, it appears that the requirement is introduced just to block the "quirky", wholesale Pythagoreanism implied by the Löwenheim-Skolem theorem; on the other hand, it is argued that a proxy function is not necessary to block the wholesale Pythagoreanism.

Last but not least, if we adopt Quine's account of ontological reduction, we may have to accept many other philosophical commitments that come together as a package. They include: behaviorism, ontological relativity, the inscrutability of reference, and meaning holism. These philosophical claims are highly controversial and may not even be consistent with each other. Some of them are particularly problematic and undesirable given our present concern. For instance, Quine suggests, "existence is a posit from the standpoint of a description of the theory-building process, and simultaneously real from the standpoint of the theory that is being build.... we can never do better than occupy the standpoint of some theory or another…" (Quine 1960, 22). This relativist view, which largely follows from the assumption that "what there is" is just the ontological commitment of the theory in question, favors a pluralist worldview. It therefore goes against our hope of founding a neutral ground for the monism/pluralism debate.

After all, these problems are not fatal. Technical modifications can be made to Quine's actual reductive strategy I have sketched so far, without forsaking its central tenets. For example, instead of just requiring the new theory to preserve the truth-value and structure of the old theory, one may also ask for co-extensive isomorphism between the two theories, as proposed by Goodman. Or one may argue that first-order predicate logic is not a strong enough system to express all our (ontological) commitment, thus switch to a second-order predicate logic for regimentation, as Rayo (2007) suggests above. Despite these variations on issues such as what the commitment of a theory is and how we could figure it out, the spirit behind remains the same, namely our ontology is determined "on grounds largely of the ability of the 'theory' to preserve such theoretic virtues as simplicity of ontology and ideology, explanatory power, and empirical adequacy better than its rivals" (Thomasson 2015, 3). Hereby I follow Thomasson (2015) in referring to this general approach as "neo-Quinean". Despite that accounts grouped under this heading may disagree with Quine's particular criteria or details of regimentation and theory choice, they are Quinean in spirit. What groups them together is the fact that they all endorse two fundamental convictions of the Quinean notion of ontological commitment. These two fundamental convictions are: first, the question of ontology is the question of "what there is"; second, the question of "what there is" is a question of what our mature scientific theories *existentially quantify*.[11]

VII. Problems of the neo-Quinean notion of ontological commitment

So does the neo-Quinean approach safeguard eliminative reduction? Unfortunately, it does not. More precisely, even it does, its two fundamental convictions are in serious

[11] Van Inwagen (1998, 2009) puts forward a list of 5 different theses of the Quinean approach (what I call the "neo-Quinean" here): (1) being, of which the question of ontology is all about, is not an activity; (2) "being" is univocal; (3) the notion of being is the notion of existence; (4) the notion of existence is captured by the existential quantifier; (5) and our ontological commitments are what our best or mature scientific theories existentially quantify. Nonetheless, my subsequent exegesis will only focus on theses (4) and (5), given that my central concern is the antirealist import of ontological reduction. In this regard, Hofweber's account (2005, 2007) will be classified as neo-Quinean, despite the fact that he separates two senses of "existence" or two uses of quantifier. This is because Hofweber still maintains that the existential quantification of a discourse is both necessary and sufficient for answering what there is. The only difference is that this "existential quantification" has to be restricted to the thick, "external" use. Likewise, despite arguing that we should focus on concepts rather than predicates of a scientific theory in order to figure out its ideological commitment, Sider's (2011) account is similarly "neo-Quinean". The difference is that Sider introduces an additional criterion to the question of existence: the concepts of a real theory should also "crave reality at its joints", i.e., they should reflect the fundamental structure of the world. Accordingly, which quantifier is ontologically relevant is not a semantic matter but a fact of the reality, in the sense that the "real" quantifiers have to capture the "quantificational aspect" of the world (Sider 2011, 92). On the contrary, Thomasson's (2015) "easy ontology" is an anti-Quinean account as she denies the claim that existential quantification is necessary for ontological commitment. According to Thomasson, we can simply infer from the fact that conditions of application of the term "*X*" are fulfilled to the fact that *X* exists (rather than the other way round), thus there is no need to appeal to the existential quantification of a regimented theory. Moreover, Hirsch's (2011) "quantifier variance" account is also anti-Quinean, yet for a different reason. The "quantifier variance" accounts suggests that existential quantifier has multiple meanings and they are equally good. Therefore, according to Hirsh, existential quantification is not sufficient for existence. Interestingly, Azzouni (2017), in distancing himself from Hirsch's "quantifier variance" account (what he re-labels as "quantifier immanence"), upholds an anti-Quinean stance for a similar reason. His "quantifier neutralism" maintains that existential quantifier does not tell us what there is.

tension with our understanding of natural kind realism. As Fine points out, one of the central problems of this quantificational understanding of ontology is that it gets "the basic logic of ontological commitment wrong" (Fine 2009, 166).

Fine asks us to consider the respective ontological commitments of a realist about integers and a realist about natural numbers. Intuitively, the realist about integers has a stronger commitment: since he or she "has a *thoroughgoing* commitment to the whole domain of integers", while the realist about natural numbers "only has a *partial* commitment to the domain [i.e., he or she remains non-committal to whether or not negative integers are real]" (Fine 2009, 165). However, the neo-Quinean quantificational view, which identifies our ontological commitments with what is existentially quantified, gives us an opposite interpretation. According to it, the realist about integers asserts that at least one integer exists. Likewise, the realist about natural numbers claims that at least one natural number, i.e., a non-negative integer, exists. Consequently, the realist about natural numbers makes a stronger claim than the realist about integers, in the sense that not only there exists at least one integer, but also that the integer is non-negative.

As Fine goes on to point out, any attempt to fix the neo-Quinean quantificational view of ontological commitment remains *ad hoc* if it fails to provide a uniform and general scheme of what it is to be committed to something *X*. Yet, any uniform and general scheme developed under the neo-Quinean framework will not be restrictive enough to distinguish between realists and antirealists about *X*. Fine concludes that we should therefore give up the quantificational account. As he points out, the *realist commitment* to *X* is not existential but *universal*: when a realist asserts that a sort of entities, namely *X*, is real, he or she is not just committing to the claim that *there exists at least one X*, but that *all X* are *real*.

Similarly, the central tenet of natural kind realism, i.e., *natural kinds are real*, is a *universal* claim. It is not an existential claim saying that there exists at least one kind or grouping that is natural, as the neo-Quinean reading would suggest; rather, it states that *all* natural kinds are real. This categorical construal in fact concurs with our *naturalist* answer to the question of *naturalness*. According to our naturalist answer, our mature scientific theories tell us what groupings are *natural*, namely those that underwrite successful scientific practices such as explanation, induction and generalization. The question of reality asks separately whether these scientific groupings, now being identified as "natural kinds", are *real*. So, our metaphysical interest lies not in whether some of these groupings happen to be real, but whether *all* of them are real. In other words, the realist commitment to natural kinds is categorical (or universal), not existential. The neo-Quinean quantificational view captures only the naturalistic component of natural kind realism, to wit, the claim that *there are natural groupings* (in contrast to non-natural or conventional groupings); however, it fails to acknowledge its metaphysical component, which is the claim that *natural groupings are real*. Therefore, the neo-Quinean model falls short of providing an adequate formulation for natural kind realism.

VIII. Relative fundamentality and natural kind realism

If the neo-Quinean notion of ontological commitment, which is supposed to resolve the inherent problem of eliminative reduction, also fails, what options are we left with? Is there any other way to fix the notion of eliminative reduction? Or should we simply give up the notion of reduction?

To answer these questions, it is important to see that both the inherent problem of eliminative reduction, as well as the problem of the neo-Quinean notion of ontological commitment, stem from the fact that they construe the reality of a given entity solely in terms of its existence. Eliminative reduction requires us to specify which particular entity is being reduced and eliminated. However, such specification is impossible, given that what can be eliminated is supposed to be not existing at all. In other words, there is *nothing* there to be eliminated. By drawing an ontological conclusion from inter-theoretic reduction, the neo-Quinean approach seems to have resolved this problem with its notion of ontological commitment. Yet, the neo-Quinean approach fails to capture the realist commitment of natural kind realism embodied in the naturalist approach of taxonomic pluralism, which suggests that *all* natural kinds are real. Again, this is because the neo-Quinean approach understands the question of reality in terms of existential quantification. Accordingly, the realist and anti-realist positions about a sort of entities X can only be formulated as existential claims, namely $\exists yXy$ and $\sim \exists yXy$.

In fact, if our naturalist approach to natural kinds is correct, then it seems that realists and anti-realists about natural kinds do not disagree about whether there *are* such things as natural kinds. This disagreement would arise only if we adopt a metaphysical answer to the question of *naturalness*, which interprets naturalness as an objective feature of the world. For it then makes sense to ask whether there *are* groupings that possess such objective feature. In contrast, once we adopt the naturalist answer to the question of *naturalness*, the existence of natural kinds is no longer an issue. For in identifying natural kinds with scientific groupings, the naturalist answer at the same time presupposes that there *are* natural kinds. Since the naturalist answer specifically focuses on the *epistemic* feature of natural kinds, it is acceptable to both realists and antirealists about natural kinds. And for those who adopt this naturalist answer of *naturalness*, they can go on to engage in a substantive disagreement concerning the reality of kinds that process this feature of *naturalness*, namely whether these *natural* kinds, i.e., scientific groupings, are *real*. As Fine puts it, "[i]t is only if the existence of these objects is already acknowledged that there can be debate as to whether they are real" (Fine 2009, 169).

So what exactly is the disagreement between natural kind realists and anti-realists, if it is not about the existence of natural kinds? I believe that this disagreement, which goes hand in hand with our *naturalist* understanding of *naturalness*, is concerned with the *relative fundamentality* of natural kinds. This re-orientation of the question of reality from existence to fundamentality actually echoes with a recent trend in metaphysics advocated by Dorr (2005), Fine (2001), Koslicki (2012), Rosen (2010), and Schaffer (2009). As Koslicki sums up (2012, 186), according to this recent trend,

> … many of the most central questions in metaphysics and perhaps philosophy in general are more profitably understood not as asking about the existence of certain apparently problematic sorts of entities (e.g., abstract objects), but rather as asking whether one type of phenomenon (e.g., a smile) is in some important sense dependent on another type of phenomenon (e.g., the mouth that is smiling). Existential questions, it seems, can often be answered trivially ("Yes, of course, there are numbers; after all, 2+2=4"); but even after these questions have been answered, the status of the entities in question still remains to be clarified, e.g., whether they are derivative of another class of phenomena (e.g., concrete spatiotemporal particulars).

Following this trend, the question of reality concerning a sort of entities, *X*, is not just about whether or not there exists something that is *X*, but also, more importantly, about the relation of dependence between *X* and another closely related class of phenomena or sort of entities, *Y*. In addition to asking whether there exists something that is *X*, one should also ask: is *X dependent on Y* (or the other way round)?[12] If it is,

[12] I will elucidate this relation of dependence in the next section.

we may claim that *X* is *less fundamental than Y*. Put it differently, the question of reality does not simply ask about what exists, but what *really* exists, or what is *fundamentally* real (Fine 2001). Our interest in reality goes beyond *what* our mature scientific theories quantify. It focuses on *how* these entities may depend on each other.

Before examining how we should formulate the disagreement between realists and anti-realists about natural kinds in terms of *relative fundamentality*, let me first outline how we are going to incorporate the notion of relative fundamentality into the relation of reduction. Van Gulick's survey of reduction is illuminating here. Instead of the dichotomy between conservative reduction and eliminative reduction proposed by Kim, van Gulick discerns five different types of linkages that are often used in the philosophical literature to explicate the relation of ontological reduction. They are: elimination, identity, composition, supervenience, and realization. Apparently, apart from elimination, the four other linkages are not *eliminative in the robust sense*, i.e., they do not eliminate the reduced entity in our ontology. We have already seen that identity is central to conservative reduction. By identifying the reduced with the (physical) reducing base, the reality of the reduced is vindicated. And clearly, whether the reduced item supervenes on, is realized by, or is composed by the reducing based, it is not thereby eliminated. Nevertheless, there is an important sense that composition, supervenience, and realization, are different from conservative reduction. While the reduced item and the reducing base are supposed to be identical in a conservative reduction, the reduced items in these three linkages *metaphysically depend on* their reducing bases.[13]

[13] One may complain that supervenience, realization, and composition are not genuinely reductive, given that they are usually employed to flesh out the doctrine of non-reductive physicalism (or

Of course, it is unlikely that we can spell out the reality of natural kinds in terms of supervenience, composition, or realization, as they are tailored for specific philosophical phenomena.[14] However, once we are clear that the question of natural kinds' reality is not about their existence, but their *relative fundamentality*, i.e., whether natural kinds *metaphysically depend on* another type of entities (or the other way round), we can formulate a notion of reduction that captures this relation of dependence between the reduced and the reducing base.[15] This relation of reduction is

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physicalism). However, given our distinction between trivial and robust senses of elimination, it should be clear that these relations can be seen as reductive, for they are at least eliminative in the trivial sense. Thus for the sake of clarification, "non-reductive physicalism" (as it is conventionally understood) should be called "non-eliminative (reductive) physicalism" in my account, given the distinction between the trivial sense and the robust sense of elimination. "Non-reductive physicalism" is only "non-reductive" when one adopts the robust sense of elimination to characterize "reduction". For instance, Friedman (1975, 371) makes a distinction between "reductive materialism" and "eliminative physicalism". (Friedman actually uses the term "materialism" instead of "physicalism", but let me put aside the subtle difference between the two.) However, a closer look suggests that his distinction is fictitious under our current concern. Friedman's "reductive physicalism" includes identity theorists as well as functionalists (i.e., those who understand the relation of reduction in terms of realization), it is therefore equivalent to what I have just re-labeled as "non-eliminative (reductive) physicalism". On the contrary, "eliminative physicalists", according to Friedman, think that "mental states and properties do not have a physical basis and that they should therefore be eliminated from scientific discourse" (Friedman 1975, 371). This suggests that for these "eliminative physicalists", a conservative reductive relation is a necessary condition to vindicate the reality of mental states and mental properties. Thus their account of reduction is also non-eliminative in the robust sense of the word. Therefore, both "reductive" and "eliminative" physicalists here agree with the claim that mental states are real if and only if they can be conservatively reduced to physical states; what they disagree about is whether mental states can be so reduced. Notice that the mismatch here between Friedman's and my distinctions is due to the fact that we are dealing with different subject matters. Friedman, in employing the word "reductive" and "eliminative" to describe different versions of physicalism, aims to illuminate what is traditionally understood as the doctrine of reduction*ism*. Conversely, my focus is about how the relation of *reduction* should be understood. At this stage, I am not concerned with the truth of reduction*ism*, which is understood as the claim that all mental states and properties are "reducible" to a physical base. Indeed, given my classification, it should be clear that reduction*ism* is at best trivial. What is crucial is how the notion of *reducibility*, or the relation of *reduction* should be understood, namely whether a trivial sense or robust sense of elimination should be employed, in understanding the reality of a given entity.

[14] Supervenience is concerned with whether a difference in one set of *properties* requires a difference in another set of *properties*. Likewise, realization is concerned with whether some higher-level *properties* are realized by some lower-level *properties*. In contrast, as I indicated above, the notion of reduction that natural kind realism should focus on is concerned with whether natural *kinds* can be reduced to something that is of a completely different nature, i.e., *non-kinds*. Composition, or more specifically material composition, is concerned with whether two or more objects compose a further composite object. Yet, it is unclear how we can construe natural kinds as a material composite object.

[15] In labeling this relation of dependence as "metaphysical", I intend to emphasize the fact that it is non-causal in nature. Moreover, I want to distinguish this dependent relation from another family of relations whose relata are entities, namely *ontological dependence* (Koslicki 2013). As we will see very soon, this "metaphysical dependence" is concerned with the explanatory relation between facts about natural kinds and facts about their members. In this regard, it resembles the relation of *metaphysical grounding*. Yet, as I will also elucidate below, this is at most a specific, rather than general grounding

not strictly eliminative; yet, it is not conservative either. The reduction will allow us to achieve a leaner ontology by establishing that the reduced, which is initially regarded as having the same ontological status as the reducing base, is in fact *less fundamental than* the reducing base. So, if natural kinds are irreducible, then it does not only mean that natural kinds *exist* (i.e., there *are* natural kinds), but more importantly, it also means that natural kinds are *more fundamental than* another type of entities that anti-realists believe natural kinds to be *metaphysically dependent on*.

By reformulating reduction in terms of relative fundamentality, we can now talk about a proper ontological linkage between the reduced item and the reducing item without running into the paradox of eliminative reduction mentioned above. For unlike eliminative reduction, the reduced is not supposed to be eliminated from our ontology. Rather, the reduced is shown to be less fundamental than the reducing base, given that it metaphysically depends on the reducing base. Thus we avoid the problem of referring to an allegedly non-existing reduced entity. Yet, similar to eliminative reduction, the reduced is shown to be less fundamental than the reducing base *through the reduction*.[16]

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relation. Therefore, I do not identify it with the relation of metaphysical grounding, as Rosen (2010) does.

[16] As we will see below, I am going to construe this relation of dependence in terms of explanation. And dependence so construed has a close resemblance with the notion of *grounding*. Yet, the linkage between grounding and reduction is not uncontroversial. On the one hand, as we have seen, there are different notions of reduction. On the other hand, there are also different interpretations of grounding. Trogdon (2013) indicates that there are at least three different conceptions of grounding resulting from three different types of reduction, formulated in terms of essence, fundamentality, and identity respectively. It is therefore doubtful whether appealing to grounding can really illuminate the notion of reduction. This suspicion, according to many grounding skeptics, stems from the assumption that grounding is unitary. Therefore, we should bear in mind that the metaphysical explanation I appeal to is not intended to be a comprehensive, all-encompassing grounding relation, as many grounding theorists intend grounding to be. More will be said in section X.

IX. Metaphysical dependence and metaphysical explanation

But what exactly is this relation of *metaphysical dependence*, which is supposed to establish the fact that the reduced item is less fundamental than the reducing item? There are several answers in the literature. In general, dependence, as a metaphysical relation, can be understood in terms of existence, modality, essence or explanation. In this section, I will briefly illustrate why I am going to formulate the relation of dependence in terms of explanation.

In the previous section I indicated that the neo-Quinean approach, which construes reality in terms of existential quantification, fails to capture the realist commitment of natural kind realism that adopts a naturalist understanding of the concept of naturalness. Thus for similar reason, I am not going to adopt an existential formulation of dependence for our current purpose. As Fine puts it, "there is more to an object is than its mere existence" (Fine 1995, 274). Moreover, Fine's much-discussed example concerning the relation between Socrates and the singleton set of Socrates demonstrates that a modal construal of ontological dependence runs into several serious problem. In particular, the notion of necessity fails to reflect the asymmetry of dependence: while the singleton set containing Socrates *depends* on Socrates but not vice versa, each exists necessarily if the other does.

Although I am not going to adopt the existential and modal accounts of dependence, I do not thereby embrace the alternative proposals that Fine (1994, 1995) and Lowe (1997) subsequently develop, which construe the relation of dependence in terms of essence. Setting aside the technical problems associated with their proposals,[17] the main reason for not accepting an essentialist construal for our current purpose is because an essentialist construal of metaphysical dependence presupposes essentialism, and essentialism is inconsistent with *taxonomic pluralism*. For if we accept the assumption that entities possess essences, i.e., sets of properties that are both necessary and sufficient for their identities, then it seems that we also have to accept taxonomic monism. Since it appears that a classification that sorts individual entities into kinds according to the essences of the individual entities should be regarded as *the* correct classification. As our current task is to formulate a notion of reality that is agreeable to *taxonomic pluralism*, an essentialist construal of ontological dependence does not seem to be appropriate.

I am going to construe the relation of dependence in terms of explanation. Under this construal, *X* ***metaphysically depends on*** *Y* if and only if fact about *X* ***explains*** fact about *Y*. Such explanation is not causal in nature, but *metaphysical*. When Socrates claims in *Euthyphro* that the gods love the pious *because* it is the pious, the explanation involved is not causal in nature. An act being pious does not cause it to become lovable by the gods. Yet, it provides an explanation nonetheless: an act is loveable by the gods *in virtue of* being pious, and not the other way round. I believe that the dependence relation required to substantiate the notion of reduction in natural kind realism should be understood in a similar fashion.

It should be noted that the above characterization is far from comprehensive: we are yet to specify what the *fact* of *X* and the *fact* of *Y* are in the explanatory

[17] For example, one main difficulty of Fine's account is how to avoid the trivialization of essential dependence. Fine (1995) tries to resolve this problem by distinguishing between constitutive dependence and consequential dependence. Koslicki (2012) complains that Fine's account is too coarse-grained to distinguish among different tokens of ontological dependence. Koslicki (2013) also argues that Lowe's (1997) account, which focuses on the identity, fails to account for the identity of empty set.

relation. The explanatory construal of dependence is not necessarily inconsistent with the existential, the modal, or the essentialist construal. In fact, many formulations of dependence in the current literature appeal to more than one of these components. For example, when understood in terms of existence, the dependence relation is usually spelt out in modal terms at the same time, i.e., *X* depends on *Y* if and only if ***necessarily,*** *X* ***exists*** only if *Y* ***exists***. Similarly, an explanatory construal of dependence may also be formulated in terms of modality and existence, such as ***necessarily***, if *X* ***exists***, then it is *in virtue of* the ***existence*** of *Y*. Without further specifying the details in the above formulation, I want to remain neutral towards these different options at this stage. As we will see later, our task is not to formulate an all-encompassing notion of metaphysical dependence, but one tailored for natural kind realism.[18] I will now elucidate the relata of this explanatory relation.

X. Kind-members reduction

In the previous chapter, I argued that Boyd's "accommodationist" account of natural kinds fails to provide a defensible conception of reality for natural kind realism. Despite acknowledging the reality of the causal structures to which the inferential architecture of our disciplinary matrix accommodates, the realism of Boyd's account is not specifically about natural kinds. As we have seen, the reality of natural kinds is not guaranteed in a successful "accommodation"; instead, it is to a very large extent dispensable. As Boyd admits, correspondence and successful referencing are exceptional cases in accommodation. Consequently, the realism of Boyd's "accommodationism" is a realism to which both realists and anti-realists about natural kinds can happily agree. In particular, anti-realists about natural kinds can approve the

[18] For a detailed survey of these different combinations, please refer to Correia (2008) and Koslicki (2013).

claim that the inferential architecture of a given disciplinary matrix is governed by the relevant causal structures of the world, without at the same time admitting that *natural kinds* are real. As I have pointed out, to claim that natural kinds are real is more than just saying that members of natural kinds or properties according to which individuals are grouped into natural kinds are real, for an anti-realist about natural kinds can be realist about all of them. As a non-trivial claim, natural kind realism should commit to the reality of *natural kinds* themselves. But what does it mean to be being realist about *natural kinds*? More specifically, what is the additional realist commitment that differentiates natural kind realism from natural kind anti-realism?

Given that we have reframed the question of reality of natural kinds in terms of *relative fundamentality*, it should be clear that the realist commitment at issue is no longer concerned with the existence of natural kinds. Indeed, anti-realists about natural kinds are no less willing than their realist opponents to accept natural kinds into their ontology. Also, anti-realists would agree with realists that natural kinds, namely scientific categories, are epistemically superior to non-natural kinds. Unlike natural kinds, non-natural kinds are not kinds endorsed by mature scientific investigations: they do not underwrite successful scientific practices, such as explanation, induction, and prediction as reliably as natural kinds do. Nevertheless, unlike natural kind realists, anti-realists are not going to grant a privileged ontological status to natural kinds. In other words, they do not think that the epistemic inequality between natural kinds and non-natural kinds stems from an inequality of their ontological statues, i.e., that natural kinds are real and non-natural kinds are not. In fact, for anti-realists, both natural and non-natural kinds are not *real*.[19]

[19] This is in stark contrast with Dupré's "promiscuous realism". According to Dupré, both scientific groupings (i.e., natural kinds) and non-scientific groupings (i.e., non-natural kinds) are real. In this

But what does it mean by saying that a kind is not *real* when its reality is not understood in terms of existence, but relative fundamentality? As I indicated before, both natural kind realists and anti-realists can regard the individuals that are being classified into kinds, as well as the properties according to which they are classified into kinds, as real. What they differ is that only natural kind realists, but not anti-realists, would regard natural kinds themselves as real. So let me take the first two items as reference. Basically, members of a given kind are *individual entities* that share a set of *properties*. If members of a given kind are real, then everything that they *depend* on is also real. In other words, if members of a given kind depend on that kind, then natural kinds are real, for natural kinds are more fundamental than their members.

So in what sense members of a given natural kind depend on that natural kind? To answer this question, let us take a closer look at the difference between natural kinds and non-natural kinds. A kind, be it natural or not, consists of individuals that share a set of properties. The difference between natural and non-natural kinds does not lie in what type of properties their members respectively share. Natural kinds are natural not because the properties their members share are *natural*. Similarly, non-natural kinds are unnatural not because the properties shared by their members are unnatural. The properties *being white in color* and *being cubic in shape* are by no means gerrymandered or bogus. Yet, the kind *white cubic thing*, which identifies its members according to the properties *being white in color* and *being cubic in shape*, is not a natural kind because there does not appear to be any genuine connection between these two properties. The color of an object does not seem to have anything

regard, Dupré's position should be more accurately regarded as *anti-naturalist* instead of *anti-realist* about natural kinds, since he does not believe that that we can maintain a defensible distinction between natural and non-natural kinds. Please refer to footnote 18 in Chapter 1 for Dupré's argument.

to do with its shape. Simply speaking, non-natural kinds are just collections of things that we can freely bring together by grouping individuals according to similarities we prefer. So there is no question about the existence of non-natural kinds. For depending on our preferences, there just *are* different collections of things. For instance, there *is* the kind *white cubic thing*, given that there *is* a collection of things that are both *white in color* and *cubic in shape*.

Yet, it should also be obvious that why non-natural kinds are not *real*: a given non-natural kind completely *depends on* the properties we choose to select its members. If we have to *explain* why members of the kind *white cubic thing* all share the properties *being white in color* and *being cubic in shape*, clearly we cannot appeal to the fact that these individuals all belong to the kind *being white in color*. This is because the kind *white cubic thing* itself does not determine what properties are being chosen, given that these properties are prior to the kind itself. In other words, non-natural kinds *depend on* their members in the sense that membership in a given non-natural kind, i.e., the fact that an individual entity is a member of the kind in question, is explained by the fact that it possesses a set of properties that we freely select to group things into that kind. Therefore, non-natural kinds are *not real* as they are *less fundamental than*, hence *reducible to*, their members, given that members of a kind are simply individual entities that share a set of properties.

The fact that properties shared by members of a given non-natural kind (or properties that define a given non-natural kind), are freely chosen by us also explains why non-natural kinds do not make the same epistemic contribution as natural kinds do. The kind *white cubic thing* does not underwrite successful scientific practices such as explanation, inference, and prediction precisely because the co-existence of these two properties is contingent. To borrow the term from Khalidi, the kind *white cubic* *thing* is not a *projectible* kind. While each of the properties *being white in color* and *being cubic in shape* may be projectible individually, the conjunction of them is not.

Now consider the natural kind *electron*. Like the non-natural kind *white cubic thing*, members of it also share a set of properties, namely *having a mass of 9.109 x* 10^{-31} *kg*, *having an electric charge of -1.602 x* 10^{-19} *C*, *having an intrinsic angular momentum of ½*.[20] Obviously, unlike the properties *being white in color* and *being cubic in shape*, this particular set of properties shared by its members allows *electron* to underwrite successful scientific practices such as explanation, induction, and prediction. However, as I have indicated, although anti-realists about natural kind admit that natural kinds are epistemically superior to non-natural kinds, they do not think that the ontological statuses of natural kinds and non-natural kinds are different. Thus despite the fact that the epistemic reliability of a given natural kind is based on the properties shared by its members, anti-realists about natural kinds argue that we

[20] In using *electron* as an example, I do not thereby suggest that members of a natural kind must all share a fix set of properties. Given that their members all invariably share a set of common properties, *electron* and many other physico-chemical kinds are usually taken as paradigmatic examples of natural kinds. Yet, this phenomenon is by no means universal among natural kinds. In cases, such as biological species and psychiatric kinds, what we find are stable clusters of properties, none of which is necessary or sufficient for being a member of that kind. Yet, we may understand members of these kinds as sharing historical essences, e.g., members of a given biological species all have the same historical origin. I therefore will remain neutral about the view that members of natural kinds must share a set of essential properties. The difference between Boyd's account of Homeostatic Property Cluster (HPC) kinds and Slater's account of Stable Property Cluster (SPC) kinds has to do with the mechanism that underlies the clustering of properties. Boyd suggests that the stable co-existence of common properties shared by members of a given natural kind is maintained by a homeostatic causal mechanism (Boyd 1999a, 2010a; Wilson *et al*. 2007). Slater makes no such requirement. In his account, the stable co-existence of a natural kind's common properties is understood in terms of *cliquishness*. According to Slater, a cluster of properties is "cliquishly-stable" if "some properties are clustered in such a way that possession of some of them reliably (if imperfectly) indicates the possession of whole cluster (if not *each* property in the cluster) *at that time*. It needs not imply that a particular that possesses any of these properties will *continue* to possess them" (Slater 2015, 397, emphasis original). Nevertheless, for Boyd, the underlying causal mechanism that holds the common properties of a given natural kind together is not necessarily a singular causal relation. Indeed, for Boyd's it is the relevant causal structures of the world taken as a whole that maintains the clustering of properties found among natural kinds. (In this regard, Ereshefsky & Reydon's (2016) critique of Boyd's HPC account should be regarded as missing the point.) Therefore, given this broad notion of homeostatic mechanism, Boyd's HPC account of natural kind is indeed compatible with Slater's SPC account. As we will see very soon, since natural kind realism is concerned with whether the co-existence or clustering of properties has a real basis, rather than the nature of this basis, therefore, I will not go into details about the subtle difference between the HPC and SPC accounts of natural kinds.

should not seek an explanation for the co-existence of these properties on a more fundamental level. For anti-realists, the co-existence of a particular set of properties among members of a given natural kind is as contingent as the co-existence of a set of properties among the members of a non-natural kind. Anti-realists must maintain that the properties shared by members of a given natural kind are ultimately determined by us, just as the properties shared by the members of a non-natural kind. Therefore, for anti-realists, the fact that a group of individual entities being members of a particular natural kind (e.g., *electron*) does not *explain* the fact that why they share a particular set of properties, such as *having a mass of 9.109 x* 10^{-31} *kg*, *having an electric charge of -1.602 x* 10^{-19} *C*, and *having an intrinsic angular momentum of ½*. Instead, the explanation goes in the opposite direction. According to anti-realists about natural kinds, the natural kind *electron* depends on its members in the same way the non-natural kind *white cubic thing* depends on its own members.

On the contrary, natural kind realists must maintain that the difference between natural and non-natural kinds has a real basis: the fact that natural kinds, but not non-natural kinds, underwrite successful scientific practices such as explanation, induction, and prediction is to be explained by the fact that *natural kinds* are *real*. Unlike the properties one uses to identify members of a non-natural kind, natural kind realists must contend that what set of properties members of a given natural kind share is not determined by us. More precisely, these properties are not prior to the natural kind itself. It is not because of our decision that members of the kind *electron* all share the properties *having a mass of 9.109 x* 10^{-31} *kg*, *having an electric charge of -1.602 x* 10^{-19} *C*, and *having an intrinsic angular momentum of ½*. More generally, what properties we found among members of a natural kind does not depend on our epistemic interest. Instead, certain properties (e.g., *having a mass of 9.109 x* 10^{-31} *kg*, *having an electric charge of -1.602 x 10^{-19} C*, *having an intrinsic angular momentum of ½*) are more readily clustered than the others (e.g., *being white in color* and *being cubic in shape*). For natural kind realists, this particular distribution of properties in the world constitutes its natural kind structure(s) and serves as the ground for *why* a particular set of properties is shared among members of a given natural kinds. And it is *in virtue of* being the member of a given natural kind (e.g., *electron*) that an individual possesses a set of properties that can be typically found among members of that kind (e.g., *having a mass of 9.109 x 10^{-31} kg*, *having an electric charge of -1.602 x 10^{-19} C*, *having an intrinsic angular momentum of ½*).[21]

XI. A potential problem of the explanatory construal

Clearly, the explanatory, or the "in virtue of" relation I referred to in the previous section is not causal in nature. Causation is typically diachronic; yet, the dependence between the common properties shared by members of a natural kind and the natural kind itself is *atemporal*.[22] Being a member of the kind *electron* does not cause an

[21] Since the properties *having a mass of 9.109 x 10^{-31} kg*, *having an electric charge of -1.602 x 10^{-19} C*, *having an intrinsic angular momentum of ½* are taken as essential for the membership in *electron*, their co-existence, from the perspective of natural kind realism, is also necessary. Again, this phenomenon is by no means universal across natural kinds. Common properties shared by members of some natural kinds, such as biological species or psychiatric kinds, may not always accompany each other. In other words, none of these common properties may be both necessary and sufficient for membership of a given natural kind. Of course, how stable the coexistence of a set of properties is required in order for a collection of individuals to be regarded as a natural kind is a question related to the *naturalness* of a grouping, and can only be answered empirically. Nevertheless, we can provide some formal criteria to distinguish genuine property clusters from bogus property clusters at the outset. For instance, one may argue that members of the kind *white cubic thing* actually share a set of common properties that co-exist necessarily, such as *being colored*, *being not-black-in-color*, *being not-green-in-color*, *being white-in-color-or-heavy*, *being white in color or dissolvable in water*, etc. Yet, this clustering of properties is *trivial* in the sense that they are logically derived from the property *being white in color*. On the contrary, there is no logical connection between the necessarily co-existing properties shared by members of the kind *electron*, namely *having a mass of 9.109 x 10^{-31} kg*, *having an electric charge of -1.602 x 10^{-19} C*, and *having an intrinsic angular momentum of ½* (Dumsday 2010). Therefore, the clustering of properties in a *bona fide* natural kind should not be trivial, in the sense I have just described. And the reason is clear, for this trivial clustering does not give rise to the epistemic reliability of a given kind.

[22] Alastair Wilson argues that temporal asymmetry is not necessary for causation. Wilson provides two examples. First, he suggests that if consistent time-travel is possible, then someone's going back in time may do something at time *t* that can be counted as a cause of his or her own time-traveling.

individual electron to possess the properties *having a mass of* 9.109×10^{-31} *kg*, *having an electric charge of* -1.602×10^{-19} *C*, and *having an intrinsic angular momentum of ½*.[23] Instead, to be an electron is just to possess these properties. However, if my exegesis of natural kind realism in the previous section is correct, then from a realist perspective, being a member of *electron* does *explain* why an individual possesses a particular set of properties. As I have mentioned, this explanatory relation, which is supposed to capture the relation of ontological dependence, is metaphysical in nature.

Metaphysical explanation has a close connection with the relation of *grounding*. In the current literature of grounding, metaphysical explanation is either construed as a grounding relation itself (Fine 2001, Dasgupta 2014) or a non-causal explanation backed by grounding (Audi 2012, Schaffer 2017). However, the notion of grounding is by no means uncontroversial. One critique of grounding that is particularly relevant to our current concern is to what extent grounding actually illuminates the notion of reduction (understood in terms of *relative fundamentality*). Grounding is generally taken to be a "constitutive form of determination" that is asymmetric, irreflexive, and transitive (Trogdon 2013). But this is far from informative, since we already know that the reductive relation that is required to formulate natural kind realism should possess these formal features.

Second, "any cases of gravitational or quantum action at a distance would (at least on some interpretations) count as synchronic causation" (Wilson 2018, 730). In this regard, causal explanation is not necessarily diachronic. In fact, Wilson also points out that metaphysical explanation can be diachronic, as "my being human as opposed to being a swampman may be grounded in my past causal history, and my present ability to refer to Montana may be grounded in my past causal interaction with Montana" (Wilson 2018, 730). Since these two cases are not directly related to my exposition here, I will simply adopt the popular view that causal explanations are diachronic and non-causal explanations are atemporal.

[23] Of course, being members of a particular natural kind may provide a *causal explanation* why its members all possess a certain properties. A case in point would be the classification of the darter in the snail darter as a single species, which led to the famous *Tennessee Valley Authority v. Hiram Hill et al.*, 437 U.S. 153 (1978). If we take speceis as natural kinds, then the fact that *snail darter* in the Little Tennessee River being classified as a single species *Percina tanasi*, rather than a subspecies or an arbitraty subdivision out of a bigger family, endows members of *Percina tanasi* the property of *being endangered*.

According to Koslicki (2015), the idiom of *grounding* is not fine-grained enough to provide any philosophical insight to the notion of *relative fundamentality*. Koslicki indicates that the various kinds of relation that are usually subsumed under the term "grounding" indeed exhibit diametrically opposite features. For instance, in the case of genus/species, the fact that a geometrical figure is a *square* (the species) is "grounded" (at least partly) in the fact that it instantiates the more general property of *being a rectangle* (the genus). On the contrary, in the case of determinable/determinate, the fact that an object is red in color (the determinable) is "grounded" in the fact that it instantiates the more specific property of being crimson in color (the determinate). As Koslicki argues, such heterogeneity creates a serious problem to a unified notion of grounding, given that the directionality or asymmetry of the relation here is crucial in explaining what grounds what. It is therefore highly suspicious that all these different types of asymmetric relation are really unified by a single relation of grounding.

Likewise, Wilson contends that an all-encompassing "(big-G) Grounding" is "too coarse-grained to do the work of appropriately characterizing metaphysical dependence on its own" (Wilson 2014, 540). In particular, a "(big-G) Grounding" fails to shed light on the notion of ontological reduction, which is our central concern here. Wilson maintains that a "(big-G) Grounding" alone is incapable of providing a consistent notion of dependence to distinguish different philosophical positions that are often not compatible with each other.[24] According to Wilson, in order for a notion of "grounding" to perform this task, it has to examine case-by-case the specific relation of dependence involved. This simply means that there is no work left for an all-encompassing notion of "(big-G) Grounding".

[24] These different philosophical positions include eliminativisn, reductionism, non-reductionism, and emergentism.

I agree with Koslicki and Wilson that appealing to an all-encompassing "(big-G) Grounding" relation is not going to illuminate the notion of reduction. However, what Koslicki and Wilson are skeptical about is a unitary, all-encompassing "(big-G) Grounding" relation, rather than the fine-grained, specific relations of "(small-g) grounding" or metaphysical dependence. Although many grounding theorists, such as Gideon and Schaffer, who believe that grounding is unitary, treat grounding as a relation of metaphysical explanation, the metaphysical explanation I appeal to here should at most be seen as a "(small-g) grounding" relation. This "(small-g) grounding" relation, or metaphysical explanation, in attempting to spell out natural kind realism, is concerned with the specific reductive relation between a given natural kind and its members, not reduction in general. If we employ the grounding idiom, then our question becomes: are membership in a given natural kind *grounded* (understood in terms of a "(small-g) grounding") in the fact its members all possess a set of common properties, or the other way round? Should it be the former, natural kinds are reducible, hence less fundamental than their members. Conversely, if membership in a given natural kind *grounds* the common properties shared by their members, then natural kinds are more fundamental than their members, hence real. Therefore, the notion of metaphysical explanation I appeal to is not intended as an all-encompassing, "(big-G) Grounding" relation that is to be applied across the board to different kinds of metaphysical dependence.[25]

[25] For the same reason, I remain open to more specific details such as whether the determination relation behind this metaphysical explanation should be construed as a form of causation, namely *metaphysical* causation (Schaffer 2016, Wilson 2018). For an objection to this view, please see Bernstein 2016.

XII. Conclusion

In this chapter, I undertook the task to formulate natural kind realism. I proposed that the reality of natural kinds should be understood in terms of *irreducibility*. Yet, the reductive relation I formulated is different from the two options commonly discussed in the philosophical literature, namely conservative reduction and eliminative reduction. Given our naturalist answer to naturalness, the realist commitment that is required to differentiate natural kind realism from anti-realism is no longer concerned with the existence of natural kinds, but their relative fundamentality. For anti-realists about natural kind, since both natural kinds and non-natural kinds are not real, a natural kind depends on its members. On the contrary, natural kind realists maintain that natural kinds are real because members of a given natural kind possess a particular set of properties *in virtue of* being members of that kind.

Our formulation of natural kind realism in terms of relative fundamentality has several advantages over different existing realist accounts we came across in the previous chapter. First, it does not dismiss natural kinds that are mind-dependent as unreal at the outset. Whether a given natural kind is mind-dependent in the traditional sense has no direct bearing on whether it *metaphysically depends* on its members. For we can still legitimately ask whether a given kind depends on its members even though both the kind and its members are mind-dependent. Second, it does not rely on any dubious distinction between epistemic and non-epistemic purposes, as we saw in Khalidi's account. Any such distinction is related to the question of *naturalness*. The notion of relative fundamentality, on the contrary, aims to answer the question of *reality* by illustrating in what sense natural kinds are real. Lastly, unlike Boyd's "accommodationist" account, the *realist commitment* in my account is specifically about natural kinds themselves, not the causal structures of the world. The question it examines is: are ***natural kinds*** *dependent on* their members?

Moreover, appealing to the concept of relative fundamentality allows us to avoid the “deep metaphysical” question of natural kinds, such as whether natural kinds are reducible to universals. For in formulating the reality of natural kinds in terms of irreducibility, we are not asking whether natural kinds are outright irreducible. Rather, the question is more specific: are natural kinds irreducible to their members? This also allows us to remain neutral to the question central to the debate between taxonomic monism and taxonomic pluralism, namely whether a given natural kind could be reduced to another natural kind. In fact, once we have re-oriented the question of reality about natural kinds from its existence to its relative fundamentality and revised the notion of reduction accordingly, we at the same time open a new way to understand the debate between taxonomic monism and taxonomic pluralism. Instead of asking whether kinds posited by one scientific classification can completely eliminate kinds posited by another scientific classification, we now ask: are kinds posited by one classification more fundamental than kinds posited by another classification? I will pick up this question in the last chapter. But before that, in the next chapter, I will first examine another presupposition of taxonomic pluralism. Recall that at the beginning of this chapter, I indicated that the debate between taxonomic monism and taxonomic pluralism is actually concerned with the reality of kinds that cannot be subsumed or integrated into a single classification. But in what sense two or more kinds cannot be subsumed or integrated into a single classification? The underlying notion of incompatibility or *disagreement* will be the subject matter of the next chapter.

Chapter 3: Taxonomic Disagreement

I. Introduction

In the previous chapter I examined the realist commitment of taxonomic pluralism. I argued that the reality of natural kinds should be construed in terms of irreducibility. Yet, the kind of reduction involved is neither conservative nor eliminative, as it is commonly understood. In demonstrating that the question of reality concerning natural kinds is not merely about their existence, the reduction I appeal to aims at capturing the relative fundamentality between natural kinds and their members. This notion of reduction provides us a new way to adjudicate the debate between taxonomic monism and taxonomic pluralism. Instead of asking whether the multiple classifications we find in a given scientific discipline can all be reduced to (in an eliminative sense), hence replaced by, a single classification, we are now dealing with a more reasonable proposal: while these multiple classifications cannot be reduced to one another, is there any one classification more fundamental than the others? However, before delving into this question, let us first ask, in what sense are there multiple classifications that cannot be reduced to one another?

The focus of this question is *how* should the pluralist claim that *there is* ***a plurality of*** *classifications* be understood rather than the justification *why* should we accept taxonomic pluralism. However, a closer look at the justification offered by taxonomic pluralists reveals a puzzle concerning this pluralist claim. Recall that taxonomic pluralists adopt a naturalist answer to the question of *naturalness*. This naturalist answer suggests that how things in a given scientific domain should be classified is determined by empirical findings and actual scientific practices, rather than by any *a priori* speculation. More specifically, it maintains that whether the groupings posited by a given scientific classification are natural depends on how well these groupings underwrite successful scientific practices such as explanation, induction, and prediction. Now taxonomic pluralists further argue that a scientific classification is tailored to the unique epistemic concerns of scientists in a given scientific context. Since scientific practices are multifarious and scientists' epistemic concerns vary from context to context, different classifications result. And it is likely that these different classifications are going to disagree with each other about how individual entities in a given scientific domain should be grouped into natural kinds. In other words, the groupings of these different classifications cannot all be neatly subsumed under a single, overarching classification. Simply speaking, taxonomic pluralists believe that the diverse epistemic concerns of scientists lead to diverse legitimate ways of grouping individual entities in a given scientific domain into natural kinds. Call this the *epistemological argument* of taxonomic pluralism.

So far so good for taxonomic pluralism; yet, this is not the end of the story. And here comes the puzzle. A genuine pluralist picture must comprise classifications that *disagree* with each other. Call this the *disagreement commitment* of taxonomic pluralism. And taxonomic disagreement presupposes an agreement in individuality: classifications that disagree about *how* individuals should be grouped into kinds must agree about *what* individuals are under consideration. Nevertheless, the epistemological argument opens the door to pluralism of individuality: is the practice of individuation investigation-bound, just as the practice of classification? If the answer is yes, then we can further ask: is there more than one correct way in dividing a given scientific domain into individuals? Apparently, at least in some cases, the epistemological argument would give positive answers to both questions. Here biology, of which the multifarious classificatory practices make a strong case for taxonomic pluralism, provides us a very good example: in order to accomplish different epistemic agendas, biologists may have to divide the living world into individuals differently. Hence biological individuality, like biological classifications, is equally multifarious. However, once we accept individuality pluralism, we may have to jeopardize taxonomic pluralism. This is because the diverse epistemic agendas that motivate scientists to classify differently may also require them to individuate differently at the same time. If this is the case, different classifications may end up classifying different individuals and there would not be any genuine taxonomic disagreement between these classifications. Thus instead of supporting taxonomic pluralism, the epistemological argument turns it into a dubious position.

This puzzle is therefore concerned with the *disagreement commitment* of taxonomic pluralism, namely in what sense there is a plurality of classification? Moreover, as a potential critique of taxonomic pluralism, it is different from the traditional monist argument, which questions the ontological status of natural kinds posited by scientific classifications that allegedly disagree with each other. Instead, the puzzle takes issue with the disagreement itself. Given that taxonomic disagreement is necessary for taxonomic pluralism, taxonomic pluralism is on shaky ground if the epistemological argument it appeals to actually threatens genuine taxonomic disagreement. In the next section, I will first clarify the debate between taxonomic monism and taxonomic pluralism by addressing the question: in what sense is there more than one equally correct classification, rather than only one? Then in section (III), I will explicate in what sense two or more classifications are in genuine disagreement. I will argue that taxonomic disagreement arises only when groupings of different classifications crosscut. In section (IV), by surveying the classificatory practice in biology, I will elucidate how the epistemological approach supports taxonomic pluralism. In section (V), I will look into the monism/pluralism debate concerning biological individuality. I will show that the epistemological approach that motivates taxonomic pluralism also supports individuality pluralism. Together, these four sections (II to V) will outline my puzzle for taxonomic pluralism. In sections (VI) and (VII), I will illustrate how this puzzle may arise in the classification of biological individuals on the organismal level. By contrasting two notions of biological individuality in the recent literature, namely evolutionary individuality and physiological individuality, I will demonstrate the tension between the epistemological approach and taxonomic pluralism. In the conclusion, I will elaborate what lessons we can draw from this puzzle.

II. Monism, pluralism, and taxonomic disagreement

According to Ereshefsky, "[taxonomic] monists desire a single preferred classification of a discipline's entities. [Taxonomic] pluralists allow a number of equally acceptable classifications of those entities" (Ereshefsky 2001, 39).[1] The definitions suggest that the two positions are *local* rather than *global*, i.e., the debate between taxonomic monists and taxonomic pluralists is concerned with the classifications of "a discipline's entities", rather than of everything in the world. Indeed, one can be a taxonomic pluralist with regard to one scientific domain but a taxonomic monist with regard to another scientific domain. For example, Slater (2005) believes that we should be pluralist about biological classifications, but monist about classifications in physics and chemistry. Moreover, the view that there exists an absolute and unified

[1] Despite the fact that I am going to focus on the classifications of biological species in illustrating the puzzle for taxonomic pluralism, by "taxonomy", I refer to scientific classification in general, not just specifically to taxonomy in biology or any particular taxonomy such as the Linnaean taxonomy, which is the target of Ereshefsky (2001).

classification of everything in the world does not seem to be popular in the current debate.[2] As Ruphy points out, such an implausible position "may appear quite imaginary—the ranks of philosophical supporters of a complete account of the world are rather sparse today" (Ruphy 2016, 81).

This global/local distinction brings us to the central question of this section: while taxonomic monists and taxonomic pluralists disagree about whether there is more than one equally correct classification of a discipline's entities, what counts as *a* classification? As Nelson Goodman indicates, "the issue between monism and pluralism tends to evaporate under analysis. If there is but one world, it embraces a multiplicity of contrasting aspects; if there are many worlds, the collection of them all is one" (Goodman 1978, 2). Therefore, the challenge for taxonomic pluralism is: what is needed to prevent *different* classifications from lumping into a single one?

Take biology as an example. Intuitively, we can speak of a botanical classification and a zoological classification as two "different" classifications. Yet, it is more accurate to call these two classifications as classifications belonging to two different (sub-)disciplines of biology rather than different classifications of the same discipline. Although there is a sense in which they are different classifications, it is strange to ask which one of them is correct and therefore should be favored over the other. On the contrary, this question, which is core to the monism/pluralism debate, arises when we compare a phenetic classification and a phylogenetic classification, for example. Given that the theory of evolution is widely accepted as the foundation

[2] Possible exceptions are *grounding* theorists such as deRosset (2013). These *grounding* theorists contend that reality comes in layers, of which "[t]he nature and existence of the entities in the higher layers are determined by, dependent upon, and derived from the more fundamental facts and entities we find lower down" (deRosset 2013, 1). Since the puzzle for taxonomic pluralism I am going to depict is not motivated by any monist assumption, I will not consider such a global monist account.

of modern biology, it is not surprising that most biologists would approve a phylogenetic classification. In tracing organisms' lineages, a phylogenetic classification reflects the mechanism of species and biodiversity formation. Yet, biologists as well as philosophers of biology also debate about whether a phenetic classification, which focuses on the morphology of organisms instead of their evolutionary relation, is as correct as a phylogenetic classification in categorizing organisms.[3]

The reason why the monism/pluralism debate does not arise between a botanical taxonomy and a zoological taxonomy is obvious. Although they are both biological taxonomies, they classify completely different entities, namely plants and animals. On the contrary, a phylogenetic classification and a phenetic classification are both supposed to classify the same set of entities, namely "organisms", and they classify them differently.[4] This explains why we can find a similar monism/pluralism debate in the classification of biochemical substances such as proteins, despite the fact that the taxonomic frameworks in question belong to different scientific disciplines, *viz.,* biology and chemistry. This is precisely because both frameworks attempt to classify the same entities, namely proteins: should we prioritize a chemical classification (which focuses on the substances' microstructures) or a biological classification (which traces their evolutionary lineages or biological functions), or should we accept both of them as equally correct?[5]

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[3] While one may complain that there are few proponents of the phenetic taxonomy left in modern-day biology, it remains a legitimate taxonomy from the perspective of the epistemological argument, as advocated by taxonomic pluralists.
[4] Please refer to footnote #14 below for a brief explication of what I mean by "organism" in this chapter.
[5] For a more detailed discussion about the monism/pluralism debate concerning the classification of proteins, please see Bartol (2016).

Therefore, we cannot rely on any intuitive understanding of what *a* classification is or what *a* scientific discipline is in formulating the debate between taxonomic monism and taxonomic pluralism. Moreover, taxonomic monists are happy to accept that there are multiple classifications of a discipline's entities, provided that they can ultimately be reduced to a fundamental one (in an eliminative sense). Consequently, what taxonomic monists and taxonomic pluralists really disagree about is not whether there are multiple classifications but whether there are multiple *incompatible* classifications. In other words, the monism/pluralism debate revolves around classifications that *disagree* with each other.

III. Taxonomic disagreement and crosscutting

In what sense are two or more classifications in genuine disagreement, so that they speak for taxonomic pluralism? The simple answer is that these classifications put forward different kinds and classify the same set of entities differently. Kinds differ either in their intensions (i.e., their criteria in selecting members) or extensions (i.e., the actual members they have). But strictly speaking, kinds with different intensions are not always at odd with each other. This is because kinds with different intensions may turn out to have the same extensions, for example, the kinds *elemental atoms with exactly one proton* and *elemental atoms with exactly one electron*. And since difference in extensions entails difference in intensions, difference in extensions is therefore a more reliable indicator of taxonomic disagreement than difference in intensions.

However, kinds with different extensions do not always stand in conflict in classification. For instance, there is no genuine taxonomic disagreement between a zoological taxonomy and a botanical taxonomy, despite the fact that their respective kinds have different extensions. This is because their respective kinds deal with completely different entities. To ensure taxonomic disagreement, the kinds in question must not only have different members, but they must also have some members in common. In other words, the taxa have to overlap each other.

Nevertheless, even if kinds with different extensions overlap each other, this still does not result in taxonomic disagreement. Consider the kind *vertebrate* and the kind *mammal*. They have different extensions and they also overlap, given that all mammals are vertebrates (but not vice versa). But this by no means suggests that they belong to two conflicting classifications. The kind *mammal* is a sub-kind of the kind *vertebrate*, so the two kinds belong to the same classification. As Quine points out, kinds admit "not only of overlapping but also of containment one in another" (Quine 1969, 119). "Containment" here is a common feature in scientific taxonomy. To say that one kind contains or subsumes another kind means that all the members of the subsumed kind are also members of the subsuming kind. Thus overlapping, in the form of containment, does not signify taxonomic disagreement. For example, in biological classification, the species *tiger* (*P. tigris*) is contained in the kind *Panthera*, which is further contained in the kind *Pantherinae*, so on and so forth. Or in chemical classification, the kind *chemical element* contains among others, the kind *halogen*, which in turn contains the kind *chlorine* that consists of its twenty-four isotopes, from *Cl-28* to *Cl-51*.

Given that complete overlapping is permitted within a single classification, the type of overlapping that is involved in taxonomic disagreement has to be *partial* overlapping. Two kinds that overlap partially *crosscut* each other. Consider the kind *quadruped* (animals using four limbs for locomotion), and *mammal*, as discussed by Tobin (2010). Both *dog* and *human* are classified as *mammal*, and both *crocodile* and *dog* are classified as *quadruped*. However, *human* and *crocodile* together can neither be classified as *mammal* nor *quadruped*. The kinds *mammal* and *quadruped* overlap each other, but not completely. While some mammals are quadrupeds, neither all mammals are quadrupeds, nor all quadrupeds are mammals. Unlike the kinds *mammal* and *vertebrate*, *mammal* and *quadruped* cannot be ordered in a relation of containment.

Taxonomic monists and taxonomic pluralists respond to crosscutting differently. On the assumptions that there is only one correct classification, taxonomic monists either discard or revise the crosscutting kinds in question, for kinds that crosscut cannot all be fit into a single classification.[6] In the case of *mammal* and *quadruped*, one way to get rid of the crosscutting is to replace the kind *quadruped* with the kind *tetrapod* (i.e., animals descended from a four-limbed ancestor). Since all mammals are *tetrapod*, there is no more partial overlapping. The underlying assumption is that the kind *quadruped* is not a genuine natural kind, but may merely be a category used for pragmatic reasons.

On the contrary, pluralists contend that crosscutting confirms their position, for they believe that kinds that crosscut actually belong to different classifications. Taxonomic pluralists cite the widespread crosscutting of scientific categories as evidence against taxonomic monism. Examples include the crosscutting of phylogenetic categories with Linnaean taxa (Hennig 1999, 5), and the crosscutting of chemical kinds that group chemical nuclides according to their atomic number with chemical kinds that group chemical nuclides according to their mass numbers (Khalidi 2013, 70) (we will look at this example in detail in the coming chapter). Hence, pluralists would suggest that the kinds *quadruped* and *tetrapoda* in our

[6] For a brief explanation as to why taxonomic monists think that kinds that crosscut each other do not all fit into a single classification, please see footnote #8 below.

example are posited by two different classifications, namely a phenetic classification and a phylogenetic classification.

According to the divergent ways taxonomic monists and taxonomic pluralists deal with crosscutting kinds, we can formulate the debate between taxonomic monism and taxonomic pluralism in terms of crosscutting.[7] On the one hand, in defending a monist picture, taxonomic monists dismiss some of the crosscutting kinds. On the other hand, taxonomic pluralists construct a pluralist picture by allocating the crosscutting kinds separately to different classifications. Hence, crosscutting serves as a formal criterion for taxonomic disagreement: without any crosscutting, there cannot be any taxonomic disagreement. And without any taxonomic disagreement, taxonomic pluralism is unintelligible, as its conflict with monism would evaporate.[8]

IV. The epistemological argument and taxonomic pluralism

The analysis of taxonomic pluralism in the last two sections lays the groundwork for the puzzle I will now present. Taxonomic pluralism presupposes taxonomic disagreement; taxonomic disagreement arises only when groupings of different classifications crosscut each other. As we have seen, in order for these groupings to crosscut each other, they have to share some, but not all of their members. The puzzle questions whether genuine crosscutting between groupings of different classifications can be secured if we employ the epistemological argument to support taxonomic

[7] One may doubt whether crosscutting is truly necessary for taxonomic disagreement if we see the different taxonomies that make up taxonomic pluralism as disagreeing about how to classify different "parts" of the same more inclusive individuals. For example, two botanists may disagree about how to divide a given clade into different species of glasses. I will address this objection below in section VI.
[8] Whether crosscutting is sufficient for taxonomic disagreement remains controversial. For example, Tobin (2010) argues that the classification of RNA that leads to the crosscutting of the kinds *proteins* and *enzymes* actually shows that crosscutting may arise within a single scientific classification. Since our current concern is whether crosscutting is necessary for taxonomic disagreement, I will bypass the question whether crosscutting is sufficient for taxonomic disagreement.

pluralism. As I am going to show, the epistemological argument is actually in tension with taxonomic pluralism. This is because, if the epistemological argument supports pluralism about individuation just as it supports pluralism about classification, then taxa of taxonomies that appear to be in disagreement may not crosscut each other, as they may not classify the same individuals. Therefore, the epistemological argument, which is supposed to justify taxonomic pluralism, may in fact undermine its very foundation, namely taxonomic disagreement. So let me now turn to this alleged justification for taxonomic pluralism.

The epistemological argument asserts that the classificatory practice employed by scientists in a given investigation is tied to their epistemic agenda. Since scientists have different epistemic agendas in different investigative contexts, it seems that no single classification would be able to fulfill all of these agendas across different investigative contexts. For example, Kitcher (1984, 1987, 2001) argues that no single classification of species can serve the diverse purposes of different biological enquiries. Ereshefsky (1994) points out that even taxonomists may agree about the aim of biological classification, it is likely that they will come up with multiple classifications, given that taxonomists may disagree about what is the best method in achieving such aim. Brigandt (2009, 2011) suggests that philosophical understanding of scientific taxa should focus on the inferential as well as explanatory roles they play in fulfilling epistemic-scientific interests. Since there are diverse epistemic-scientific interests within a single scientific discipline, it is mistaken to assume that any single classificatory scheme alone would satisfy all these diverse interests. This resonates with Kitcher's "pluralist realism" about species: "[p]luralistic realism rests on the idea that our objective interests may be diverse, that we may be objectively correct in pursuing biological inquiries which demand different forms of explanation, so that the patterning of nature generated in different areas of biology may cross-classify the constituents of nature" (Kitcher 1984, 330).

So what are these different epistemic-scientific interests? As we came across in the first chapter, Khalidi (2013) contends that genuine epistemic-scientific interests must aim at discovering the causal nexus of the world. In contrast, Ereshefsky & Reydon adopt a more liberal notion of epistemic-scientific aims. In their critique of the Homeostatic Property Cluster (HPC) kinds, they point out that although scientific kinds are usually employed for causal explanation, scientists also adopt classifications that are not causally oriented. For example, instead of tracking causal connections, the Phylo-Phenetic Species Concept (PPSC) of microbiology aims to capture "stable kinds that have clear identity conditions" (Ereshefsky & Reydon 2015, 973). Despite their different interpretations of what scientific-epistemic interests amount to, Khalidi, Ereshefsky & Reydon share with Kitcher the conviction that scientists need a plurality of classificatory systems to fulfill their diverse epistemic-scientific interests.

Emphasizing the epistemic role of scientific categories in actual investigative contexts, the epistemological argument basically rejects metaphysical considerations in general, of which traditional monists appeal to in formulating their monist worldview.[9] The epistemological argument therefore concurs with our naturalist

[9] One such metaphysical consideration that is relevant to our current concern is the hierarchy assumption. The hierarchy assumption explains why taxonomic monists reject crosscutting kinds, as it maintains that the correct classification of entities in a given scientific domain should only be composed of taxa ordered in a nested hierarchy. This assumption is closely connected with essentialism, which suggests that each entity has a set of attributes that makes it the type of entity it is. Accordingly, a classification that mirrors the structure of the reality should be constructed according to the essences of things. Therefore, an entity should not belong to more than one kind when those kinds are not hierarchically ordered. Otherwise, if an entity belongs to two kinds that are not hierarchically ordered, it means that we do not know what that entity *really* is, given that membership of a kind stands for an entity's identity. Likewise, Ellis (2001, 2014) argues that partial overlapping of kinds is problematic because it runs afoul the requirement that divisions between genuine kinds (i.e., kinds that

answer to the question of *naturalness*, according to which natural kinds are identified as groupings that underwrite successful scientific practices, such as explanation, induction, and prediction. Reydon (2014) describes this shift from metaphysical considerations to the epistemic roles of actual scientific categories as the "epistemological turn" in thinking about natural kinds. Similarly, Brigandt (2011a) advocates a "methodological naturalist" approach that emphasizes the pragmatic value taxonomy contributes to actual scientific practices. Once we take the epistemic roles of scientific categories as their defining features, and further admit that such epistemic roles are determined by the scientific-epistemic interests scientists have in a given investigative context, taxonomic pluralism appears to be a logical conclusion. This is because, according to the epistemological argument, scientists' epistemic interests are multifarious, and no *a priori* consideration can justifiably privilege any particular taxonomy over all the others. Along the same lines, Longino (2002) argues that epistemic virtues such as empirical adequacy, consistency, explanatory power, simplicity, and fruitfulness do not form a set of universal, unchanging criteria. Although it is important to take them into consideration in assessing a given scientific classification, the relevance of each virtue varies from context to context, depending on what epistemic agenda is at stake.

Moreover, contextual consideration is not the sole component in the epistemological argument that pushes for a pluralist stance towards scientific classification. As Brigandt (2011b) points out, variation and heterogeneity are biological realities, and they have scientific importance. The complexity of the living

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capture the real divisions of the world) have to be categorically distinct. This is because if there is a gradual transition from one kind to another, to the effect that it is indeterminate to which kind a thing belongs, then any distinction we wish to make will be a distinction drawn by us rather than by nature. And this contradicts the view that a correct classification should capture a mind-independent reality.

world explains why biologists employ diverse classificatory, methodological, conceptual, theoretical, as well as explanatory frameworks that cannot be reduced to one another. Kellert et al. also suggests that some parts of the world may be so complicated that "cannot be fully accounted for from the perspective of a single representational idiom" (Kellert et al. 2006, xii).

V. The epistemological argument and individuality pluralism

In the previous section, I examined how the epistemological argument may justify taxonomic pluralism.[10] It should be noted that the epistemological argument does not only yield pluralism about classification but also pluralism about other scientific practices. As Kellert et al. affirms, "[t]here can be plurality of representational or classificatory schemes, of explanatory strategies, of models and theories, and of investigative questions and the strategies appropriate for answering them" (Kellert et al. 2006, ix).

Among these scientific practices, what is relevant to our current concern is the practice of individuation. Suppose the epistemological argument also supports pluralism about individuation: if the different epistemic agendas that motivate scientists to classify differently also motivate them to individuate differently at the same time, then the resulting classifications may not be in genuine disagreement. Why? This is because these classifications may not classify the same set of individuals. If this is the case, their groupings may not crosscut each other.

[10] Whereas the epistemological argument I presented in the previous section is predominantly concerned with the classifications of biological species, taxonomic pluralists believe that the same argument also supports pluralism about classifications of other biological entities, for example, proteins (Slater 2009), as well as about classifications in other scientific domains, such as chemistry (Chang 2012), medicine (Kutschenko 2011), ethnobiological kinds (Ludwig 2018), kinds in psychology such as cognitive kinds (Sullivan 2017) and memory (Pöyhönen 2016), psychiatric kinds (Tsou 2017), and among other things, planets (Brusse 2016).

To advance my argument, I will continue to focus on biology. Two questions come to the fore. First, is the notion of biological individuality also susceptible to an epistemological appraisal? If the answer to this question is yes, then we can ask a second question: does the epistemological argument also support individuality pluralism in biology?[11] Let us listen to what the philosophers of biology say.

The analysis of biological individuality is preoccupied with part-whole relationships, as Guay & Pradeu point out, "most, if not all, biological entities appear to be constituted of smaller biological entities" (Guay & Pradeu 2016, 9). Yet, as Love & Brigandt reckon, mereological theories in the tradition of analytic metaphysics merely offer a general logical characterization of the part-whole relationship, without putting any "empirical constraint[s] on what objects can count as a whole" (Love & Brigandt 2017, 320). Just as in the case of classification, these metaphysical doctrines are being accused of overlooking the concrete details and questions pertaining to biology. Indeed, Love & Brigandt's view encapsulates a more general anti-metaphysical attitude. It suggests that we should proceed by examining the specific scientific context in which the question of individuality is asked (Dupré 2012, Godfrey-Smith 2013, Hull 1992, Wilson 2005), rather than attempting to settle the question of what a biological individual is by appealing to any *a priori*, metaphysical speculation.[12] We thus find in the current discussion of biological

[11] One may question at the outset whether we need a clear criterion with regard to what counts as genuine disagreement between individuality monism and individuality pluralism, just as we need such a criterion in the debate between taxonomic monism and taxonomic pluralism. I surmise that a more precise criterion can be formulated in the same vein, namely with regard to the partial overlapping of parts of different individuals. Accordingly, an individuality monist account would argue that all legitimate biological individuals have to fall into complete part-whole relation; on the contrary, a pluralist account would have to maintain that parts of individuals partially overlap. However, it will soon be clear that such a precise criterion is not necessary to vindicate my argument against taxonomic pluralism.

[12] Kaiser (2018) develops a monist account that takes into consideration both epistemological and metaphysical concerns. Kaiser suggests that the two necessary and jointly sufficient conditions of

individuality a similar "epistemological turn" as we observe in the case of scientific classification.

Once we apply this epistemological argument to the practice of individuation, individuality pluralism becomes imminent. Given that part-whole relations play an important explanatory role in biology, Wimsatt (1972, 2007) affirms that different explanatory interests lead to different ways of breaking down a system, i.e., different ways of dividing the whole into parts. According to Wimsatt, in biology, different theoretical perspectives, such as the anatomical perspective, the physiological perspective, and the developmental perspective, all "interact with criteria of evolutionary significance in the analysis of organisms into functional systems and subsystems" (Wimsatt 1972, 72). Since such interactions will significantly increase the complexity of the system under study, thus in order to make accurate predictions and inferences, "the investigator must consider the system from more than one theoretical perspective" (Wimsatt 1972, 72). Different theoretical perspectives are likely to endorse diverse notions of biological individuality, as they attempt to decompose a given biological system differently, i.e., they individuate differently. Not only is it true that these diverse notions of biological individuality may not be compatible with each other, but more importantly, none of them is superior to the others, for the different theoretical perspectives that endorse them are all indispensable for analyzing the biological system at issue.

Of course, one may argue that the epistemological argument does not entail individuality pluralism. For example, one may contend that since evolutionary theory

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biological parthood relationship are (1) spatial inclusion, and (2) compositional relevance. Nevertheless, Kaiser admits that her account, as do other monist alternatives, fails to cope with hard cases.

forms the backbone of modern biology, evolutionary theory will ultimately answer the question of what a biological individual is. Consequently, we should identify biological individuals with evolutionary individuals or regard evolutionary individuals as the paradigm of biological individuals (Clarke 2013, Hull 1992, Okasha 2006).

Generally speaking, evolutionary individuals are bearers of fitness. Be they cells, organisms, superorganism, or species, evolutionary individuals are entities upon which natural selection acts. Nevertheless, how the notion of evolutionary individuality should be understood is far from uncontroversial. For instance, Godfrey-Smith (2009) suggests that evolutionary individuality should be understood in terms of reproduction. Acknowledging the shortcomings of the traditional understanding of reproduction, Godfrey-Smith proposes three parameters to measure three different dimensions of evolutionary individuality. They are: bottlenecks (i.e., a narrowing that marks the divide between generations), reproductive specialization (i.e., whether there is germ-soma division of labor), and overall integration (i.e., coordinated activity and division of labor for reproduction) (Godfrey-Smith 2009). On this picture, biological individuality appears to be a matter of degree. Yet, any entity that possesses all three parameters to a high degree is definitely a paradigmatic individual. But to the contrary, Clarke (2010) contends that these three parameters do not exhaust the notion of biological individuality. With an eye to accommodate multiple realizability, Clarke formulates two conditions that are both necessary and sufficient for being a biological individual. These two conditions are supposed to circumscribe the thirteen features of evolutionary individuality she identifies. The two conditions are: the policing mechanism and the demarcation mechanism. The former is "any mechanism that inhibits the capacity of an object to undergo within-object selection" (Clarke 2013, 421);[13] the latter is "any mechanism that increases or maintains the capacity of an object to undergo between-object selection" (Clarke 2013, 424). Likewise, Ereshefsky & Pedroso (2016), by examining biofilms (i.e., single or multispecies communities of microorganisms), argue that Godfrey-Smith's account is far from comprehensive. Ereshefsky & Pedroso maintain that biofilms are eligible biological individuals, despite the fact that they lack a reproductive bottleneck, high division of reproductive labor, and unified reproductive lineages. This is because biofilms possess features that are generally associated with genuine biological individuals. These features include: internal integrity, repeatable life-cycles, coordination among parts, and heritable adaptive traits. According to Ereshefsky & Pedroso, the example of biofilms demonstrates why no single account of evolutionary individuality is flexible enough to capture the immense variety of reproductive mechanisms in the living world. Ereshefsky & Pedroso therefore propose a broader, "sortal" framework in tackling the issue of biological individuality in general: "the world consists of different sorts of individuals, and whether or not an entity is an individual depends on whether that entity's parts interact (among themselves or with its environment) in a sortal-specific way" (Ereshefsky & Pedroso 2016, 252).

Although both Clarke and Ereshefsky & Pedroso are dissatisfied with Godfrey-Smith's account and attempt to further expand the notion of biological individuality, their stances are different. On the one hand, Clarke's functional account of evolutionary individuality is considered to be monistic (Clarke 2013, Love & Brigandt 2017, Pradeu 2016b, Sterner 2015), as it is "meant to undergird all notions

[13] This condition is necessary because mutations in different parts of an evolutionary individual (e.g., different cells of a multicellular individual) could in principle endow these parts with a different fitness, which by definition would make them different evolutionary individuals. Likewise, two of the three parameters proposed by Godfrey-Smith, namely bottleneck between generations and germ-soma reproductive specialization, also aim to ensure that such within-object differences are of no evolutionary consequence, so there is no within-object selection.

of individuality across theory and practice in the life sciences" (Love 2018, 169). On the other hand, Ereshefsky & Pedroso's proposal is thoroughly pluralistic. Instead of clinging to evolutionary individuality, Ereshefsky & Pedroso believe that the "sortal" framework they propose "allows for multiple theories of individuality corresponding to the multiple kinds of individuals in the world" (Ereshefsky & Pedroso 2016, 252). Indeed, the interests of biologists often go beyond evolution. From the perspective of the epistemological argument, it is unlikely that any single account of evolutionary individuality would effectively guide biologists' investigations in areas such as anatomy, ontogeny, neurology, embryology, and ecology. Individuality pluralists therefore contend that in addition to evolutionary criteria, we should also adopt non-evolutionary criteria in delineating the living world. Again, no single set of criteria would exclusively identify the "real" individuals, for what criteria should be chosen ultimately depends on what epistemic interests biologists have in a given investigative context.

Apart from expanding the notion of biological individuality beyond the horizon of a single theoretical framework such as the evolutionary theory, individuality pluralists also advocate that we should go beyond theories themselves and pay attention to actual experimental practices of biologists (Love 2018, Love & Brigandt 2017, Pradeu 2016b). This "turn to practice" attitude, which upholds that we should reverse the traditional "theory guides practice" methodological assumption, can be seen as a continuation of the "epistemological turn" in thinking about biological individuality. Love suggests that surveying the actual practices in various contexts allows us to understand "why these questions were provoked apart from any consensus or commitment to a fundamental theory of individuality" (Love 2018, 170), given that scientists are concerned with different questions about biological individuality in different investigative contexts. For example, in developmental biology, scientists are not only interested in counting individuals, but also in tracking them, i.e., following them empirically; yet the practice of counting individuals and the practice of tracking individuals are different in nature. More importantly, as Love points out, not all biological investigations are theory-guided. For instance, "molecular, cell, and developmental biology do not rely on a fundamental theory, evolutionary or otherwise, to govern their highly successful practices" (Love 2018, 170).

This "turn to practice" attitude is best observed in the study of genes and proteins. As Waters (2017) argues, there is no universal or absolute answer to the question "what is a gene?", for how a gene is parsed or individuated depends ultimately on the scientific practices at issue. Waters (2018) therefore suggests that we should not ask the question "what is an individual?", as if there is an essence or paradigm of biological individuality. Rather than figuring out an absolute, universal notion of biological individuality, he contends that we should focus on how biologists actually individuate, and what purposes these individuating practices can serve (see also Love 2018, Love & Brigandt 2017). All in all, the take home lesson is: any fundamental theory of biological individuality as uphold by individuality monists "is neither necessary nor warranted for the individuation practices of experimental biologists" (Love 2018, 185).

Again, as in the case of classification, the diversity of biologists' epistemic interests is only one side of the pluralist story. A pluralist stance towards individuation is also a consequence of the complexity of the living world. Apart from the fact that different domains of the living world are often radically different, biologists also recognize individuals on different scales. In addition to organisms, entities such as cells, organs, and superorganisms are also regarded as biological individuals. These different entities call for their own unique notions of individuality. As a result, biologists and philosophers of biology adopt a pluralist stance towards individuation. As Wimsatt points out, assumptions and simplifications made on a lower level, may be appropriate for some questions at that level, but may not be appropriate for questions at a higher level of organization (Wimsatt 2007, 275). Love & Brigandt (2017) also suggest that considerations for decomposing individuals into parts, i.e., lower-level individuals, may vary from context to context. For example, in order to account for evolutionary transition, suppression of selective dynamics among constituent parts is required; in order to study changes during ontogeny, biologists will focus on the spatial boundaries of parts; in order to track activities in various systems and reveal their functional interconnection, biologists may need to decompose individuals into parts that transgress spatial boundaries (Brigandt 2017, Love 2018, Love & Brigandt 2017). In short, according to the epistemological argument, a plurality of biological individuality is needed not only to satisfy the diverse epistemic interests of human practitioners, but also to steer through a complex biological reality.

Therefore, according to the epistemological argument, the practice of individuation, like the practice of classification, is equally governed by the scientific-epistemic interests. Given that scientists have different epistemic concerns in different investigative contexts and the biological reality they are facing is extremely complex, pluralism about individuation seems inevitable. Now let us return to the puzzle for taxonomic pluralism. Taxonomic monists and taxonomic pluralists debate about whether there are multiple correct classifications, in the sense that these classifications are conflicting ways of categorizing the same collection of individuals. However, my survey of biological individuality in this section suggests that this may not be the case.

According to the epistemological argument, not only should the way in which things are classified answer to the epistemic agenda in question, but so should the manner in which these things are individuated in the first place. So if different epistemic concerns motivate different classifications, then one may wonder, would these different epistemic concerns also motivate different ways of dividing the living world into individuals? If the answer is yes, then we can no longer be confident that these classifications will always be in genuine disagreement. This is because, if these different classifications were dealing with different individuals, then their groupings would not crosscut each other. Thus, to the surprise of taxonomic pluralists, these different classifications may not be different ways of classifying the same set of individuals, but may merely be classifications of different individuals. In other words, the epistemological argument taxonomic pluralists employ to support their position may threaten genuine taxonomic disagreement.

Of course, one may object that my puzzle only hints at a possibility that different taxonomies may classify different sets of individuals, but it is also possible that the various epistemic agendas that motivate different taxonomies may all posit the same set of individuals. Indeed, according to the epistemological argument, whether this turns out to be the case is thoroughly empirical, as no metaphysical argument would allow us to settle the issue of biological individuality at the outset.

Nevertheless, given the diversity of scientists' epistemic interests and the complexity of the living world, it is more reasonable to assume that no default, universally agreed upon notion of biological individuality can be taken for granted. Therefore, taxonomic pluralists should not take it for granted that classifications adopted for different epistemic agendas would always classify the same set of individuals

My puzzle is not intended as a knockdown argument against taxonomic pluralism. Instead, it aims to show that the epistemological argument is far from sufficient for taxonomic pluralism, as its proponents believe. In fact, the epistemological argument may even undermine taxonomic pluralism. Taxonomic pluralists thus owe us an argument as to why epistemic agendas that are distinct enough to call for different classifications are likely to yield the same set of individuals, given that biological individuality conditions are no less determined by biologists' epistemic considerations than biological classifications are.

VI. The classification of "organisms"

In this and the next sections, I am going to explore how the puzzle I have depicted may arise in the classification of "organisms", or more precisely, biological individuals on the organismal level.[14] The classification of "organisms" constitutes a

[14] What I am going to present in this and the next section should not be regarded as a separate argument for "organism pluralism", i.e., the view that the notion of "organism" is susceptible for multiple interpretations. On the contrary, my discussion of the classification of "organisms" in this and the next section should be taken as a concrete example of the puzzle I outlined in the previous sections. Following Lidgard & Nyhart (2017) and Pradeu (2016), I take *organism* as a subset of *biological individuals*. More importantly, I do not assume "organisms" to be paradigmatic example of biological individuals like Pepper & Herron (2008). This is not to deny that, from a theoretical perspective, the notion of "organism" is ambiguous at best. In fact, as we will see, my exegesis of the puzzle in the case of classifying biological individuals into species partly hinges on the fact that the notion of "organism" is susceptible to more than one interpretation. Nevertheless, the following discussion focuses on how the aforementioned puzzle would arise if we apply the epistemological argument in support of taxonomic pluralism. Although one may argue that the individuation of "organisms" is less committal and mostly based on pragmatic and intuitive considerations, my discussion exactly aims to show that from the perspective of the epistemological argument, such intuitive interpretation of the notion of "organisms" may not be adequate. Thus instead of examining whether there is a plurality of correct readings for the notion of "organism", my argument should be viewed as questioning what kind of

strong case for taxonomic pluralism, since biologists endorse different species concepts in different investigative contexts and these different species concepts lead to different ways of classifying "organisms". In what follows, I will first survey how different species concepts result in different classifications of "organisms". I will then elaborate, in the next section, why these different classifications may endorse different notions of "organisms". In particular, I will scrutinize the question whether symbionts are biological individuals, from both an evolutionary perspective and a physiological (or more specifically, an immunological) perspective.

A species concept tells us what a species is. Apparently, it instructs us what similarities we should refer to in groupings "organisms" into species, and species into more inclusive taxa, such as families (Ereshefsky 2001). Biologists in different investigative contexts adopt different species concepts. For instance, the biological species concept, which focuses on reproductive isolation, is important for biologists in studying hybrid zones; the ecological species concept, which focuses on the occupation of a distinct niche or adaptive zone, serves the purposes of ecologists; a phenetic species concept, which focuses on morphological similarities, is crucial for paleontologists (de Queiroz 2007, 880).[15] Accompanying these different species concepts are usually conflicting taxonomies, i.e., taxonomies that presumably cross-classify (Hennig 1999, Tobin 2010). For example, if one adopts the phenetic species concept, then one would group "organisms" into species according to their overall

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biological individuals are being classified into species. Roughly speaking, I attempt to show that both evolutionary individuals and physiological individuals are legitimate entities to be classified into species in different classifications. Therefore, in the following, I will put the term "organism" in scare quotes, to refer to potential biological individuals that may be classified into species, without committing to any particular reading of what "organisms" are (i.e., whether "organisms" are evolutionary individuals or physiological individuals).

[15] For a summary of different species concept, please see Ereshefsky (2001), Mallet (1995), and Wilkins (2018); for the discussion of two new species concepts, namely the mitonuclear compatibility species concept and the inclusive species concept, please see Zachos (2018).

similarity (whether the similarity is macroscopic concerning their morphological features, or microscopic such as their amino acid sequences). On the contrary, if one adopts the phylogenetic species concept, one would take species as monophyletic groups, i.e., groups of "organisms" that consist of all descendants of a common ancestor.

The monism/pluralism debate with regard to the classification of "organisms" can therefore be formulated in terms of species concept. As Ereshefsky puts it, "[m]onists believe that biologists should settle on a single species concept. Pluralists maintain that a number of species concepts should be accepted as legitimate" (Ereshefsky 1998, 103). While taxonomic monists would not deny the merits of these different taxonomies, they privilege a particular species concept over all the others. For example, as Hennig suggests, although "[e]ach organism may be conceived as a member of the totality of all organisms in a great variety of ways", these different ways of investigating or conceptualizing an organism is "done most usefully by choosing one system [i.e., the cladistic taxonomy] as the general reference system with which all others are compared" (Hennig 1999, 7). On the contrary, taxonomic pluralists claim that these diverse species concepts are on a par with each other as their respective classifications capture different fundamental divisions of the living world.

So how does the puzzle, or the tension between taxonomic pluralism and individuality pluralism, arise in the classification of "organisms" into species? The puzzle indicates that the diverse epistemic concerns that motivate scientists to classify differently may also motivate them to individuate differently. Should this be the case, taxonomic pluralism would break down, since the living world may be carved up differently in different investigative contexts and taxonomies adopted in these contexts may end up classifying different individuals. So in order to vindicate taxonomic pluralism in the case of classifying "organisms" into species, we must first ensure that the various classifications underwritten by different species concepts actually deal with the same set of biological individuals, i.e., "organisms". But what are "organisms"? Apparently, "organisms" are biological individuals. So the question is: would this particular type of biological individuals, i.e., "organisms", be equally exposed to multiple readings, or would it be well defined enough to resist the pluralist call and serve as the ground of disagreement between different taxonomies?

Before moving on to examine how the puzzle may arise, let me first address an earlier issue, for we now have a better understanding of how the epistemological argument supports multiple ways of classifying "organisms" into species. This earlier issue is concerned with whether crosscutting is necessary for genuine taxonomic disagreement. Let us ask specifically whether this is the case for the classification of "organisms". Apparently, there is a sense in which two or more taxonomies disagree with each other without their groupings crosscutting in the way I presented above: these taxonomies may disagree with regard to how many species there are in a given clade (i.e., a single monophyletic group). Accordingly, biologists disagree with regard to how this clade should be divided into lineages. For example, two botanists might see the task of classifying grasses as a task of classifying the parts of a clade that contains different grasses, and they disagree about what these parts are, i.e., what groups of lineages form species in that clade. If this is the case, it seems that my puzzle characterizes taxonomic disagreement on the wrong level by focusing on crosscutting. For this alternative understanding of taxonomic disagreement suggests that what biologists disagree about is not how individuals are grouped into kinds but how a more inclusive class, i.e., the clade, should be divided into smaller groups, i.e., species. Consequently, crosscutting may not be necessary, for two biologists may disagree about whether a given clade consists of only one species or two species. And the species taxa they posit would not crosscut each other.

A quick reply to this objection is that it is unclear to what extent two cladistic taxonomies would disagree about how a single clade should be divided without their phylogenetic groups crosscutting each other at the same time. If these two taxonomies are endorsed by completely different species concept (e.g., a biological species concept and an ecological species concept), it is very likely that they will posit branching on different levels, rather than just at the end of a single lineage, as suggested by the example in the previous paragraph. As a result, the phylogenetic groups of these two taxonomies would eventually crosscut. So I think crosscutting would still remain a reliable indicator of genuine taxonomic disagreement under this alternative reading of taxonomic disagreement.

Yet, there is a bigger issue with this alternative reading of taxonomic disagreement. In construing the disagreement between two or more conflicting taxonomies as a disagreement about how the *same* more inclusive class, i.e., the clade, should be divided into smaller groups, this alternative reading overlooks the issue that is at stake here, namely *what* individuals are being classified. Given their diverse epistemic concerns, the puzzle questions exactly whether biologists agree about what individuals are there that forms the more inclusive class (i.e., the clade) in the first place.[16] More importantly, according to the epistemological argument,

[16] Of course, this is not to deny that, should biologists have similar epistemic concerns, they may agree about what individuals are there that form the clade under consideration. But then the question is, can we employ these similar epistemic concerns, which ground the agreement on the level of individuality,

taxonomic disagreement arises because biologists adopt different species concepts in different investigative contexts. And these species concepts may not be primarily evolution-oriented. For instance, they may be the phenetic species concept or the ecological species concept. It is unclear whether biologists engaging in investigations that require them to adopt a non-evolution-oriented species concept would readily employ a cladistic taxonomy or take the clade as the point of reference. Indeed, the taxonomic disagreement in question is not just among different evolutionary taxonomies; taxonomic disagreement may also arise between evolutionary and non-evolutionary taxonomies. I therefore believe that crosscutting remains the best way in spelling out the taxonomic disagreement required for taxonomic pluralism.[17]

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as a reference to privilege any particular taxonomy? I will say more about this in the concluding section below.

[17] There is yet another way of interpreting this alternative reading of taxonomic disagreement. We may see it as suggesting that species should not be seen as kinds, but individuals; and "organisms" belonging to a given species taxon should not be seen as its members but its parts. Different ways of classifying "organisms" thus amount to different ways of classifying "the parts" of a more inclusive whole, i.e., the clade. This alternative way of understanding taxonomic disagreement then calls for a completely different commitment to the metaphysics of species, namely species are individuals, not kinds. If we follow the logic of the epistemological approach, then whether species are kinds or individuals is not an issue to be settled *a priori*. Brigandt explicitly suggests that the two metaphysical stories are compatible and one can be "*pragmatically* preferable depending on the *epistemic* considerations that are in play in a certain scientific context" (Brigandt 2009, 78, emphasis original). Thus, although this alternative understanding of taxonomic disagreement is perfectly legitimate, the puzzle I depict would still arise among contexts under which species are being regarded as kinds rather than individuals. It goes without saying that the puzzle would also arise in other scientific disciplines such as chemistry, where we have no good epistemic reason to see the relevant taxa as individuals rather than kinds. In fact, if we look at the motivation for adopting this "species as individuals" thesis, we may wonder whether this alternative understanding of taxonomic disagreement can faithfully reflect the taxonomic pluralism at issue. One of the main motivations for adopting the "species as individuals" thesis is to accommodate the fact that species evolve, since kinds traditionally understood are unchanging (Ghiselin 1974, Hull 1978). Thus in seeing species as individuals, it seems that this alternative way of understanding taxonomic disagreement has already assumed that evolution is the central issue in taxonomy. However, as we have seen, taxonomic disagreement arises between taxonomies underwritten by different species concepts, and these different species concepts are not necessarily evolution-based. It is therefore doubtful whether biologists adopting a non-evolution-based species concept would be willing to embrace the "species as individuals" thesis. As Brigandt suggests, "[t]axa are best construed as natural kinds when they are viewed as *taxonomic units*, while it is preferable to view taxa as individuals when they are conceived of as *units of evolutionary change*" (Brigandt 2009, 78, emphasis added).

VII. The individuation of "organisms"

Let us now inspect the individuation of "organisms". The epistemological argument upholds that biological individuals are not given; rather, like taxonomies, they are the unique products shaped by biologists' theoretical and experimental concerns in a given investigative context. In other words, our everyday, pre-theoretical intuition cannot legitimately settle the issue of what a biological individual is (Hull 1992, Goodnight 2013). This seems to be true also in the case of "organisms" (or biological individuals on the organismal level). While we may tend to identify "organisms" as entities having spatially distinct boundaries, this ordinary, "phenomenal individuation" flounders when we inspect "organisms" such as siphonophores, fungi, and plants like strawberry plant and aspens (Pradeu 2012), or even trickier examples like biofilms and eusocial insect colonies. And these "organisms" are by no means rare in the living world.[18] For the sake of classification, we thus need a more reliable indicator of what counts as a biological individual (in a given investigative context), since it is necessary for us to first figure out *what* counts as an individual before deciding which taxon group a biological individual belongs to. If the epistemological argument is sound, then the only defensible way to delineate the biological individuals is by consulting the epistemic agenda of the investigative context in question. Nevertheless, as both taxonomic pluralists and individuality pluralists maintain, biologists have different epistemic agendas in different investigative contexts, so it is unlikely that biologists would have agreed upon any single notion of "organisms" at the outset. Of course, taxonomic pluralists may still hope that these different epistemic agendas, in

[18] As Pradeu points out, "[m]ore often than not, common sense [i.e., phenomenal individuation] limits itself to examples of familiar organisms around it, forgetting that these make up but a tiny fraction of life" (Pradeu 2012, 232).

endorsing different species concepts, may nevertheless return us with the same set of individual entities. But would this be the case?

To answer this question, let me now turn to a concrete example. In the previous section, we saw that the debate concerning biological individuality revolves around whether we should accept an evolution-based notion of biological individuality or a non-evolution-based notion. A biological individual as recognized by the theory of evolution, i.e., an evolutionary individual, simply speaking, is "any entity upon which natural selection acts" (Pradeu 2012, 230). Apart from the fact that there is no unaniomous agreement on what an evolutionary individual is,[19] evolutionary individuals are by no means the only candidates for "organisms" in biology. For instance, Pradeu (2012, 2016) criticizes Clarke (2013) and Queller & Strassmann (2009) for confusing "organisms" with evolutionary individuals. Indeed, as many critics point out, evolutionary individuality should be clearly distinguished from another type of biological individuality that are traditionally associated with the term "organism", namely physiological individuality (Baker & Wilson 2013, Godfrey-Smith 2013, Pradeu 2012, 2016). Physiological individuals, generally speaking, are understood as "functionally integrated units that change continuously and that are made up of causally interconnected elements" (Pradeu 2012, 230). Pradeu further develops an immunology-based understanding of physiological individuality, according to which "[an] immunological individual is a functionally-integrated whole

[19] Again, given that theorists interpret the evolutionary theory differently, they also come up with different notions of evolutionary individuals. Ereshefsky & Pedroso (2016), Hull (1980), and Lloyd (2017) champion a framework that hinges on the distinction between replicator and interactor. While a replicator is an entity "that passes on their structure directly in replication" (Hull 1980, 318), an interactor is an entity "that directly interacts as a cohesive whole with its environment in such a way that replication is differential" (Hull 1980, 318). According to this framework, genuine evolutionary individuals are interactors. Godfrey-Smith rejects this framework and focuses specifically on what he calls "Darwinian individuals". As he points out, "variation, heredity, and differences in reproductive success are the features of populations that give rise to Darwinian change. Any collection that has these features can be called a *Darwinian population*, and any member of such a collection is a *Darwinian individual*" (Godfrey-Smith 2013, 19, emphasis original).

made up of heterogeneous constituents that are locally interconnected by strong biochemical interactions and controlled by systemic immune interactions" (Pradeu 2016, 804). In contrast, Godfrey-Smith puts forward an account of physiological individuality in terms of metabolism: "organisms are systems comprised of diverse parts which work together to maintain the system's structure, despite turnover of material, by making use of sources of energy and other resources from their environment" (Godfrey-Smith 2013, 25).

Whether physiological individuality is to be understood in terms of immunology or metabolism, or some other notions such as collaboration or mutualism, there is no guarantee that what is being recognized as a physiological individual would be recognized as an evolutionary individual at the same time. As Pradeu points out, "entities individuated on the basis of physiological criteria (often called 'organisms') and entities individuated on the basis of evolutionary criteria (sometimes called 'organisms') *do not always coincide*" (Pradeu 2016, 806, emphasis mine). As Godfrey-Smith also admits, while reproduction is essential to evolutonary individuals (or Darwinian indviduals to be exact), it is optional for "organisms", understood as physiological individuals. More specifically, "[o]rganisms, in this sense, are metabolic units, which may or may not reproduce. Darwinian individuals are reproducing entities, which may or may not have the metabolic features of organisms" (Godfrey-Smith 2013, 184). For example, in allowing "individuals" to contain heterogeneous components, Pradeu's notion of immunological indivduality would recognize holobionts, i.e., multispecies assemblages, as biological individuals. However, it is unlikely for these holobionts to be regarded as evolutionary individuals (or Darwinian individuals), given that the assemblages do not reproduce in the relevant sense: they do not form parent-offspring lineages (Godfrey-Smith 2013).

We ourselves, human beings, are a good example here. Look in the mirror. Let us call that familiar, skin-bound being you see a "phenomenal individual" (in the aforementioned sense). Although this phenomenal individual, which appears to have a clear spatial boundary, is commonly referred to as an individual "organism", its individuaity is ambiguous from a theoretical point of view. At least two individuals that occupy the same spatial and temporal region can be singled out from different theoretical perspectives. If we identify this phenomenal individual with *Homo sapiens*, then from the perspective of an evolutionary theory, this phenomenal individual is an (evolutionary) individual. However, from the perspective of a physiological, or more specifically, immunological theory, this phenomenal "individual" is not a genuine individual. As Pradeu points out, a human being is "made up of symbiotic bacteria numbering at least ten times those that are its 'own' cells", and these bacteria play indispensable physiological/function roles, such as digestion and immunology (Pradeu 2012, 247). More importantly, "there is no fundamental difference between interactions between host immune receptors and these symbiotic bacteria, and the interactions between host immune receptors and the host's 'own' cells" (Pradeu 2012, 247). Thus, from the perspective of a physiological theory, it is the unity of a *Homo sapiens* host and these symbiotic bacteria, rather than the *Homo sapiens* alone, that is deemed a biological individual. The *Homo sapiens* alone would not be counted as an (physiological) individual; it will at most be a part of it.

Meanwhile, appealing to the notion of evolutionary individuality (or Darwinian individuality) would give us a completely different story. The physiological (or immunological) indiviudal in question, namely the holobiont that is composed of the *Homo sapiens* and the symbiotic bacteria, would not be counted as an evolutionary individual. This is becasue this holobniont does not inherit the symbiotic bacteria from its parents; rather, its acquires them from the external environments. Given that this holobiont acquires these symbiotic bacteria from various sources, we can never make sure that the symbiotic bacteria in its intestines are the offsprings of the symbiotic bacteria in its parents' intestines. This holobiont is just horizontally transmitted symbionts (a similar case is the holobiont squid-*Vibrio*). Despite the fact that the host (i.e., *Homo sapiens*) forms a parent-offspring lineage with its own parents, the symbiotic combination of the *Homo sapiens* and the symbiotic bacteria do not form a parent-offspring lineage with its "parents", i.e., the symbiotic combinations of its parent *Homo sapiens* and symbiotic bacteria (Godfrey-Smith 2013, Pradeu 2016).[20]

One may argue that most evolutionary individuals and physiological individuals in fact coincide, and it is mistaken to treat them as distinct individuals. For

[20] One may argue that this example merely shows that different taxonomies classify individuals of slightly different boundaries, rather than different individuals. In other words, the epistemological argument does not lead to a version of individuality pluralism as robust as the one I have presented. In contrast, this less robust version of individuality pluralism can be viewed in the light of the "problem of the many" formulated by Unger (1980). Given that the boundaries of individual "organisms" are vague, biologists disagree about how their boundaries should be drawn. There are different equally legitimate ways of precisfying their boundaries, as they are backed by biologists' specific epistemic concerns. Therefore, biologists in different investigative contexts still deal with the same individuals. If this less robust version of individuality pluralism is correct, then not only individuality pluralism and taxonomic disagreement are preserved, but also our pre-theoretical, intuitive understanding of what counts as an "organism". However, despite all these advantages, I am afraid that this less robust version of individuality pluralism may not fully capture the ambiguity of the notion of "organisms" here. This is because in appealing to vagueness, this less robust version of individuality pluralism has to assume that the various accounts of "organisms" would agree about the cardinality of individual "organisms" in the domain. But this does not seem to be the case. In this example, both the evolutionary and the immunological accounts of individuality agree about the spatiotemporal boundary of the individuals in question. Yet, they disagree about how many individuals are there within that boundary. According to the immunological account, there is one only, namely the holobiont of *Homo sapiens* and the symbiotic bacteria in its intestine; according to the evolutionary account, there is more than one, namely the *Homo sapiens* ***and*** the symbiotic bacteria. That said, there is a sense in which these different investigative contexts, despite counting different individuals, deal with the same set of individuals. Suppose we agree that a whole is part of itself. Then at least in the example of "organisms", the investigative context that posits evolutionary individuals and the investigative context that posits physiological individuals agree about how many and what parts are there. What they disagree about is whether certain combinations of parts also form an individual. Be this as it may, this will not lead to genuine crosscutting, as these parts themselves may not be regarded as genuine members of any taxon under consideration.

instance, apart from single species "organisms", such as fruit flies, some multiple species symbiotic associations are identified as both evolutionary and physiological individuals at the same time. One example is the holobiont aphid-*Buchnera*: since the bacteria in an offspring aphid are descendants of the bacteria in the parent aphid, they are vertically transmitted symbionts. Thus Aphid-*Buchnera*, which being idenified as a physiological individual, is also regarded as an evolutionary or Darwinian individual (Godfrey-Smith 2013).

I admit that if the majority of evolutionary individuals are physiological individuals, and vice versa, then we no longer have a plurality of individuals. Instead, we merely refer to different aspects of the same individuals. Gilbert & Tauber (2016) and Gilbert et al. (2017) seem to uphold this view when they argue that holobionts constitute a unit and level of evolutionary selection. If this is the case, then it means that whenever we classify evolutionary individuals, we are classifying the physiological individuals at the same time. In this case, taxa of an evolution-based taxonomy would genuinely crosscut taxa of a physiology-based taxonomy, since both taxonomies deal with more or less the same collection of entities.

On the other hand, if the majority of evolutionary individuals are not physiological individuals, and vice versa, then there is a clear sense that their respective classifications deal with different collections of individuals. Of course, whether or not the majority of evolutionary and physiological individuals actually coincide is ultimately an empirical question. As Pradeu points out, "a major result of recent biological research is precisely that very often a physiological individual is not as such a reproducing entity, but rather a local nexus of different lineages of reproducing entities" (Pradeu 2016, 809). I therefore think that we have good empirical ground to question whether different species taxonomies actually classify the same collection of individuals.

While I appeal to the mismatch between evolutionary individuals and physiological individuals, the actual situation is much more complicated. Evolutionary individuality and physiological individuality are by no means the only two accounts of biological individuality. Investigations in othe areas of biology, such as ecology, developmental biology, and cognitive science may all call for different principles of individuation. These different principles of individuation identify and count biological individuals in radically different manners. As we have just seen, even the same spatial temporal region can be occupied with more than one type of biological individuals. This means that what are being recognized as individuals in one investigative context may not be so recognized in another investigative context. We have no good reason to expect that different investigative contexts, in adopting different accounts of biological individuality, would provide us with the same set of biological individuals. The puzzle for taxonomic pluralism therefore lingers on.

VIII. Conclusion

In this chapter I have presented a puzzle for taxonomic pluralism. The puzzle questions the *disagreement commitment* of taxonomic pluralism. In justifying taxonomic pluralism on epistemological grounds, taxonomic pluralists maintain that conflicting taxonomies are necessary to answer to biologists' diverse epistemic interests as well as the manifoldness of the living world. Nonetheless, this epistemological argument also supports pluralism about biological individuality. Diverse epistemic concerns do not only motivate different ways of grouping individuals into kinds, but also different ways of carving up the living world into individuals. Yet tension arises between these two forms of pluralism. Taxonomic pluralism presupposes taxonomic disagreement, and genuine taxonomic disagreement requires agreement on the level of individuals: classifications that disagree about *how* things should be classified must agree about *what* things are being classified. Yet, individuality pluralism threatens this agreement. Since different investigative contexts may call for different principles of individuation, the taxonomies adopted in these different investigative contexts may end up classifying different individuals.[21]

I illustrate this puzzle with the case of classifying "organisms" into species. On the one hand, an investigative context that studies the physiological features of biological individuals is likely to pick out physiological individuals at the outset; on the other hand, an investigative context that sets sight on evolutionary lineages is likely to select evolutionary individuals. We cannot take it for granted that the classifications respectively adopted by biologists in these two investigative contexts are in genuine disagreement. This is not because some of their groupings that allegedly crosscut are not genuine scientific kinds, as the traditional monists would

[21] One may ask: if taxonomic pluralism can no longer be maintained in the face of individuality pluralism, how should we understand the ontological relation between kinds of different classifications that appear to crosscut each other? To this question, I think the quote from Pradeu in the previous section provides a very good hint: a physiological individual is a "local nexus of different lineages of reproducing entities [i.e. evolutionary individuals]" (Pradeu 2016, 809). Similarly, Godfrey-Smith, despite adopting a different reading of physiological individuals, suggests that they are "metabolic knotting of reproductive lineages that remain distinct" (Godfrey-Smith 2013, 30). Bartol provides an illuminating answer to this question in his discussion of protein's classification. He argues that there is no cross-classification of proteins between a classificatory framework that focuses on their microstructure and a classificatory framework that focus on their evolutions. This is because [t]he microstructure is just one part of the biological unit" (Bartol 2016, 546). The individuals of these two different classifications intertwine, in the sense that "[a] single chemical molecule may contain multiple biological individuals" and "the same biological kinds will often exist on different chemical kinds" (Bartol 2016, 548). However, the two classifications do not result in crosscutting as they are classifying different individuals. Bartol calls this solution a category-dualist one: proteins, as biochemical entities, are "at the nexus of two kinds (of kinds)" (Bartol 2016, 549). Thus, in spite of the fact that these kinds do not partially overlap, they are still connected—in the sense that some parts of the individual members of one kind are also parts of the individual members of another kind (as the evolutionary individual squid is part of the physiological individual squid-*Vibrio* holobiont). I believe that this is how we should understand the relation between the alleged crosscutting kinds. Of course, a comprehensive exegesis of this ontological picture is beyond the scope this chapter.

argue. Rather, their groupings, despite all being genuine scientific kinds, may not crosscut each other, as these two classifications may not be classifying the same individuals. While one classification deals with physiological individuals, the other classification deals with evolutionary individuals. And individuals in one investigative context may not even be recognized as individuals in another investigative context. Failing to secure any genuine taxonomic disagreement, taxonomic pluralists cannot maintain that these are *different* classifications in the sense that they are conflicting ways of groupings things into kinds. Hence, taxonomic pluralism is in doubt.[22]

Undeniably, the example of classifying biological individuals into species is only a singular case. It does not mean that taxonomic pluralism in other areas of biology or disciplines of science would encounter the same problem. Indeed, I do not attempt to generalize from this particular example to other areas of biology or other disciplines of natural science. As the epistemological argument suggests, whether the puzzle would arise can only be evaluated on a case-by-case basis. Yet, the choice of this example is not accidental. For one thing, taxonomic pluralists have long been appealing to the conflicting classifications of "organisms" endorsed by divergent species concepts in justifying their position. For another, the individuation of "organisms" appears to be least disputed by taxonomic pluralists, compared to other biological individuals, such as genes and proteins. My illustration shows that even

[22] It should now be clear that I do not attempt to depict how biologists *actually* classify "organisms". Instead, I try to illustrate what the classification of "organisms" *would* become if we embrace the epistemological argument. And my illustration shows that the epistemological argument provides us with good reason to expect biologists to classify different "organisms" in different investigative contexts. So, even if in reality biologists *actually* classify the same individuals in different investigative contexts, this does not provide taxonomic pluralists a strong enough reason to dismiss the puzzle. On the contrary, the inconsistency between what biologists actually do in reality and what they would do under the prediction of the epistemological argument should become a concern for taxonomic pluralists, who rely on the epistemological approach in justifying their position. Indeed, to what extent can we prove that biologists in reality *actually* classify the same "organisms", if there is no incontrovertible notion of "organisms" we can fall back on, as suggested by the epistemological argument?

this paradigm case of taxonomic pluralism is not immune from the puzzle. In fact, similar concern also arises in other typical examples of taxonomic pluralism. For instance, as Khalidi points out, "In some cases, the individuals classified according to crosscutting systems of kinds may not be exactly coincident: Mass number applies to the atomic nucleus, while atomic number pertains to the whole atom (including electron orbitals); mental states are states of persons, while neural states are states of the brain" (Khalidi 2013, 122). That being said, the puzzle would not apply to areas of biology where the monism/pluralism debate is predominantly concerned with individuation rather than classification. For example, in the classification of genes, the crucial issue is how genes are actually parsed. So the debate on how genes should be "classified" is in fact a debate on how genes should be individuated.

After all, the puzzle does not show that, for each investigate context, there is one unique way of individuating and one unique way of classifying. Therefore, the puzzle leaves open the possibility that genuine crosscutting may arise within a given investigative context, rather than among different contexts. Should this be the case, taxonomic pluralism is vindicated by the conflicting classifications within a given investigative context.

Nevertheless, I believe that even in investigative contexts where genuine taxonomic disagreement arises, taxonomic pluralism is not necessarily the case. Consider an investigative context where biologists are concerned with tracing the lineages of "organisms". Presumably, these biologists would focus on evolutionary individuals; moreover, they would privilege a phylogenetic classification since it represents better the evolutionary history of "organisms" than other classifications. Of course, these biologists may additionally adopt a phenetic classification that focuses on the overall similarity of these evolutionary individuals, regardless of their evolutionary relation. And the groupings of the two classifications would genuinely crosscut, given that the two classifications are dealing with the same set of individuals. Nonetheless, in this case, it is more reasonable to posit the phylogenetic classification as a reference for the phenetic one. This is because the phylogenetic classification, in grouping individuals together according to their most common ancestor, aligns better with evolutionary individuals than the phenetic classification.

Building on this example, let me briefly sketch how we may derive a new way of resolving the monism/pluralism debate about classification based on the puzzle I have depicted. In the original story, where the notion of individuality is not being questioned, taxonomic pluralists justify their position by referring to the fact that conflicting classifications are required to satisfy the unique epistemic concerns of scientists in various investigative contexts. The puzzle I have depicted shows that this epistemological argument also supports pluralism about individuation and may therefore undermine taxonomic pluralism, since it would imply that the two sides were describing different individuals, and hence no real conflict would arise. Thus to vindicate their position in the face of the puzzle, taxonomic pluralists must first ensure that the different taxonomies are dealing with the same set of individuals. Since the practice of individuation, according to the epistemological argument, is also determined by the epistemic concerns of the scientists in a given investigative context, we can therefore take it as a point of reference, and ask: which of the conflicting classifications best meshes with the chosen principle of individuation in answering the epistemic concerns in question? In other words, in our new story, the question at stake in resolving the monism/pluralism debate about classification is: would the chosen principle of individuation privilege a particular classificatory framework?

Of course, whether a given principle of individuation would ultimately privilege a particular classification and how such classification is chosen, can only be determined on a case-by-case manner, given that scientists have different epistemic concerns in different investigative contexts. Yet, in the next chapter, I am going to present a way of evaluating the relative fundamentality of different classifications in a given scientific domain. The important lesson we can draw from the puzzle in the current chapter is: the epistemological argument does not necessarily lead to taxonomic pluralism even in cases where genuine taxonomic disagreement obtains.

Finally, it may appear that I am pushing for an incommensurability thesis with my puzzle in this chapter: there is no genuine disagreement between "different" classifications because they are classifying different individuals. Yet, it should be clear that no Gestalt switch, paradigm shift, or revolutionary change in the Kuhnian sense is involved in my puzzle.[23] In fact, if my puzzle has anything to do with Kuhn's account of incommensurability, it can only be that it tells a completely different story from the one Ian Hacking presents in response to the "new-world problem" formulated by Kuhn. The "new-world problem" asks: how should we make sense of

[23] In particular, my puzzle does not rely on the incommensurability of meaning, which is central to Kuhn's (and Feyerabend's) claim that rational disagreement between paradigms is impossible. As Plunkett & Sundell (2013) rightly point out, the fact that interlocutors assign different meanings to the same term at issue does not necessarily preempt rational disagreement between them. For disagreement can take the form of metalinguistic negotiation concerning how the term should be understood and used in a particular context. On the contrary, incommensurability of meaning, if any, comes as a corollary instead of a presupposition in my argument. In applying the epistemological argument of taxonomic pluralists to the notion of individuality, I argue that pluralists may lose the ground for genuine taxonomic disagreement. This is because epistemic agendas that motivate different classifications will at the same time advance different principles of individuation—thus different classifications may fail to classify the same individuals. Such failure does not require a shift of meaning of the term "individuals". Genuine taxonomic disagreement requires different classifications to cross-classify the same *individuals*; it does not require different classifications to assign the same *meaning* to the term "individuals". So, it is possible for taxonomic disagreement to arise between two classifications even though "biological individuals" may refer to evolutionary individuals in one classification and physiological individuals in another—provided that most of these evolutionary individuals are at the same time recognized as physiological individuals, and vice versa, as I indicate above.

the fact that "[t]hough the world does not change with a change of paradigm, the scientist afterwards works in a different world" (Kuhn 2012, 121)? For Hacking, the answer is, "[t]he world that does not change is a world of individuals. The world in and with which we work is a world of kinds. The latter changes; the former does not" (Hacking 1993, 306). This "nominalist" solution relies heavily on the crosscutting between groupings of different classifications. According to Hacking, scientific kinds are projectible: they "are used in making generalizations, forming expectations about the future (or unexamined events in the past or distant present). They can be used in counterfactual conditionals. They occur in lawlike sentences" (Hacking 1993, 293). Therefore, the crosscutting of scientific kinds exactly shows that, through these different classifications, scientists see the world differently. On the contrary, I contend that the crosscuttings of groupings we find in the natural sciences may not be genuine because the "different" classifications in question may deal with different individuals. If the world of kinds change, the world of individuals may change at the same time: as the diverse epistemic interests would not only require scientists to group things into kinds differently, they may also require them to carve up the world into individuals differently.

Chapter 4: Relative Fundamentality between Classifications

I. Introduction

In the previous chapters, I presented two problems for taxonomic pluralism. Both of them stem from the *naturalist* answer to the question of naturalness, which forms the backbone of taxonomic pluralism. According to this naturalist answer, natural kinds are construed as theoretical groupings that underwrite successful scientific practices, such as explanation, induction, and prediction. As taxonomic pluralists maintain, the unique epistemic concerns of scientists in different investigative contexts may call for scientific practices that are radically different in nature; consequently, these scientific practices would endorse different classifications that are usually in conflict. For taxonomic pluralists, not only are different classifications needed to fulfill different scientific-epistemic tasks, but also, more importantly, these different classifications are *equally correct* in capturing the divisions in the world. Call this the *equality commitment* of taxonomic pluralism. Taxonomic pluralists therefore argue that taxonomic monism is wrong.

As I have pointed out, to be a defensible alternative to taxonomic monism, taxonomic pluralism must hold on to two commitments, namely the *realist commitment* and the *disagreement commitment*. The *realist commitment* asserts that, while pluralists adduce multiple classifications in supporting their position, the groupings posited by these multiple classifications must be *real*. I contended in Chapter 1 that a genuine commitment to the reality of scientific kinds requires pluralists to be realist about the *kinds* themselves, not just the world, the individual members of the kinds, or the properties according to which individuals are classified into kinds. The *disagreement commitment* suggests that a pluralist picture must comprise classifications that *disagree* with each other. I argued in Chapter 3 that in order for different classifications to be in genuine disagreement, these classifications have to cross-classify the same individuals. Yet, as I have demonstrated, in adopting a *naturalist* answer to the question of naturalness, taxonomic pluralism fails to secure these two commitments.

Now suppose that both the realist commitment and the disagreement commitment are secured with regard to the classifications in a given scientific domain, i.e., the theoretical groupings of two or more classifications crosscut each other and they are all real. Does it mean that taxonomic pluralism is thereby vindicated? The current chapter aims to tackle this question. I am going to argue that satisfying the realist commitment and the disagreement commitment are still not sufficient for taxonomic pluralism. This is because one of the classifications in question may be more *fundamental* than the others. In other words, these different classifications may violate the *equality commitment*. Should this be the case, taxonomic pluralism would not fully reflect the ordering between different classifications in a given scientific domain. This is the final challenge I pose against taxonomic pluralism in this dissertation.

In order to establish this challenge, I will elucidate in what sense one classification is more fundamental than another classification. This requires me to further develop and apply the notion of relative fundamentality I have outlined in Chapter 2. The basic idea is that a classification is more fundamental than another classification if membership in a kind posited by this classification (partially) *explains* or *accounts for* the membership in a kind posited by another classification, but not vice versa. What is at stake is therefore to expound what this *asymmetric* relation amounts to, or more specifically, to clarify how kind membership in one classification *explains* or *accounts for* kind membership in another classification. While the *naturalist* answer to the question of naturalness emphasizes the epistemic aspect of natural kinds, i.e., scientific categories, I am going to show that the *asymmetric* relation that may exist between two or more classifications is not an epistemic but a *worldly* relation. To illustrate this *asymmetry*, I will compare the different classifications of nuclides in chemistry.

In the following, I will return to the two different versions of taxonomic pluralism I discussed in Chapter 1, namely the modest version of Khalidi and the radical version of Boyd. I will first look at how Khalidi interprets the relation between the different classifications of nuclides and explain why it fails to establish taxonomic pluralism. This is because Khalidi's interpretation, in relying on the notion of *projectibility*, is actually compatible with a monist reading. Next, I will survey how the radical version of taxonomic pluralism might put forward a more consistent pluralist picture than the modest version. To reply to this radical version of taxonomic pluralism, I will advance the notion of *relative fundamentality* and explicate the *asymmetry* between the different classifications of nuclides. I will conclude by pointing out how the resulting monist picture, incorporating the notion of relative fundamentality, is different from the traditional monist picture.

II. Taxonomic pluralism in chemistry

To begin with, let me explain why I turn to the classifications in chemistry in this chapter. The current chapter aims to explore to what extent taxonomic pluralism is vindicated by classifications of a given scientific domain where both the realist commitment and the disagreement commitment are secured. One such discipline is chemistry. This is because first, unlike biological kinds, chemical kinds are less controversial examples of natural kinds. For example, Ellis argues that biological kinds are not natural kinds given that the boundaries between them are not categorically distinct. On the contrary, basic kinds of chemical (and physical) substances, which exist "at a much deeper level than that of living species", do not face the same problems and are therefore genuine natural kinds (Ellis 2001, 12).[1] Similarly, Ghiselin (1974) and Hull (1978) argue that since species evolve, they should be treated as individuals rather than kinds. Whether or not these complaints against biological kinds are justified, they do not seem to apply to chemical kinds. Moreover, unlike the classifications of biological entities, the classifications of chemical entities seem to agree about how chemical entities should be individuated. In other words, the classifications of chemical entities do not encounter the problem of individuation I put forward in the previous chapter.[2] Thus the different classifications in chemistry are expected to cross-classify the same individuals.

Another reason for embracing taxonomic pluralism in chemistry, as Woody maintains, is the failure of reducing chemistry to quantum mechanics. For instance, in comparing the "valence bond approach" and the "molecular orbit approach" with

[1] Ellis' view with regard to the discreteness of chemical kinds is controversial. As Hendry indicates, not only that continuous transition between distinct chemical substances is conceivable, but more importantly, "such continuous transition between distinct chemical species is exactly how theoretical explanations depict chemical transformation" (Hendry 2015, 256). Nevertheless, whether or not continuous transformation between two chemical kinds entails that they are not objectively distinct, such issue should not bother us in the following discussion, where I will focus on the classifications of nuclides. As Hendry also admits, chemical variety among elements (presumably including nuclides) is not continuous.

[2] As I briefly pointed out in the previous chapter, a pluralist picture of chemical classification is not completely free of this problem. As Khalidi points out, "[i]n some cases, the individuals classified according to crosscutting systems of kinds may not be exactly coincident: Mass number applies to the atomic nucleus, while atomic number pertains to the whole atom (including electron orbitals)" (Khalidi 2013, 122). For the sake of the argument, in the following, I will stipulate the classification of nuclides according to their atomic numbers (i.e., the number of protons in a nuclide's nucleus) as classifying the nuclides themselves instead of the whole atom.

regard to representations of the hydrogen molecule,[3] Woody (2012) argues that neither empirical results nor the Schrödinger equation shows which approach is conceptually more satisfying than the other. While Woody's argument focuses mainly on the diverse forms of *representation* in chemical practices, she also indicates that chemical classification faces a similar indeterminacy in aligning with quantum mechanics. As she points out, "[t]he periodic law gives us 'halide' while the Schrödinger equation does not" (Woody 2014, 142). We can therefore develop a similar argument to support pluralism in chemical classifications along the same lines.[4] Given that most contemporary chemists regard quantum mechanics as the foundational theory of chemistry but no single model in chemistry can be adequately reduced to quantum mechanics, hence, no classification is privileged from the point of view of quantum calculation (suppose different models in chemistry postulates different classifications). As these different chemical classifications all have their own epistemic merits, the groupings they posit, from a naturalist point of view, are all genuine natural kinds. Since these classifications cannot be reduced to a single classification on a quantum level, therefore taxonomic pluralism is true in chemistry.

One example that will illustrate the alleged pluralist picture in chemistry is the classification of nuclides. According to Truman Kohman, who first invented the term,

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[3] In short, the "valence bond approach" regards molecule as a composition of atoms, in the sense that these atoms serve as "home bases" for electrons. As a result, each electron is associated with a particular nucleus. On the contrary, the "molecular orbit approach" sees the molecular system as an indivisible whole, where the same electron, without "home bases", is associated with more than one nucleus (Woody 2012, 431-432).

[4] At first glance, it is controversial to what extent Woody is a realist about representations in general in chemistry. For example, in analyzing the shift from the earlier "valence bond approach" and "molecular orbit" approach to the later "configuration interaction approach" in representing atoms, Woody points out that unlike the two earlier approaches, the "configuration interaction approach" no longer defines the representation by a single configuration. Under this more advanced approach, wavefunction is defined by a set of distinct configurations. Yet, as Woody argues, it is incorrect to see multiple configuration wavefunctions as a direct result of quantum theory. Prior to ontological consideration, i.e., fundamental uncertainty of the position or momentum of the moving electrons, we should take into account the "deeply pragmatic representational character of wavefunction formulations", as such representation "dramatically increases the flexibility of the final representation" (Woody 2012, 450). Nevertheless, Woody goes on to point out that "such flexibility appropriately recognizes fundamental uncertainty regarding the physical system it represents" (Woody 2012, 450).

"nuclide" refers to "[a] species of atom characterized by the constitution of its nucleus, in particular by the numbers of protons and neutrons in its nucleus" (Kohman 1947, 357). Consequently, there are at least five different ways to classify nuclides. Nuclides can be grouped together according to:

(1) the number of protons in their nuclei; thus *isotopes* of the same element will be grouped together

(2) their nuclidic weight, or the number of nucleons, i.e., protons and neutrons, in their nuclei; thus *isobars* of different elements will be grouped together

(3) the number of neutrons in their nuclei; thus *isotones* will be grouped together

(4) according to the difference between the numbers of protons and neutrons in their nuclei, thus *isodiapheres* will be grouped together

(5) according to the number of protons and neutrons in their nuclei, as well as the levels of nuclear of excitation; thus *isomers* will be grouped together

Simply put, the monism/pluralism debate concerning the classification of nuclides revolves around whether only one of these classifications is correct, or whether more than one of them are correct.

Let me begin with the first classification, namely the classification of nuclides according to the number of protons in their nuclei, i.e., their atomic number (henceforth the *Z Classification*). Should monism be the case, *the* classification to be privileged by taxonomic monists is probably the *Z Classification*, since it aligns with the way chemical elements are classified on the periodic table.[5] Indeed, instead of acknowledging isotopes of the same elements as distinct elements, the International Committee on Chemical Elements (as appointed by the International Union of Pure and Applied Chemistry (IUPAC)) decided in 1923 that the identity of the chemical elements should be determined by their atomic number instead of their mass number. This decision gave up the original classification that identifies chemical elements by their atomic weight, as was originally adopted by Mendeleev when he formulated the Periodic Law and constructed the periodic table. Focusing on the atomic number allows chemists to construct a classification that better reflects the chemical behaviors of different elements. This is because atoms with the same atomic number but different atomic mass, i.e., isotopes of the same element, generally share similar chemical properties, such as combustibility, reactivity, and preferred oxidation state. In contrast, atoms with the same atomic mass but different atomic number, i.e., isobars of different elements, generally do not exhibit the same similarity.

Nevertheless, chemistry is not only about elements; chemists also study other chemical substances such as molecules, compounds, and many other chemical kinds. More importantly, the focus of the monism/pluralism debate is not whether there exists an optimum classification for a given epistemic task but whether classifications that underwrite different epistemic practices are equally correct. Thus pluralists can happily accept that the *Z Classification* is the classification that best captures the relevant chemical properties of nuclides, while at the same time advance the claim that other classifications that do not rely on the nuclides' atomic number are equally

[5] Notice that the *Z Classification* is not completely identical to the classification of *chemical elements* according to their atomic numbers. Since the former classifies nuclides but the latter classifies elements; and a nuclide refers only the nucleus of an atom, which make up an element together with its shell electrons.

correct, as these classifications may focus on other aspects of nuclides that are of epistemic significance.

For instance, apart from chemical properties that depend on atomic charge (i.e., number of protons), chemists are also interested in non-chemical properties of an atom. And these non-chemical properties, such as entropy and thermodynamic functions, depend on atomic mass rather than atomic charge (Kragh 2000). In fact, apart from these non-chemical properties, atomic mass also aligns with some of an element's chemical properties better than atomic number. For example, one can only determine the stoichiometric amount of a chemical compound after figuring out the atomic weight of its elements (Hendry 2005, 42).[6] Therefore, for chemists who are concerned with these properties, a classification that groups nuclides into kinds according to their mass number rather than one that groups them into kinds according to their atomic number would provide a better catalogue in showcasing these properties, and allowing chemists to fulfill the relevant epistemic tasks. Pluralists therefore argue that a single classification is not sufficient to satisfy the multifarious epistemic interests of chemists, or captures the complexity of the chemical reality. So in addition to the *Z Classification*, other classifications, such as the classification of nuclides according to the number of nucleons in their nuclei, i.e., their mass number (henceforth the *A Classification*), are also needed. The crucial point, according to taxonomic pluralists, is that these classifications are no less correct than the one based on the atomic number of nuclides in depicting the chemical reality. The divisions they

[6] Stoichiometry measures the quantitative relationship between reactants and products in chemical reactions. The calculation involved is based on the principle of mass conservation, according to which the total mass of the reactants equal the total mass of the products in a reaction.

capture are no less real or fundamental than the divisions captured by the *Z Classification*.

III. Khalidi's taxonomic pluralism

Let me now turn to a concrete example. Khalidi contends that the various classifications of nuclides in chemistry testify to taxonomic pluralism. He begins his discussion by focusing on the classification according to the number of protons (i.e., the *Z Classification*) and the classification according to the number of nucleons (i.e., the *A Classification*). Khalidi maintains that while *lithium-8* can be grouped together with other isotopes of lithium, such as *lithium-6* and *lithium-7*, as is in the *Z Classification*, it can also be grouped together with other isobars of different nuclides, such as *helium-8* and *beryllium-8*, as is in the *A Classification*. According to Khalidi, "[a]lthough classification by atomic number and electric charge enables us to predict and explain its chemical properties better, classification by mass number is more efficacious for understanding its nuclear properties" (Khalidi 2013, 70). This is because *lithium-8* has different nuclear properties than other isotopes of lithium, such as *lithium-6* and *lithium-7*. On the contrary, *lithium-8* exhibits similar nuclear properties with *helium-8*, such as having a short half-life and decaying by beta-minus decay.

Yet, Khalidi further points out that the kind that groups *lithium-8* with its isobars, such as *helium-8* and *beryllium-8* is not as projectible as the kind that groups *lithium-8* with its isotopes, namely *lithium-6* and *lithium*-7. In other words, the kind that groups all nuclides with mass number 8 is not a *natural* kind, as it would not be able to fully perform the requisite epistemic tasks, such as explanation, induction, and prediction. Generally speaking, although the *A Classification* excels the *Z*

Classification in presenting a better picture of the nuclear properties of nuclides, it does not result in kinds that are as *projectible* as those posited by the *Z Classification*, i.e., the kinds posited by the *A Classification* are not as reliable as kinds posited by the *Z Classification* in underwriting successful explanation, induction, and prediction. More importantly, as Khalidi indicates, the *A Classification* is not even the best classification we can come up with in capturing the nuclear properties in question. As he suggests, instead of the kind that groups all nuclides with mass number 8, the grouping *beta-minus decay nuclides* should be used. This is because "[s]uch nuclides [i.e., beta-minus decay nuclides] have genuine causal properties in common and a number of generalization can be made about them" (Khalidi 2013, 115). Khalidi therefore concludes (Khalidi 2013, 115),

> Since classification other than by atomic number is warranted for nuclides, this gives rise to crosscutting natural kinds, each system of kinds picking out a different aspect of the phenomena being classified. These classifications are pitched at the same spatiotemporal level and concern the same individuals but they pertain to different types of causal process that the relevant individuals can enter into.

However, it is unclear to what extent the kind *beta-minus decay nuclides* is as *projectible* as, and therefore as equally *natural* as, the kind *lithium*. In suggesting that members of *beta-minus decay nuclides* "have genuine causal properties in common and a number of generalization can be made about them", Khalidi does not specify what these causal properties and generalization are. Beta-minus decay nuclides are radioactive nuclides that decay by converting a neutron into a proton and emitting an electron and an electron neutrino, resulting in a nuclide with the same mass but a different atomic number and a different atomic charge. As Khalidi rightly indicates, since only *helium-8* and *lithium-8*, but not *beryllium-8*, decay by beta-minus decay, the kind *beta-minus decay*, in excluding *beryllium-8*, is more *projectible* than the kind that groups all these three isobars together. Nonetheless, given that "[r]adioactive nuclides of many elements exhibit this characteristic pattern of decay" (Khalidi 2013, 115), the kind *beta-minus decay nuclides* would therefore encompass a wide range of nuclides with different atomic number and atomic mass: in addition to *lithium-8* and *helium-8*, it would also include nuclides as diverse as *cobalt-60*, *strontium-90*, and *thorium-231*, as well as isotopes of the same element such as *lead-210* and *lead-214*.[7] It is unclear that apart from *decaying by beta-minus decay*, what other common causal properties these nuclides with different atomic charge and atomic mass would share.

As I briefly pointed out in Chapter 2, members of a genuine natural kind should share a set of similarities instead of just a single property. Questions like how many properties should members of a given natural share, whether properties associated with a given natural kind have to be inexhaustible, how these properties should be characterized, and whether the clustering of these properties has to be grounded by the causal structures of the world are not our concern at this stage. Nevertheless, It should be clear that a single property would not suffice to ground the projectibility of natural kinds. In fact, as Khalidi himself admits, "[o]n a simple causal theory of natural kinds, a natural kind is associated with *a set of properties* whose co-instantiation causes the instantiation of other properties" (Khalidi 2013, 80). This crucial feature of natural kinds explains why they are projectible, and in particular, why "a set of generalization can be made about them". Consequently, if members of *beta-minus decay nuclides* do not share other properties apart from *decaying by beta-minus decay* (and those properties that can be logically deduced from it), then we have no reason to treat it as a natural kind at all. Moreover, even if the kind *beta-*

[7] What further complicates the matter is that although both *Lead-210* and *Lead-214* are *beta-minus decay nuclides*, they actually have different radioactive properties. In addition to beta-minus decay, *Lead-210*, but not *Lead-214*, will also undergo alpha decay to form *Mercury-206*.

minus decay nuclides turns out to share other stable similarities that cannot be logically deduced from the property *decaying by beta-minus decay*, it still may not be qualified as a genuine natural kind. For if it is not as projectible as the kind *lithium*, then perhaps it should be dismissed just like the kind *nuclides with mass number 8*.

Of course, although the kind *beta-minus decay nuclides* may not be as projectible as the kind *lithium*, Khalidi may still maintain that, since both kinds meet a minimum degree of projectibility, they are both natural kinds. Yet, while projectibility comes in degree, it is unclear how we can non-arbitrarily set up such a minimum threshold of projectibility. And even if we can come up with a threshold value of projectibility, taxonomic monists can still reject the pluralistic conclusion of Khalidi's account by appealing to the notion of projectibility itself. Given that naturalness is understood in terms of projectibility in Khalidi's account and projectibility comes in degree, then naturalness also comes in degree in Khalidi's account. In other words, natural kinds could have different degree of naturalness. For example, if *lithium* and *beta-minus decay nuclides* have a different degree of projectibility, then they should have a different degree of naturalness. So, we can rank different classifications according to the degree of naturalness (i.e., projectibility) of the kinds posited by these different classifications. If, from the perspective of natural kind realism, we further accede that the degree of naturalness of natural kinds reflects their degree of correctness in capturing the divisions in the reality, we may even go on to conclude that a single classification should be privileged, namely the classification that posits kinds that are the most natural. This is inconsistent with the pluralist picture originally conceived by taxonomic pluralists, according to which the different classifications are *equally correct*. In other words, focusing primarily on projectibility, Khalidi's account fails to exclude a "monist" reading.

IV. Boyd's taxonomic pluralism

The "monist" reading of Khalidi's pluralist account I sketched at the end of the previous section is different from the traditional monist reply to taxonomic pluralism: this "monist" reading does not counter taxonomic pluralism by arguing that either one of the two kinds at issue, namely *lithium* and *beta-minus decay nuclides,* is not a natural kind. On the contrary, it admits that both of them are genuine, real natural kinds. Yet it further suggests that the classification that posits *lithium*, i.e., the *Z Classification*, should be privileged over the classification that posits *beta-minus decay nuclides*, given that the kinds posited by the *Z Classification* are more projectible.

Without further pursuing this monist reply, at this stage, I want to turn my attention to the radical pluralist account endorsed by Boyd. This is because, although this "monist" reply points to a new way in responding to taxonomic pluralism, it hinges on the assumption that the naturalness of theoretical groupings depends exclusively on a single criterion, namely projectibility. However, it is unlikely for those who adopt a radical pluralist account to accept this assumption in the first place.[8] We have already seen in Chapter 1 that Khalidi's modest pluralist account presupposes that natural kinds are theoretical groupings that answer to a particular type of epistemic tasks: natural kinds are groupings that reveal the causal structures of the world. According to Khalidi, natural kinds are "nodes in causal networks, serving

[8] Thus for the same reason, Boyd and Brigandt would probably reject Khalidi's claim that isobars are "not good candidate for natural kinds" merely because they are weakly projectible kinds (Khalidi 2013, 115).

either as starting points of branching networks or as endpoints (etiological kinds), or both" (Khalidi 2013, 200). And this is why projectibility is central to the understanding of natural kinds in Khalidi's account.

On the contrary, Boyd's "accommodationist" account of natural kinds capitalizes on the idea that different investigative contexts aspire to different epistemic goals. And these epistemic goals are not confined to the discovery of the causal structures of the world. Hence, natural kinds are not only restricted to "nodes in causal networks"; rather, they are understood more broadly as theoretical groupings posited by the "inferential architecture" of a given disciplinary matrix that successfully "accommodates" to the relevant causal structures of the world. Should this be the case, it is mistaken to posit any single, universal standard such as projectibility to measure the naturalness of theoretical groupings across different investigative contexts or disciplinary matrices. This is because, in order to achieve different epistemic goals, theoretical groupings are expected to perform different tasks in different investigative contexts; while a minimum degree of projectibility may be necessary for a grouping to perform the epistemic tasks in question, it is by no means the sole relevant factor. Recall the Phylo-Phenetic Species Concept (PPSC) we came across in Chapter 1. According to Ereshefsky & Reydon (2015), the PPSC is adopted to facilitate stable identification rather than to trace the causal mechanism underlying bacterial species. If Ereshefsky & Reydon are correct, then projectibility cannot be the dominant consideration why microbes are classified according to the PPSC. Similarly, we can conclude that, for Boyd, even if the kind *beta-minus decay nuclides* may not be as projectible as the kind *lithium,* it can still be regarded as a natural kind so far as it underwrites various scientific practices in an appropriate investigative context. More importantly, the kind *minus decay nuclides* will be no less *natural* than the kind *lithium*, for the epistemic agenda that calls for it may not require the same degree of projectbility as the epistemic agenda that calls for the kind *lithium*.

In Boyd's account, how things should be classified into natural kinds ultimately depends on the unique epistemic concerns scientists have in a given investigative context, and these concerns do not always call for a classification with the most projectible groupings. Thus unlike Khalidi's account, for Boyd, projectibility should not be regarded as an ultimate standard in evaluating a given scientific classification. In fact, according to Boyd's "accommodationist" account, not only does projectibility not provide us a universal standard to compare classifications across different investigative contexts, it is in fact unlikely that any such universal standard can be formulated, given that scientists have diverse epistemic concerns in different investigative contexts. In other words, classifications adopted in different investigative contexts are on a par with each other as long as they each adequately address the unique epistemic concerns at issue. In this regard, Boyd's more radical account seems to offer a more promising picture of taxonomic pluralism than the modest account of Khalidi. Unlike Khalidi's approach, Boyd's approach does not face the monist objection I sketched at the end of the previous section. Without presupposing any universal criterion in sanctioning natural kinds, Boyd's account does not leave room for monists to construct a universal scale according to which classifications across different investigative contexts can be ranked. In the next two sections, I am going to reply to this radical version of taxonomic pluralism. I shall explain how we might still rank these different classifications of nuclides in terms of relative fundamentality.

V. Relative fundamentality revisited

In Chapter 2, I developed a notion of relative fundamentality to spell out the reality of natural kinds. I argued that a genuine realist commitment to natural kinds should regard natural kinds as more fundamental than their members. And the reason for believing that natural kinds are more fundamental than their members is because *membership* in a given natural kind *explains* the co-existence of a given set of properties typically found among members of that kind. Thus the natural kind *electron* is real because membership in it *explains* the coexistence of properties such as *having a mass of* 9.109×10^{-31} *kg*, *having an electric charge of* -1.602×10^{-19} *C*, and *having an intrinsic angular momentum of ½* among its members, rather than the other way round. On the contrary, the kind *white cubic thing* is not a real kind, because an object is white in color and cubic in shape not in virtue of being a member of the kind *white cubic thing*. In other words, the kind *membership* of *white cubic thing* does not *explain* the distinctive feature according to which its members are picked out, namely *being white in color* and *cubic in shape*. Simply put, what distinguishes a natural kind from a non-natural one is the *direction of explanation:* in the case of a natural kind, it is the *membership* in that kind that *explains* the fact that members of a given natural kind possess certain characteristic features; in the case of a non-natural kind, it is the other way round, i.e., the fact that a given set of individual entities all possess a certain features *explains* the fact that they are members of a particular kind.

Once we accept that *membership* in a given natural kind *explains* the fact that its members possess certain characteristic features, we can go on to ask whether the *membership* in a given natural kind may also explain the *membership* in another natural kind. If this is the case, then we can assert that some natural kinds are more fundamental than other natural kinds. And suppose we can further show that the natural kinds of a given classification are more fundamental than the natural kinds of another classification, then we can establish the claim that one classification is more fundamental than another classification. What is at stake, of course, is how to formulate an appropriate explanatory relation that would allow us to establish the claim of relative fundamentality between different *classifications*. In the following, I will outline this explanatory relation before further illustrating it with a concrete example in the next section.

Let me begin by looking at the relata of this explanatory relation. First, I want to point out that any explanatory relation focusing on individual kinds would not be sufficient to vindicate the general claim that one classification is more fundamental than another classification. This is because even if we are able to show that a particular kind of a given classification is more fundamental than another kind of a different classification, it still does not mean that the kinds of the first classification are more fundamental than the kinds of the second classification. Indeed, it is unclear which particular groupings of the classifications in question would allow us to establish the general claim that one *classification* is more fundamental than another *classification*. And it is practically impossible for us to compare every single grouping of one classification with every single grouping of another classification.

So, in order to establish the general claim that one classification is more fundamental than another classification, we have to shift our focus from the level of kinds to the level of *classifications* in formulating the requisite explanatory relation. In arguing why natural kinds are more fundamental than their members, I appeal to *membership* in a given kind to explain why members of that kind share certain characteristic properties. Similarly, in order to establish that kinds in a classification are more fundamental than kinds in another classification, we also have to show that *membership* in a kind in one classification explains the *membership* in a kind in another classification. Yet, we should no longer focus on membership in any particular kind, but membership in a kind in general according to a particular classification.

Such a *membership* in a kind in a given classification can be understood as how things are being grouped into kinds in that classification. For example, membership in a kind in the *Z Classification* is defined by the number of protons in a nuclide's nucleus, for it is based on the number of protons in a nuclide's nucleus that an individual nuclide is being classified into a particular grouping in the *Z Classification*. Therefore, if we want to prove that a classification *P* is more fundamental than another classification *Q*, we have to show that membership in a kind in classification *P* explains the membership in a kind in classification *Q*, i.e., how individuals are being grouped into kinds in classification *P* explains how individuals are being grouped into kinds in classification *Q*.

Next, let me turn to the explanatory relation itself. The explanatory relation I appeal to in showing that natural kinds are more fundamental than their members is metaphysical in nature. As I suggested in Chapter 2, this metaphysical explanation is similar to a *grounding* relation: membership in a natural kind *grounds* the fact that members of that kind share stable similarity. So it appears that a metaphysical explanatory relation like grounding is needed if we want to prove that one classification is more fundamental than another classification.

However, at this stage, I do not want to commit myself to formulating a metaphysical grounding relation to spell out the notion of relative fundamentality between different classifications. There are several reasons for me to back off from this commitment. First, the grounding relation concerning kinds has already been widely discussed in the literature. According to Rosen, if we understand a species (not necessarily a biological species) as being defined by genus and differentia, then it is correct to say that the genus *grounds* its species. This is because in this "old-fashioned", namely Aristotelian, sense, "a thing must belong to the species in part because it belongs to the genus", but not the other way round (Rosen 2009, 128). For example, the fact that a geometrical figure is a square (species) is *grounded* in the fact that this geometrical figure is an equilateral rectangle (genus). Given that both species and genus are taken as kinds here, we therefore have a grounding relation between kinds. Yet, this is not the type of explanatory relation between kinds I am concerned with, as it is one between kinds of the same classification. My concern, on the contrary, is the relation between kinds of different classifications. Thus to avoid confusion, in the following, I will avoid employing the label "grounding" in my exposition of the explanatory relation at issue.

The second, more crucial reason has to do with the explanatory power of the notion of grounding. I already presented this critique in Chapter 2. Let me briefly recap it here. According to Koslicki (2015), the phenomena that are collectively referred to as "grounding" are not homogeneous. For instance, while both determinate-determinable and species-genus are regarded as genuine cases of "grounding", they involve different directions of explanation. Thus it is unclear to what extent these phenomena are really unified by the presence of a single relation, namely "grounding". In this regard, any single notion of "grounding" that tries to subsume all these heterogeneous phenomena would not be fine-grained enough to truly illuminate the notion of relative fundamentality. Likewise, Wilson (2014) also questions whether an all-encompassing, unified notion of "grounding", namely "(big-G) Grounding", is of much use to philosophical analysis of fundamentality. As she indicates, in each particular case, it is always the various "(small-g) grounding" relations (i.e., specific metaphysical relations such as composition, set membership, and determinable-determinate) that do the explanatory job.

Both critiques by Koslicki and Wilson imply that any explanatory relation attempting to vindicate the claim of relative fundamentality must pay attention to the specific entities involved in each particular case. Different entities are likely to call for different types of metaphysical relations. It is for this reason that the grounding relation I put forward in Chapter 2 is a "(small-g) grounding" relation, i.e., it is a grounding relation specifically concerned with the relative fundamentality between natural kinds and their members. It is unlikely that this "(small-g) grounding" can be directly applied to what I am dealing with here, namely the relative fundamentality between kinds in different *classifications*. In fact, following the advice of Koslicki and Wilson, it would not be of much help to model the requisite explanatory relation after any existing notion of grounding relation, be it a "big-G" or a "small-g" one.

After all, it may not even be appropriate to refer to the explanatory relation between different classifications, if any, as a metaphysical grounding relation at all. The "(small-g) grounding" relations, such as supervenience, part-whole relation, compositions, are all concerned with real entities in the world. While as natural kinds realists, we may gesture at a metaphysical grounding relation between different *natural kinds*, it is unclear to what extent it is accurate to speak of a metaphysical relation between *classifications*. As we have seen, according to the radical naturalist approach to natural kinds, how things are being classified largely depends on the epistemic concerns of scientists in a given investigative context. So understood, classifications are epistemic in nature; they are not real entities in the world. Given that our current goal is to put forward a critique of this approach, it is thus inappropriate to assume at the outset that we can talk of a metaphysical relation between classifications, as if classifications are real entities like natural kinds.

Yet, a closer look at our current goal seems to suggest that, it is actually not necessary for us to appeal to a metaphysical relation such as a "(small-g) grounding" relation in order to establish the claim that one classification is more fundamental than another classification. According to proponents of the radical version of taxonomic pluralism, different ways of classifying the entities in a scientific domain are on a par with each other because they are intended to answer to different epistemic concerns of scientists in different investigative contexts. Thus what is at stake here is to demonstrate not only that some ways of classifying things in a scientific domain actually account for the other, but also, more importantly, that this explanatory relation is independent of the epistemic interests of scientists in a given investigative context, despite that classifications themselves are epistemic in nature. Thus rather than committing myself to the assumption that the explanatory relation between different classifications is metaphysical in nature, what I am going to show is merely that this explanatory relation is not an epistemic relation, but a worldly one.

Although I leave it open whether this explanatory relation is metaphysical in nature, it is clearly not causal in nature. This is because causal explanation is supposed to be diachronic,[9] yet whether a classification is chronologically prior to another does not seem to have any philosophical bearing with regard to their degree of fundamentality. Take the classification of nuclides as an example. Clearly, the answer to the question whether the *Z Classification* or the *A Classification* is more fundamental does not depend on which classification came first in the history of chemistry. Indeed, if there is an explanatory asymmetry between the two classifications, it cannot be due to the fact that grouping nuclides with the *Z Classification* eventually *causes* chemists to group nuclides with the *A Classification*. This is because how the nuclides should be classified is supposed to be independent of how they were classified previously.

Nevertheless, we should be aware that, although the explanatory relation in question is non-causal, whether it has to be backed by a non-causal relation is another matter. At this stage I want to leave open the possibility that this explanatory relation may be backed by a causal relation, rather than a metaphysical relation. The main reason for leaving this possibility open is to accommodate biological classifications. I will say more about this concern below. Right now I will return to the classification of nuclides in chemistry. I will expound in what sense one way of classifying nuclides *explains* another way of doing so, and why this explanatory relation would allow us to establish the claim that one way of classifying nuclides is more fundamental than the other.

[9] Please refer to footnote #22 in Chapter 2 for a brief discussion of possible exceptions.

VI. The classification of nuclides

Let us pick up where we left off. Recall the reason why Khalidi replaces the kind *nuclides with mass number 8* with the kind *beta-minus decay nuclides*. As he indicates, “isobars are not good candidates for natural kinds, or they are at best weakly projectible natural kinds” (Khalidi 2013, 115). This is because most isobars have one beta-decay stable nuclide that does not tend to undergo beta-minus decay. For example, unlike *lithium-8* and *helium-8*, *beryllium-8* decays into two alpha particles. Although Khalidi is correct in pointing out that isobars are weakly projectible kinds, he is mistaken in replacing the kind *nuclides with mass number 8* with the kind *beta-minus decay nuclides* in an attempt to justify taxonomic pluralism.

What I am concerned with here is not whether the kind *beta-minus decay nuclides* is as *projectible* as the kind *lithium*, but the fact that the kind *beta-minus decay nuclides* is simply the wrong type of grouping to start with. This is because both the kinds *lithium* (i.e., *nuclides with atomic number 3*) and *nuclides with mass number 8* are identified by their subatomic constitutions, such as the number of protons or the number of nucleons. On the contrary, the kind *beta-minus decay nuclides* focuses on a nuclear property of nuclides, i.e., the property of *decaying by beta-minus*. It should be noted that such nuclear property is supposed to be explained by the subatomic constitution of a nuclide. As the classification of nuclides according to atomic number as well as the initial attempt of classifying nuclides by their mass number suggest, chemists do not only aim to group together nuclides that share similar manifest properties, i.e., their chemical and nuclear properties, but also to explain these manifest properties.[10] Clearly, the grouping *beta decay nuclides* fail to

[10] I do not thereby advocate a microstructural approach to natural kinds, like Kripke and Putnam do. According to this microstructural approach, membership of a given natural kind is conferred by micro-structural properties, for instance, to be gold is not just to possess the observable properties such as being golden and shinny. Instead, to be gold is to have atomic number 79. Kriple and Putnam advance

meet this objective. Indeed, Khalidi (2013, 2015) labels these manifest properties as "derivative" or causally "secondary", and calls the properties associated with the subatomic constitutions of nuclides (e.g., their atomic number and mass number) as "core" or causally "primary" properties. Therefore, an appropriate substitution should also be a classification that focuses on the same level of explanation like the *Z Classification* or the *A Classification*, i.e., a classification that rely on the "core" or causally "primary" properties. One such substitution would be a classification according to the neutrons to protons ratio of the nuclides' nuclei (henceforth *N-Z Classification*),[11] given that whether a nuclide would undergo beta-minus decay largely depends on the neutron-proton ratio of its nucleus (Laird 2008, 860).[12]

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this microstructural approach not only to chemical kinds but also to biological species (Kripke Putnam). As Hendry rightly points out, such micro-structural approach is probably correct in scientific classification of chemical elements (nuclides included), but clearly not appropriate in the case of biological species and chemical compounds. This is because there is a "wide microstructural variation within biological species" (Hendry 2006, 865); and isomerism in chemical compounds makes elemental composition inadequate to account for kind membership (Hendry 2006, 869). Yet, it is correct to say that a scientific classification of chemical kinds do aim to account for the manifest properties of chemical substance by referring to the structures on a molecular level. For generally speaking, "[c]hemistry is concerned with the properties and behaviour of chemical substances, and explaining both in terms of their structure at the molecular scale" (Hendry 2015, 252). And more specifically, as Woody points out, "the periodic law, by facilitating central aspects of inquiry, plays a significant role in *explaining* the properties of the elements" (Woody 2015, 139, italic original).

[11] I do not consider the classification of nuclides according to the number of neutrons in their nuclei as an alternative *scientific* classification alongside *Z Classification* and *N-Z Classification*. This is because although nuclear properties such as nuclear mass and nuclear cross-sections are determined by the number of neutrons, these properties are better captured by the number of nucleons than just by the number of neutrons in their nuclei. Therefore, with regard to the epistemic tasks of explaining and inferring a nuclide's nuclear properties, the classification according to the number of nucleons, i.e., *A Classification*, does a better job than the classification according to the number of neutrons. Nevertheless, I will omit *A Classification*, together with the classification according to the difference between the number of protons and the number of neutrons in the nuclei. This is because my account, which demonstrates that *Z Classification* is more fundamental than *N-Z Classification*, would similarly show that *Z Classification* is more fundamental than both *A Classification* and the classification according to the difference in the number of protons and the number of neutrons. Lastly, I do not examine the classification according to the nuclides' level of excitation since it is a classification that can be subsumed under *Z Classification*. Thus there will not be any genuine crosscutting between this classification and *Z Classification*. In other words, this classification will not constitute a case for pluralism.

[12] Another factor that determines the nuclear stability of a nuclide is the actual number of protons and neutrons in its nucleus. Nuclides having 2, 8, 20, 50, 82, and 126 protons or neutrons tend to have stable nuclei (Laird 2008, 862).

Understanding this mistake of Khalidi allows us to see why the *Z Classification* is more fundamental than other classifications of nuclides, and in particular, the *N-Z Classification*. With regard to the kinds posited by the two different classifications of nuclides, stable similarities can be observed among their members. For instance, members of a given grouping in the *Z Classification* have similar chemical properties, such as flammability, toxicity and reactivity. Likewise, members of a given grouping in the *N-Z Classification* possess similar nuclear properties, such as radioactivity and the kind of decay they would undergo. Given that groupings posited by these two classifications are genuine natural kinds, memberships in these kinds are supposed to *explain* the stable similarities, or the coexistence of the aforementioned properties shared by their members. It is in this sense that the kinds posited by these two classifications are all real, as the kinds themselves are more fundamental than their members. Nevertheless, this is not the end of the story. If relative fundamentality between natural kinds and their members is established on the fact that membership in a natural kind *accounts for* the stable co-existence of properties shared by its members, but not vice versa, then relative fundamentality between groupings of different classifications can be similarly established if membership in a kind of one classification *accounts for* membership in a kind of another classification.

So in what sense does membership in the *Z Classification* ***explain*** or ***account for*** membership in the *N-Z Classification*? Here is a quick answer: the former classification tracks a more fundamental property than the latter classification. As I have just mentioned, chemists are concerned with capturing the different types of properties of nuclides by classifying them according to their subatomic constitution.

Focusing on the subatomic constitution of nuclides, it should be clear that the neutron-proton ratio of a nuclide's nucleus (and likewise, its number of nucleons or atomic mass, as well as the difference between the number of protons and the number of neutrons) depends on the number of protons in its nucleus, but not vice versa. Although the radioactive properties of a nuclide are directly determined by its nucleus' neutron-proton ratio, its nucleus' neutron-proton ratio is partly determined by its number of protons. Thus the number of protons in a nuclide's nucleus not only determines the nuclide's chemical properties, but also (partially) determines the nuclide's radioactive properties. Of course, this does not mean that the chemical properties of a nuclide determine its radioactive properties; rather, both sets of properties have a common determinant. The *Z Classification* is therefore more fundamental than the *N-Z Classification*, given that it classifies nuclides by directly referring to this common determinant, i.e., the number of protons in their nuclei.

A more detailed answer as to why the *Z Classification* ***explains*** or ***accounts for*** the *N-Z Classification* requires us to take a closer look at the relation between the number of protons in a nuclide's nucleus and its neutron-proton ratio. While the neutron-proton ratio of a nuclide's nucleus explains its radioactive properties, i.e., whether it has a stable nucleus, and whether it will undergo alpha, beta-plus or beta-minus decay, its number of protons also plays a key role in explaining these phenomena. Nuclides with different neutron-proton ratios undergo different types of decay in order to release the extra energy in their nuclei and get closer to a ratio that is more stable. For instance, a nuclide that has a low neutron-proton ratio is likely to undergo beta-plus decay during which a proton of its nucleus will turn into a neutron and a positron. On the contrary, a nuclide that has a high neutron-proton ratio is likely to undergo beta-minus decay during which a neutron of its nucleus will turn into a proton and an electron. A nuclide with high atomic number is likely to undergo alpha decay, during which an alpha particle is released (Laird 2008, 861).

Apart from the fact that we must take into consideration the number of protons when calculating the neutron-proton ratio in order to determine the radioactive properties of a nuclide, the number of protons in a nuclide's nucleus also explains (partially) why nuclides with a certain neutron-proton ratio tend to be unstable. The nucleus of a nuclide is made up of protons and neutrons. Since protons are positively charged, they expel each other due to the electric force (i.e., Coulomb's force) between them. Although a nuclear force, i.e., the residual of the strong force from gluons that bind the three quarks that make up a proton, also acts between protons, extra nuclear force provided by the neutrally charged neutrons is needed to hold the protons together in a nucleus. Thus nuclei with too high or too low a neutron-proton ratio are unstable due to the imbalance of nuclear force in its nucleus. Furthermore, nuclides possessing more than twenty protons in general require more neutrons to stabilize the repelling electric force between their protons. Therefore, instead of a neutron-proton ratio around 1, a stable nuclide possessing more than twenty protons generally has a stable neutron-proton ratio around 1.5.

As I have shown above, the number of protons in a nuclide's nucleus is partially determined by its neutron-proton ratio, but not vice versa. Therefore, while the stable coexistence of radioactive properties of a given kind in the *N-Z Classification* is *explained* by the very membership of that kind, this very membership, which is spelt out in terms of the neutron-proton ration, is in turn (partially) *accounted for* or *explained* by the number of protons, which defines the membership in the *Z Classification*. Yet, the reverse does not hold, since the neutron-proton ratio of a nuclide's nucleus does not determine its number of protons, the membership in the *N-Z Classification* does not (partially) *explain* the membership in the *Z Classification*. We therefore find an *explanatory asymmetry* between the *Z Classification* and the *N-Z Classification*; the former is therefore more fundamental than the latter.

Consider the nuclide *lead-214* as an example. Under the *N-Z Classification*, *lead-214* will be classified into the kind *nuclides having a neutron-proton ratio larger than 1.5* (for the neutron-proton ratio of *lead-214* is 1.609) since nuclides that have a neutron-proton ratio larger than 1.5 are likely to undergo beta-minus decay. Under the *Z Classification*, *lead-214* will be classified together with its other isotopes into the kind *lead*, for they all share similar chemical properties. However, the fact that *lead-214* has a neutron-proton ratio of 1.609 is determined by its number of neutrons and number of protons, but not vice versa. More interestingly, the fact that nuclides having a neutron-proton ratio larger than 1.5 are likely to undergo beta-minus decay is (partially) explained by the number of protons as well, as I have mentioned above. Thus the fact that *lead-214* is being classified into a particular kind in the *N-Z Classification* and the fact that members of that particular kind, namely *nuclides having a neutrons to protons ratio larger than 1.5*, would share stable radioactive properties, are both (partially) explained by its number of protons, i.e., 82. And this particular number of protons represents its kind membership in the *Z Classification*. Therefore, we can say that it is (partially) *in virtue of* being a member of the kind *lead* in the *Z Classification* that a nuclide of *lead-214* is a member of the kind *nuclides* *having a neutron-proton ratio larger than 1.5*, a kind that belongs to the *N-Z Classification*.

VII. Explanatory asymmetry

I argued in the previous section that the *Z Classification* is more fundamental than the *N-Z Classification*. This is because how nuclides are classified in the *Z Classification* partially *accounts for* how nuclides are classified in the *N-Z Classification*, but not vice versa. This is in contrast to the picture envisioned by the more radical version of taxonomic pluralism. According to the more radical version of taxonomic pluralism, since scientists have different epistemic concerns in different investigative contexts, they put forward different classifications. More importantly, these different classifications are on a par with each other, i.e., none of them is more correct or fundamental than the others, since each of them is supposed to answer to a specific epistemic agenda. In this regard, classifications are largely tied to the epistemic concerns of scientists in a given investigative context. So the question is, in what sense is the explanatory asymmetry I depicted in the previous section independent of the specific epistemic concerns of scientists in a given investigative context, so that it would allow us to vindicate the claim that some classifications are more fundamental than the others? In this section, I am going to further clarify this explanatory relation between different classifications of a given scientific domain, as well as to elucidate why it would allow us to vindicate the claim that one classification is more fundamental than another.

First, based on my illustration in the previous section, we should note that the way things are classified in a more fundamental classification does not *fully* account for the way things are classified in a less fundamental classification. On the contrary, the way things are classified in a more fundamental classification only *partially* accounts for the way things are classified in a less fundamental classification. This is in fact a desirable result. If we demand that a classification is more fundamental than another classification only when it *fully* explains the less fundamental classification, then we are assuming that relative fundamentality has to be understood in terms of reduction (in an eliminative sense). For if the way things are being classified in one classification can fully explain the way things are being classified in another classification, then the latter classification can be completely reduced to, and subsequently replaced by, the former classification. However, eliminative reduction should be out of the picture once we take into consideration the multifarious epistemic goals scientists attempt to achieve with their diverse classificatory practices. According to the radical version of taxonomic pluralism, how things are classified in a particular scientific classification is tied to the specific epistemic concerns of scientists in a given investigative context. It is therefore unlikely that a classification adopted in one investigative context, even if it was a more fundamental one, would fully account for a classification adopted in another investigative context, where scientists are working under a different epistemic agenda. For instance, neither the *Z Classification* nor the *N-Z Classification* I discussed in the previous section can be reduced to the other. Chemists have good reason to put forward these two classifications separately, given that the two classifications answer to different epistemic interests of the chemists. The *Z Classification* aims to capture the chemical properties of nuclides while the *N-Z Classification* is tailored for studying their nuclear properties. Yet, my example also shows that the *Z Classification* partially *accounts for* the *N-Z Classification*, and is therefore the more fundamental classification among the two.

Once we understand the reason why the relative fundamentality between different classifications is based on the fact that one classification *partially* accounts for another classification, it should also be clear why the explanatory asymmetry involved is independent of scientists' specific epistemic concerns that motivate different classifications in different investigative contexts. Despite the fact that the *Z Classification* and the *N-Z Classification* are motivated by different epistemic concerns, the explanatory asymmetry between them, as I portrayed in the previous section, is based on facts in the world rather than the epistemic interests of scientists. Of course, it is true that if radioactive isotopes were more abundant on Earth or chemical properties of nuclides were mostly inert due to an unfavorable atmospheric condition, scientists might not even have come up with the *Z Classification*, and the *N-Z Classification* might have been treated as the only correct classification of nuclides in chemistry. Yet, so far as the world follows the same physical and chemical laws, i.e., so far as a nuclide's nucleus is composed of protons and neutrons, then a classification that groups nuclides according to the number of protons in their nuclei will always *explain* the classification that groups nuclides according to the neutron-proton ratio in their nuclei, provided that these two classifications answer to chemists' epistemic interests. Therefore, while a full explanation as to why entities in a given scientific domain are classified in a particular way might vary from context to context, the (partial) explanatory relation between how things are being classified in different classifications is not. The explanatory asymmetry between classifications is therefore context independent.[13]

Despite the fact that the explanatory relation between two given classifications is context independent and non-causal, I leave it open whether it is a metaphysical relation like grounding. As I mentioned above, the crucial issue here is not whether such an explanatory relation is metaphysical in the same sense as a grounding relation, but whether such an explanatory relation is backed by a worldly relation or merely based on the epistemic interests of scientists. Yet, there is another important issue: while the explanatory relation between two different classifications is non-causal, does the worldly relation that backs this explanatory relation also have to be non-causal? In my example, the relation that backs the explanatory asymmetry between the *Z Classification* and the *N-Z Classification* is non-causal. It is synchronic and it resembles a relation of constitution: protons, being part of a nuclide's nucleus, also determine (partially) its neutron-proton ratio. Yet, this constitutional or dependence relation seems to be unique to chemical classification, especially when chemists are interested in accounting for various chemical phenomena in terms of the atomic or subatomic constitutions of the substances in question. However, this constitutional approach may not be relevant in the classification of other scientific disciplines. Consider the different classifications of species in biology such as the

[13] Van Fraassen famously argues that explanation is not *sui genris* relation between theory and fact, but only an application of science, thus explanatory asymmetry is not fact of the world but a feature of our explanatory interest and therefore is context dependent. Van Fraassen describes a case that the shadow of the pole would explain the height of the pole, in order to show that the explanatory asymmetry ultimately depends on our explanatory interest. According to Van Fraassen, "[A]n explanation is an answer to a why-question" (Van Fraassen 1977, 134). What answer would be appropriate ultimately depends on the context under which the why-question is asked. In the regard, the direction of explanation ultimately depends on the epistemic context under which the "why-question" is asked. Yet, it should be clear that the explanatory relation between classifications I am concerned with here does not intend to answer any "why-question". Thus the explanatory asymmetry between different classifications is not epistemic in nature. On the contrary, it reflects the structure of the world.

phenetic classification, and the evolutionary classification. Should the way organisms are being classified in one classification partially *accounts for* the way organisms are being classified in another classification, this explanatory relation is likely to be backed by a causal determination instead of a constitutional determination (as it is in the case of the classification of nuclides). Suppose an evolutionary classification, which groups organisms into monophyletic groups, is more fundamental than a phenetic classification, which groups organisms into kinds according to their morphological similarities, then it can only be the case that having the same common ancestor partially accounts for the fact that a group of organisms have similar morphological features.[14] Of course, whether this is really the case, can only be verified by biological studies, and it is beyond the scope of the current chapter. My goal here is to illustrate how the claim that one classification is more fundamental than another classification can be established by appealing to an explanatory relation between the memberships posited by these two classifications. Yet, how membership is construed vary from classification to classification. Therefore, if there exists any explanatory relation between memberships of different classifications, it would be backed by different types of worldly relation, e.g., constitution, composition, or causation.

Moreover, this explanatory relation, despite being partial, complies with the naturalist approach to natural kinds. Despite resembling a metaphysical explanation in

[14] What I have in mind here is something similar to the "evolutionary explanation" as discussed by Shaheen (2017) and Weber *et. al.* (2005). According to Shaheen (2017), "evolutionary explanation" accounts for "an object x's having a property P at time t by adducing a causal interaction at an earlier time t_0 in which x took on a suite of properties $Q_1, \ldots, Q_n$ which jointly triggered an evolution resulting in x having property P at time t" (Shaheen 2017: 564). If the explanatory asymmetry that backs the notion of relative fundamentality is supported by causal explanation, then it seems that a classification that groups individual objects according to their properties (e.g., $Q_1, \ldots, Q_n$) at t_0 would result in a classification that is more fundamental than one that groups the same individual objects according to their properties (e.g., P) at t.

being non-causal, this explanatory relation between different classifications does not depend on any *a priori* presupposition about reality. Whether one classification accounts for another classification, in the way I illustrated above, ultimately depends on empirical investigations, instead of any *a priori* consideration or metaphysical assumption. It is through empirical discoveries of chemists that we come to find out that radioactive properties of a given nuclide depends on the neutron-proton ratio of its nucleus, which further depends on the number of protons of its nucleus. And the number of protons of a nuclide's nucleus is exactly what determines the membership in the *Z Classification*.[15]

VIII. Conclusion

I argued in this chapter that securing the *realist* commitment and the *disagreement* commitment is not sufficient to vindicate taxonomic pluralism. This is because different classifications in a scientific domain may differ in their degree of fundamentality, i.e., they may violate the *equality commitment*. This notion of relative fundamentality, as I illustrated with the classification of nuclides in chemistry, is spelled out in terms of an explanatory asymmetry between memberships of different classifications, i.e., how entities are being grouped into kinds in different classifications.

[15] This also explains the mistakes of the original periodic table formulated by Mendeleev based on the atomic mass of the elements, and why Mendeleev was *wrong*. As Hendry points out, Mendeleev believes that the chemical relationship between atomic mass and chemical behaviors as explaining the empirical periodic law. And this causal explanation is "erroneous of course" (Hendry 2005, 45). This is because, again, "the correlation between atomic weight and elemental behaviour encoded in the periodic law reflects their joint determination by a common cause: nuclear structure. Nuclear structure determines chemical behaviour through electronic shell structure" (Hendry 2005, 45). The reason for this mistake is that neutron was not discovered at that time and Mendeleev believes that different elements are composed of different types of atoms. He mistook the average weight of a population of atoms for the weight of individual atoms.

The appeal to the notions of relative fundamentality and explanatory asymmetry is not *ad hoc* proposals. We have already come across these notions in Chapter 2. Yet, given that I am concerned with the relative fundamentality between different *classifications*, I do not attempt to show that the explanatory relation behind it is just the metaphysical (grounding) relation I formulated in Chapter 2. Indeed, the crucial issue here is whether this explanatory relation is equally tied to the specific epistemic concerns of scientists in a given investigative context, as scientific classifications themselves are, according to taxonomic pluralists. I demonstrated in this chapter that it is not. If there exists any explanatory relation between different classifications in the sense I portrayed above, such explanatory relation should base on a worldly relation. Nevertheless, what type of worldly relation it is, for example, whether it is causal or non-causal, varies from case to case. Thus the explanatory relation between different classifications can only be verified by empirical studies.

My goal in this chapter is therefore modest. Instead of arguing that an explanatory asymmetry always exists between any two scientific classifications in a given scientific domain, I merely pointed out what is being overlooked by taxonomic pluralists, namely the *equality commitment*. According to the radical version of taxonomic pluralism, the diverse epistemic concerns of scientists in different investigative contexts call for conflicting ways of classifying the same collection of entities, and these classifications are all equally legitimate. Instead of privileging any particular classification like their monist opponents, taxonomic pluralists maintain that these conflicting classifications are on a par with each other, as they are independently sanctioned by different epistemic agendas. Unlike Khalidi's modest version of taxonomic pluralism, the radical version of taxonomic pluralism does not posit any single epistemic criterion, such as projectibility, that would allow us to further rank these different classifications. Without any overarching epistemic aim, it seems that we do not have any context-independent ground to claim that one classification is more fundamental than another classification. Nevertheless, in the example of the classification of nuclides in chemistry I presented above, I showed that we can actually formulate a context-independent explanatory relation between membership of different classifications, and this explanatory relation would allow us to rank different classifications in terms of relative fundamentality.

While the challenges I put forward in the previous chapters draw on the fact that the two fundamental convictions of taxonomic pluralism, namely the realist commitment and the disagreement commitment, would actually be undercut by the naturalist approach that motivates a pluralist stance, the challenge I present in this chapter is of a different nature. Instead of measuring taxonomic pluralism against its own standard, I bring in the notion of fundamentality, which is traditionally associated with a monist worldview. Yet, my account does not thereby endorse monism. This is because the notion of fundamentality I rely on is *relative fundamentality*. Unlike absolute fundamentality, the notion of relative fundamentality does not presuppose a fundamental layer of reality where the chains of dependence terminate. Moreover, whether one classification is more fundamental than another classification, as I have shown, is a worldly fact to be discovered by empirical studies rather than by any *a priori* speculation.

Therefore, on the one hand, my account differs from the traditional monist account: I maintain that theoretical kinds posited by the less fundamental classifications are nevertheless real. On the other hand, my account also departs from the typical pluralist account: I believe that theoretical kinds posited by different classifications, even though they are equally real, may differ in their degree of fundamentality. My account can therefore be described as *tempered monism*. Or one may prefer to call it *tempered pluralism*, in favor of the fact that there is more than one classification of which kinds are real.

Conclusion

As I indicated in the Introduction, the goal of this dissertation is not to vindicate taxonomic monism—at least not taxonomic monism traditionally understood. Traditionally, proponents of taxonomic monism maintain that *the* correct classification in a given scientific domain is the classification that captures the natural-kind structure in that domain. By appealing to various metaphysical principles, e.g., essentialism, they argue that the universe possesses a mind-independent natural-kind structure. Instead of defending this natural-kind structure along the lines of these monists, I embrace the naturalist approach adopted by taxonomic pluralists in thinking about natural kinds. According to this naturalist approach, natural kinds are understood as groupings that underwrite successful scientific practices, such as explanation, induction, and prediction. I identify three different commitments that are necessary for taxonomic pluralism and argue that the naturalist approach alone fails to fulfill each of them. The three commitments are: the *realist commitment*, the *disagreement commitment*, and the *equality commitment*. The realist commitment suggests that *natural kinds* are *real*. The disagreement commitment claims that, in a domain where taxonomic pluralism is true, the different classifications involved must *disagree* about how individual entities in that domain should be classified. The equality commitment maintains that these different classifications are *equally* correct in capturing the divisions in the world.

I illustrated the critiques related to two of these three commitments, namely the equality commitment and the disagreement commitment, with different examples. In Chapter 4, I appeal to the classification of nuclides in chemistry in explicating how one classification (the classification of nuclides according to the number of protons in their nuclei) is more fundamental than another classification (the classification of nuclides according to the neutron-proton ratio of their nuclei). In Chapter 3, I refer to the classification of living "organisms" in biology in demonstrating the tension between the epistemological argument and the disagreement commitment. One may complain that these two arguments are far from comprehensive, because they are merely singular examples and we cannot generalize them to classifications in other scientific domains or disciplines.

To a certain extent, I admit that this is a justified complaint (although I also believe that these two examples I used, i.e., the classification of living "organisms" and the classification of chemical nuclides, are paradigm cases of taxonomic pluralism). Yet, I do not think that this complaint reveals a shortcoming or limitation of my critiques. Recall that my critiques are based on the naturalist approach to natural kinds, which prioritizes empirical findings over metaphysical considerations in thinking about natural kinds. More specifically, according to the naturalist approach, scientific classifications in different investigative contexts are supposed to answer to the unique epistemic concerns in those contexts. Thus there is no context independent principle that can be applied universally to settle issues about classification in different investigative contexts. Whether the epistemological argument would lead to individuality pluralism and threaten the disagreement commitment, or whether the membership in a kind of one classification explains the membership in a kind of another classification, are questions that can only be answered on a case by case basis, rather than by any all-encompassing argument.

In fact, I do not aim to repudiate taxonomic pluralism with these critiques. On the contrary, in elucidating the reasons why the naturalist approach to natural kinds falls short of providing the commitments necessary for taxonomic pluralism, I attempt to set up the missing desiderata required to secure these three different commitments. More importantly, I investigate how these additional considerations would alter the way we adjudicate the monism/pluralism debate concerning scientific classifications.

One such additional consideration that will alter the way we arbitrate the monism/pluralism debate is how entities are individuated in a given scientific domain. Taxonomic pluralists argue that since scientists have different epistemic concerns in different investigative contexts, they are likely to classify the entities in a given scientific domains differently. In Chapter 3, I argued that this epistemological argument is in tension with the *disagreement commitment* of taxonomic pluralism. This is because this epistemological argument may also support individuality pluralism. If individuality pluralism is true, i.e., if there is more than one correct way to divide a scientific domain into individuals, the "different" classifications may not classify the same set of individuals, thus may not genuinely disagree with each other.

However, as I have also pointed out, even if we accept that the epistemological argument supports individuality pluralism, it is still possible for the different classifications in question to fulfill the disagreement commitment of taxonomic pluralism. This is because the different epistemic concerns that motivate these classifications may all uphold the same principle of individuation. Yet, even if the disagreement commitment is fulfilled, it still does not mean that taxonomic pluralism is confirmed. As I have indicated, depending on the epistemic concerns in question, the principle of individuation being adopted may favor a particular classification.

Another alteration can be seen in the connection between the *realist commitment* and the *equality commitment*. Although the two commitments are supposed to be independent from each other, in the sense that fulfilling one commitment does not presuppose the fulfillment of the other, the equality commitment is derived from the realist commitment. As I argued in Chapter 2, given the naturalist answer to the question of naturalness, we have to give up the traditional conception of reality, which construes what is real as what is mind-independent. I proposed that we should construe the reality of natural kinds in terms of irreducibility. However, the reduction involved is neither conservative nor eliminative. Instead, reduction here is understood in terms of *relative fundamentality*: natural kinds are not reducible to their members if and only if they are more fundamental than their members. Since natural kinds are more fundamental than their members, natural kinds are real. This notion of relative fundamentality is further spelled out in terms of metaphysical explanation: the membership in a natural kind *explains* the fact that members of that natural kind typically share a set of common properties.

I then employed this notion of relative fundamentality to examine the relation between different classifications in Chapter 4. Traditionally, taxonomic monists believe that different classifications in a given scientific domain can in principle be reduced to *the* correct classification, i.e., the classification that captures the natural-kind structure of the world. On the contrary, I side with taxonomic pluralists in maintaining that most of these different classifications are in fact irreducible, as each of them is tailored to answer scientists' unique epistemic concerns in a specific investigative context.

However, I also contend that these classifications are not necessarily on a par with each other, because one classification may be more fundamental than another classification. By appealing to the notion of *relative fundamentality*, we can say that classification A is more fundamental than classification B, if the membership in a kind of classification A *explains* the membership in a kind of classification B. The resulting picture is different from what the traditional monists envision. As I pointed out at the end of Chapter 4, the position I put forward is an intermediate position between traditional monism and full-fledged pluralism. Instead of postulating a single correct classification, my account consists of multiple classifications that are of a different degree of fundamentality. And whether we like to call it *tempered monism* or *tempered pluralism*, it is clear that the focus of the monism/pluralism debate has shifted from reducibility (understood in an eliminative sense) to relative fundamentality.

Therefore, this dissertation has outlined a framework for research into the practice of classification in different scientific domains or disciplines. By pointing out the inadequacy of the naturalist approach to natural kinds, I indicate that substantial metaphysical considerations are required to settle the monism/pluralism debate about classifications. These metaphysical considerations, as we have seen, include relative fundamentality, metaphysical explanation, and the notion of individuality. Instead of proclaiming the final verdict of taxonomic pluralism, this dissertation opens up new ways of understanding the monism/pluralism debate in scientific classification.

Bibliography

Audi, Paul (2012): "A Clarification and Defense of the Notion of Grounding" in Correia & Schnieder (eds.), pp. 101-121.

Azzouni, Jody (2017): *Ontology without Borders*. Oxford: Oxford University Press.

Bach, Theodore (2012): "Gender is a Natural Kind with a Historical Essence", *Ehtics*, vol. 122, no. 2, pp. 231-272.

-------------------- (2016): "Social Categories are Natural Kinds, not Objective Types (and Why it Matters Politically)", *Journal of Social Ontology*, vol. 2, no. 2, pp. 177-201.

Baetu, Tudor M. (2016): "From Interventions to Mechanistic Explanations", *Synthese*, vol. 193, pp. 3311-3327.

Barrett, Lisa Feldman (2006): "Are Emotions Natural Kinds", *Perspectives on Psychological Science*, vol. 1, no. 1, pp. 28-58.

-------------------- (2012): "Emotions are Real", *Emotion*, vol. 12, no. 3, pp. 413-429.

Bartol (2016): "Biochemical Kinds", *British Journal of Philosophy of Science*, vol. 67, pp. 531-551.

Beebee, Helen & Sabbarton-Leary, Nigel (2010): "Are Psychiatric Kinds Real?", *European Journal of Analytic Philosophy*, vol. 6, no. 1, pp. 11-27.

-------------------- (eds.) (2010): *The Semantics and Metaphysics of Natural Kinds*. London: Routledge.

Bernstein, Sara (2016): "Grounding is Causation", *Philosophical Perspectives*, vol. 30, no. 1, pp. 21-38.

Bickle, John (2003): *Philosophy and Neuroscience: A Ruthlessly Reductive Account*, Dordrecht, NL: Kluwer.

Bird, Alexander (2018): "The Metaphysics of Natural Kinds", *Synthese*, pp. 1-30.

Bonevac, Daniel A. (1982): *Reduction in the Abstract Science*. Indianpolis: Hackett.

Bouchard, Frédéric & Huneman, Philippe (eds.) (2013): *From Groups to Individuals: Evolution and Emerging Individuality*. Cambridge MA: MIT Press.

Boyd, Richard N. (1988): "How to be a Moral Realist" in Geoffrey Sayre-McCord (ed.) *Essays on Moral Realism*. Ithaca: Cornell University Press, pp. 181-228.

-------------------- (1990): "Realism, Conventionality, and 'Realism About'" in George Boolos (ed.) *Meaning and Method: Essays in Honor of Hilary Putnam*. Cambridge: Cambridge University Press, pp. 171-196.

-------------------- (1999a): "Homeostasis, Species, and Higher Taxa" in R. Wilson (ed.) *Species: New Interdisciplinary Essays*, 141–185. Cambridge: MIT Press.

-------------------- (1999b): "Kinds as the "Workmanship of Men" in J. Nida-Rümelin (Ed.), *Rationality, Realism, Revision: Proceedings of the 3rd international congress of the Society for Analytic Philosophy*. Berlin: de Gruyter, pp. 52-89.

-------------------- (2010a): "Homeostasis, Higher Taxa, and Monophyly", *Philosophy of Science*, vol. 77, no. 5, pp. 686-701.

-------------------- (2010b): "Realism, Natural Kinds, and Philosophical Methods" in Beebee & Sabbarton-Leary (eds.), pp. 212-234.

-------------------- (forthcoming): "Rethinking Natural Kinds, Reference and Truth: Towards More Correspondence with Reality, Not Less", *Synthese*, pp. 1-39. [https://doi-org.login.ezproxy.library.ualberta.ca/10.1007/s11229-019-02138-4]

Brigandt, Ingo. (2009). “Natural Kinds in Evolution and Systematics: Metaphysical and Epistemological considerations”, *Acta Biotheoretica*, 57, pp. 77-97.

-------------------- (2010): “The Epistemic Goal of a Concept: Accounting for the Rationality of Semantic Change and Variation.” *Synthese*, vol. 177, no. 1, pp. 19–40.

-------------------- (2011a). “Natural Kinds and Concepts: a Pragmatist and Methodologically Naturalistic Account” in J. Knowles & H. Rydenfelt (Eds.), *Pragmatism, Science and Naturalism*. Berlin: Peter Lang Publishing, pp. 171-196.

-------------------- (2011b): “Philosophy of Biology”, in S. French and J. Saatsi (eds.), *The Bloomsbury companion to the philosophy of science*, London: Bloomsbury Academic, 246–267.

-------------------- (2012): “The dynamics of scientific concepts: the relevance of epistemic aims and values” U. Feest and F. Steinle (eds), in *Scientific Concepts and Investigative Practice*. pp. 75–103. Berlin: de Gruyter.

-------------------- (2017): ““Bodily Parts in the Structure-Function Dialectic” in S. Lidgard and L. Nyhart (eds.), pp. 249–274.

Brigandt, Ingo and A. C. Love (2017): "Reductionism in Biology", *The Stanford Encyclopedia of Philosophy* (Spring 2017 Edition), Edward N. Zalta (ed.), URL = <https://plato.stanford.edu/archives/spr2017/entries/reduction-biology/>.

Brusse, Carl (2016): “Planets, Pluralism, and Conceptual Lineage”, *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, vol. 53, pp. 93-106.

Bueno, Otávio, Chen Ruey-Lin & Fagan, Melinda Bonnie (eds.) (2018): *Individuation, Process, and Scientific Practices*. Oxford: Oxford University Press.

Campbell, Joseph Keim, O’Rourke, Michael & Slater, Matthew H. (eds.) (2011): *Carving Nature at its Joints: Natural Kinds in Metaphysics and Science*. Cambridge, MA: MIT Press.

Cameron, Ross (2008): “Turtles All the Way down: Regress, Priority and Fundamentality”, *The Philosophical Quarterly*, vol. 58, no. 230, pp.1-14.

Chakravartty, Anjan (2007): *A Metaphysics for Scientific Realism: Knowing the Unobservable*. Cambridge: Cambridge University Press.

-------------------- (2011): “Scientific Realism and Ontological Relativity”, *The Monist*, vol. 94, no. 2, pp. 157-180.

Chang, Hasok (2012): *Is Water H_2O: Evidence, Realism and Pluralism*. Dordrecht: Springer.

Churchland, Patricia (1986): *Neurophilosophy*. Cambridge, MA: MIT Press.

Churchland, Paul M. (1981): “Eliminative Materialism and the Propositional Attitudes”, *The Journal of Philosophy*, vol. 78: 67–90.

-------------------- (1985): “Reduction, Qualia and the Direct Introspection of Brain States”, *The Journal of Philosophy*, vol. 82, pp. 8–28.

Clarke, Ellen (2010): “The Problem of Biological Individuality”, *Biological Theory*, vol. 5, no. 4, pp. 312-325.

------------------ (2013): “The Multiple Realizability if Biological Individuals”, *Journal of Philosophy*, vol. cx, no. 8, pp. 413-435.

Correia, Fabrice (2008): “Ontological Dependence”, *Philosophy Compass*, vol. 3, no. 5, pp. 1013-1032.

-------------------- (2013): “Metaphysical Grounds and Essence” in Hoeltje et al. (eds.), pp. 271-296.
Correia, Fabrice & B. Schnieder (eds.) (2012): *Metaphysical Grounding: Understanding the Structure of Reality*. Cambridge: Cambridge Univesity Press.
de Queiroz, Kevin (2007): “Species Concepts and Species Delimitation”, *Systematic Biology*, vol. 56, no. 1, pp. 879-886.
deRosset, Louis (2013): “Grounding Explanations”, *Philosophers' Imprint*, vol. 13, no. 7, pp. 1-26.
Devitt, M. (2005), “Scientific Realism,” in F. Jackson & M. Smith (eds.) *The Oxford Handbook of Contemporary Philosophy*. Oxford: Oxford University Press, pp. 767–791.
-------------------- (2011): “Natural Kinds and Biological Realism” in Campbell, O'Rourke & Slater (eds.), pp. 155-174.
Dorr, Cian (2005): “What We Disagree about When We Disagree about Ontology” in Mark Eli Kalderon (ed.) *Fictionalism in Metaphysics*, pp. 234-286. Oxford: Clarendon Press.
Dumsday, Travis (2010): “Natural Kinds and the Problem of Complex Essence”, *Australasian Journal of Philosophy*, vol. 8, no. 4, pp. 619-634.
Dupré, John (1993): *The Disorder of Things: Metaphysical Foundations of the Disunity of Science*. Cambridge, MA: Harvard University Press.
-------------------- (1999): “On the Impossibility of a Monistic Account of Species” in Wilson (ed.), pp. 2-22.
-------------------- (2012) *Process of Life: Essays in the Philosophy of Biology*. Oxford: Oxford University Press.
Ellis, Brian D. (2001): *Scientific Essentialism*. Cambridge: Cambridge University Press. 2001.
-------------------- (2014): *The Metaphysics of Scientific Realism*. Montreal: MQUP.
Ereshefsky, Marc (1994): “Pluralism, Normative Naturalism, and Biological Taxonomy”, *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, vol. 1994, volume two: Symposia and Invited Papers, pp. 382-389.
-------------------- (1998): “Species Pluralism and Anti-realism”, *Philosophy of Science*, vol. 65, no. 1, pp. 103-120.
-------------------- (2001): *The Poverty of the Linnaean Hierarchy: A Philosophical Study of Biological Taxonomy*. Cambridge: Cambridge University Press.
Ereshefsky, Marc & Pedroso, Makmiller (2016): “What Biofilms Can Teach Us about Individuality” in A. Guay & T. Pradeu (eds.), pp. 103-121.
Ereshefsky, Marc & Reydon, Thomas A. C. (2015): “Scientific Kinds”, *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition*, vol. 172, no. 4, pp. 969-986.
Esfeld Michael & C. Sachse (2011): *Conservative Reduction*. London: Routledge.
Fine, Kit (2001): “The Question of Realism”, *Philosophers' Imprint*, vol. 1, no. 1, pp. 1-30.
-------------------- (2009): “The Question of Ontology” in D. Chalmers & R. Wasserman, and David Manley (eds.), pp. 157-177.
-------------------- (2012): “Guide to Ground” in Correia & Schnieder (eds.), pp. 37-80.
Franklin-Hall, Laura R. (2015): “Natural Kinds as Categorical Bottlenecks”, *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition*, vol. 172, no. 4, pp. 925-948.

Friedman, Michael (1975): “Physicalism and the Indeterminacy of Translation”, *Noûs*, vol. 9, no. 4, pp. 353-374.
Ghiselin, Michael (1974): “A Radical Solution to the Specie Problem”, *Systematic Zoology*, vol. 23, no. 4, pp. 536-544.
Gilbert, Scott F. & Tauber, Alfred I. (2016): “Rethinking Individuality: The Dialectics of the Holobiont”, *Biology & Philosophy*, vol. 31, no. 6, pp. 839-853.
Gilbert, Scott F., Sapp, Jan & Tauber, Alfred I. (2012): “A Symbiotic View of Life: We Have Never Been Individuals”, *The Quarterly Review of Biology*, vol. 87, no. 4, pp. 325-341.
Godfrey-Smith, Peter (2009): *Darwinian Populations and Natural Selection.* Oxford: Oxford University Press.
-------------------- (2013): “Darwinian Individuals” in F. Bouchard & P. Huneman (eds.), pp. 17–36.
Goodman, Nelson (1978): *Ways of Worldmaking*. New York: Hackett.
--------------------- (1983): *Fact, Fiction and Forecast*. Cambridge, MA: Harvard University Press.
Goodnight, Charles J. (2013): “Defining the Individual” in F. Bouchard & P. Huneman (eds.), pp. 37-51.
Gottlieb, Dale (1976): “Ontological Reduction”, *The Journal of Philosophy*, vol. 73, no. 3, pp. 57-76.
Guay, Alexandre & Pradeu, Thomas (eds.) (2016): *Individuals Across the Sciences*. Oxford: Oxford University Press.
-------------------- (2016): “Introduction: Progressive Steps toward a Unified Conception of Individuality Across the Sciences”, in A. Guay & T. Pradeu (eds.), pp. 1-21.
Hacking, Ian (1991): “A Tradition of Natural Kinds”, *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition*, vol. 61, no. 1/2, pp. 109-126.
-------------------- (1993): “Working in a New World: The Taxonomic Solution” in P. Horwich (ed.) *World Changes: Thomas Kuhn and the Nature of Science*. Cambridge, MA: The MIT Press, pp. 275-310.
-------------------- (1999): *The Social Construction of What?* Cambridge, MA: Harvard University Press.
--------------------- (2007): “Natural Kinds: Rosy Dawn, Scholastic Twillight”, *Royal Institute of Philosophy Supplement*, vol. 61, pp. 203-239.
Hawley, Katherine & Bird, Alexander (2011): “What are Natural Kinds?”, *Philosophical Perspectives*, vol. 25, Metaphysics, pp. 205-221.
Haslanger, Sally (1995): “Ontology and Social Construction”, *Philosophical Topics*, vo. 23, no. 2, pp. 95-125.
-------------------- (2016): “Theorizing with a Purpose: the Many Kinds of Sex” in Kendig (ed.), pp. 129-144.
Haslam, Nick (2002): “Kinds of Kinds: A Conceptual Taxonomy of Psychiatric Categories”, *Philosophy, Psychiatry, & Psychology*, vol. 9, no. 3, pp. 203-217.
Hendry, Robin Findlay (2005): “Lavoisier and Mendeleev on the Elements”, *Foundations of Chemistry*, vol. 7, pp. 31-48.
--------------------- (2015): “Are Chemical Kinds Natural Kinds?” in U. Mäki et al. (eds.) *Recent Developments in the Philosophy of Science: EPSA 13 Helsinki*. G Springer International Publishing, pp. 251-261.

Hennig, Willi (1999): *Phylogenetic Systematics*. Trans. by D. Dwight Davis & Rainer Zangerl. Chicago: University of Illinois Press.
Hirsch, Eli (2011): *Quantifier Variance and Realism.* Oxford: Oxford University Press.
Hoeltje, Miguel, B. Schnieder, & A. Steinberg (eds.) (2013): *Varieties of Dependence: Ontological Dependence, Grounding, Supervenience, Response-Dependence*. Munich: Philosophia Verlag.
Hofweber, Thomas. (2005): "A Puzzle about Ontology", *Noûs*, vol. 39, no. 2, pp. 256–83.
------------------ (2007): "Innocent Statements and Their Metaphysically Loaded Counterparts", *Philosophers' Imprint*, vo. 7, no. 1, pp. 1–33.
Hull, David (1978): "A Matter of Individuality", *Philosophy of Science*, vol. 45, no. 3, pp. 335-360.
------------------- (1980): "Individuality and Selection", *Annual Review of Ecology and Systematics*, vol. 11, pp. 311–332.
------------------- (1992): "Individual" in E. Keller & E. Lloyd (eds.) *Keywords in Evolutionary Biology*. Cambridge MA: Harvard University Press, pp. 181–187.
Hylton, Peter (2007): *Quine*. London: Routledge.
Iwan, St. (2000): "An Analysis of Quine's "Ontological Reduction and the World of Numbers", *Erkenntnis*, vol. 53, no. 1/2, pp. 195-218.
Kaiser, Marie I. (2015): *Reductive Explanation in the Biological Sciences*. Dordrecht: Springer.
------------------- (2018): "Individuating Part-Whole Relations in the Biological World" in O. Bueno, R. Chen & M. Fagan (eds.).
Kellert, Stephen H., Longino, Helen E. & Waters, C. Kenneth (2006): "The Pluralist Stance"*, Minnesota Studies in the Philosophy of Science*. Vol. XIX, pp. 1-25.
Kendig, Catherine (ed.) (2016): *Natural Kinds and Classification in Scientific Practice*. London: Routledge.
Kellert, Stephen H., Longino, Helen E. & Waters, C. Kenneth (2006): "The Pluralist Stance"*, Minnesota Studies in the Philosophy of Science*. Vol. XIX, pp. 1-25.
Khalidi, Muhammad Ali (1993): "Carving Nature at the Joints". *Philosophy of Science*. Vol. 60, No. 1. pp. 100-113.
------------------- (1998): "Natural Kinds and Crosscutting Categories". *The Journal of Philosophy*. Vol. 95, No. 1. pp. 33-50.
------------------- (2013): *Natural Categories and Human Kinds: Classification in the Natural and Social Sciences*. Cambridge: Cambridge University Press.
-------------------- (2016): "Mind-Dependent Kinds"*, Journal of Social Ontology*, vol. 2, no. 2, pp. 223-246.
--------------------- (2018): "Natural Kinds as Nodes in Causal Networks", *Synthese*, vol. 195, no. 4, pp. 1379-1396.
Kim, Jaegwon (1999): "Making Sense of Emergence", *Philosophical Studies*, vol. 95, pp. 3-36.
-------------------- (2007): "Reduction and Reductive Explanation: Is One Possible without the Other?" in J. Hohwy & J. Kallestrup (eds.) *Being Reduced: New Essays on Reduction, Explanation, and Causation*, pp. 93-114. Oxford: Oxford University Press.
Kitcher, Philip (1984): "Species", *Philosophy of Science*, vol. 51, no. 2, pp. 308-333.

-------------------- (1987). “Ghostly Whispers: Mayr, Ghiselin, and the ‘Philosophers’ on the Ontological Status of Species,” *Biology and Philosophy*, vol. 2, pp. 184-192.
-------------------- (2001): *Science, Truth, and Democracy*. Oxford: Oxford University Press.
Kohman, Truman P. (1947): “Proposed New Word: Nuclide”, *American Journal of Physics*, vol. 15, no. 4, pp. 356-357.
Koslicki, Kathrin (2008): “Natural Kinds and Natural Kind Terms”, *Philosophy Compass*, vol. 3, no. 4, pp. 789-802.
-------------------- (2012): “Varieties of Ontological Dependence” in Correia & Schnieder (eds.) (2012), pp. 186-213.
-------------------- (2013): “Ontological Dependence: An Opinionated Survey” in Hoeltje et al. (eds.) (2013), pp. 31-64.
-------------------- (2015): “The Coarse-grainedness of Grounding” in K. Bennett & W. Zimmerman (eds.) *Oxford Studies in Metaphysics, vol. 9*, pp. 306-344. Oxford: Oxford University Press.
Kragh, Helge (2000): “Conceptual Changes in Chemistry: The Notion of a Chemical Element, ca. 1900-1925”, *Studies in History and Philosophy of Science Part B: Studies in History and Philosophy of Modern Physics*, vol. 31, no. 4, pp. 435-450.
Kroon, Frederick, W. (1992): “Against Ontological Reduction”, vol. 36, no. 1, pp. 53-81.
Kuhn, Thomas S. (2012): *The Structure of Scientific Revolutions* (50th Anniversary Edition). Chicago: The University of Chicago Press.
Kutschenko, Lara K. (2011): “How to Make Sense of Broadly Applied Medical Classification Systems: Introducing Epistemic Hubs”, *History and Philosophy of Life Science*, vol. 33, pp. 583-602.
Laird, Brian B. (2008): *University Chemistry*. New York: McGraw-Hill.
Le Poidevin, Robin (2005): “Missing Elements and Missing Premises: A Combinatorial Argument for the Ontological Reduction of Chemistry”, *The British Journal for the Philosophy of Science*, vol.56, pp. 117-134.
Lidgard, Scott & Nyhart, Lynn K. (2017): “The Work of Biological Individuality: Concepts and Contexts”, in Lidgard & Nyhart (eds.) (2017), pp. 17-62.
-------------------- (eds.) (2017): *Biological Individuality: Integrating Scientific, Philosophical, and Historical Perspectives*. Chicago: University of Chicago Press.
Lloyd, Elisabeth (2017): "Units and Levels of Selection" in Edward N. Zalta (ed.), *The Stanford Encyclopedia of Philosophy* (Summer 2017 Edition). URL = <https://plato.stanford.edu/archives/sum2017/entries/selection-units/>.
Loeffler, Roland (2005): “Intertheoretical Identity and Ontological Reductions”, *Erkenntnis*, vol. 62, no. 2:157-187.
Longino, Helen E. (1996): “Cognitive and Non-cognitive Values in Science: Rethinking the Dichotomy” in L. H. Nelson & J. Nelson (eds.), *Feminism, Science and the Philosophy of Science*. Dordrecht: Kluwer, pp. 39-58.
-------------------- (2002): *The Fate of Knowledge*. Princeton: Princeton University Press.
Love, Alan (2018): “Individuation, Individuality, and Experimental Practice in Developmental Biology” in O. Bueno, R. Chen & M. Fagan (eds.), pp. 165-191.

Love, Alan C. & Brigandt, Ingo (2017): "Philosophical Dimensions of Individuality" in S. Lidgard & L. Nyhart (eds.), pp. 318-348.
Lowe, E. Jonathan (2006): *The Four-Category Ontology: A Metaphysical Foundation for Natural Science*. Cambridge: Oxford: Clarendon Press.
-------------------- (2014): "How Real are Artefacts and Artefact Kinds?" in M. Franssen et. al. (eds.) *Artefact Kinds: Ontology and the Human-Made World.* Dordrecht: Springer, pp. 17-26.
-------------------- (2016): "Non-individuals" in A. Guay & T. Pradeu (eds.), pp. 49-60.
Ludwig, David (2018): "Letting Go of "Natural Kind": Toward a Multidimensional Framework of Nonarbitrary Classification", *Philosophy of Science*, vol. 85, pp. 31-52.
Magnus, P. D. (2012): *Scientific Enquiry and Natural Kinds: From Planets to Mallards*. New York: Palgrave Macmillan.

Mallet, James (1995): "A Species Definition for the Modern Synthesis", *Trends in Ecology and Evolution*, vol. 10, no. 7, pp. 294-299.
Mayr, Ernst (1969): *Principles of Systematic Zoology*. Cambridge, MA: Harvard University Press.
McIntyre, Lee (2007): "Emergence and Reduction in Chemistry: Ontological or Epistemological Concepts?", *Synthese*, vol. 155, no. 3, pp. 337-343.
Merricks, Trenton (2001): *Objects and Persons*. Oxford: Clarendon Press.
Mill, John Stuart (1858): *A System of Logic, Ratiocinative and Inductive; Being a Connected View of The Principles of Evidence and Methods of Scientific Investigation*. New York: Harper & Brothers.
Mikkelson, Gregory M. (2003): "Ecological Kinds and Ecological Laws", *Philosophy of Science*, vol. 70, no. 5, pp. 1390-1400.
Mizzoni, John (2017): *Evolution and the Foundations of Ethics.* London: Lexington.
Mumford, Stephen (2005): "Kinds, Essences, Powers", *Ratio*, vol. 18, no. 4, pp. 420-436.
Mumford, Stephen & Tugby, Matthew (eds.) (2013): *Metaphysics and Science*. Oxford: Oxford University Press.
Nanay, Bence (2011): "What if Reality has no Architecture?", *The Monist*, vol. 94, no. 2, pp. 181-196.
Okasha, Samir (2006): *Evolution and the Levels of Selection*. Oxford: Clarendon Press.
Perpper , John W. & Herron, Matthew D. (2008). "Does Biology Need an Organism Concept?", *Biological Reviews*, vol. 83, no. 4, pp. 621-627.
Plunkett, David & Sundell, Tim (2013): "Disagreement and the Semantics of Normative and Evaluative Terms", *Philosophers' Imprint*, vol. 13, no. 23, pp. 1-37.
Pöyhönen, Samuli (2016): "Memory as a Cognitive Kind: Brains, Remembering Dyads, and Exograms" in C. Kendig (ed.), pp. 145-156.
Pradeu, Thomas (2012): *The Limits of the Self: Immunology and Biological Identity*. Trans. by Elizabeth Vitanza. Oxford: Oxford University Press.
-------------------- (2016): "Organisms or Biological Individuals? Combining Physiological and Evolutionary Individuality", *Biology & Philosophy*, vol. 31, no. 6, pp. 797-817.
Psillos, Stathis (2005): *Scientific Realism: How Science Tracks Truth*. London: Routledge.

-------------------- (2011): "Choosing the Realist Framework", *Synthese*, vol. 180, no. 2, pp. 301-316.
Putnam, Hilary (1975): *Mathematics, Matter and Method*. Cambridge: Cambridge University Press.
Queller, David C. & Strassmann, Joan E. (2009): "Beyond Society: The Evolution of Organismality", *Philosophical Transactions of The Royal Society B*, vol. 364, pp. 3143-3155.
Quine, W. V. O. (1948): "On What There is", *Review of Metaphysics*, vol. 2, no. 5, pp. 21-38.
-------------------- (1960): *Word and Object*. Cambridge, MA: MIT Press.
-------------------- (1964): "Ontological Reduction and the World of Numbers", *The Journal of Philosophy*, vol. 61, no. 7, pp. 209-216.
-------------------- (1969): *Ontological Relativity and Other Essays*. New York: Columbia University Press.
-------------------- (2008): "Facts of the Matter" in D. Føllesdal (ed.) *Confessions of a Confirmed Extensionalist and Other Essays*, pp. 271-286. Harvard: Harvard Unviersity Press.
Railton, Peter (1993): "Naturalism and Prescriptivity", *Social Philosophy and Policy*, vol. 7, no. 1, pp. 151-174.
Rayo, Augustín (2007): "Ontological Commitment", *Philosophy Compass*, vol. 2, no. 3, pp. 428-444.
Reydons, Thomas A. C. (2014): "Metaphysical and Epistemological Approaches to Developing a Theory of Artifact Kinds" in M. Franssen, P. Kroes, T. Reydon & P. Vermaas (eds) *Artefact Kinds. Synthese Library (Studies in Epistemology, Logic, Methodology, and Philosophy of Science)*, vol 365. Cham: Springer, pp. 125-144.
-------------------- (2016): "From a Zooming-in Model to a Co-creation Model: Towards a more Dynamic Account of Classification and Kinds", in Kendig (ed.), pp. 59-73.
Rosen, Gideon (2009): "Metaphysical Dependence: Grounding and Reduction" in B. Hale & A. Hoffmann (eds.), *Modality: Metaphysics, Logic, and Epistemology*, pp. 109-136. Oxford: Oxford University Press.
Rosenberg, Alexander (1985): *The Structure of Biological Science*. Cambridge: Cambridge University Press.
-------------------- (1994): *Instrumental Biology or the Disunity of Science*. Chicago: University of Chicago Press.
Ruphy, Stéphanie (2016): *Scientific Pluralism Reconsidered: A New Approach to (Dis)unity of Science*. Pittsburgh: University of Pittsburgh Press.
Schaffer, Jonathan (2009): "On What Grounds What" in D. Chalmers & R.Wasserman, and David Manley (eds.), pp. 347-383.
------------------ (2016): "Grounding in the Image of Causation", *Philosophical Studies: An International Journal for Philosophy in the Analytic Tradition*, vol. 173, no. 1, pp. 49-100.
------------------ (2017): "Laws for Metaphysical Explanation", *Philosophical Issues*, vol. 27, no. 1, pp. 302-321.
Sider, Theodore (2011): *Writing the Book of the World*. Oxford: Oxford University Press.
Slater, Matthew (2005): "Monism on the One Hand, Pluralism on the Other", *Philosophy of Science*, vol. 72, no. 1, pp. 22-42.

-------------------- (2015): "Natural Kindness", *British Journal of Philosophy of Science*, vol. 66, pp. 375-411.
Sorensen, Roy (2011): "Para-Natural Kinds" in Campbell, O'Rourke & Slater (eds.), pp. 113-128.
Stanford, P. Kyle (1995): "For Pluralism and against Realism about Species", *Philosophy of Science*, vol. 62, no. 1, pp. 70-91.
Sterner, Beckett (2015): "Pathways to Pluralism about Biological Individuality", *Biology and Philosophy*, vol. 30, no. 5, pp. 609-628.
Sullivan, Jacqueline Anne (2017): "Coordinated Pluralism as a Means to Facilitate Integrative Taxonomies of Cognition", *Philosophical Explorations*, vol. 20, no. 2, pp. 129-145.
Sveinsdóttir, Ásta Kristjana (2011): "The Metaphysics of Sex and Gender", in C. Witt (ed.), *Feminist Metaphysics*. New York: Springer, pp. 47–65.
Tahko, Tuomas E. (2012): "Boundaries in Reality", *Ratio (new series)*, vol. XXV, no. 4, pp. 405-424.
-------------------- (2015): "Natural Kind Essentialism Revisited", *Mind*, vol. 124, no. 495, pp. 795-822.
Thomason, Richmond H. (1969): "Species, Determinates and Natural Kinds", *Noûs*, vol. 3, no. 1, pp. 95-101.
Thomasson, Amie L. (2015): *Ontology Made Easy*. Oxford: Oxford University Press.
Tobin, Emma (2010): "Crosscutting Natural Kinds and the Hierarchy Thesis" in Beebee & Sabbarton-Leary (eds.) pp. 179-191.
Trogdon, Kelly (2013): "An Introduction to Grounding" in Hoeltje et al. (eds.), pp. 97-122.
Tsou, Jonathan Y. (2016): "Natural Kinds, Psychiatric Classificaiton and the History of the DSM", *History of Psychiatry*, vol. 27, no. 4, pp. 406-424.
Unger, Peter (1980): "Problem of the Many", *Midwest Studies in Philosophy*, vol. 5, no. 1, pp. 411-468.
Van Fraassen, Bas C. (1977): "The Pragmatics of Explanation", *American Philosophical Quarterly*, vol. 14, no. 2, pp. 143-150.
Van Gulick, Robert (2001): "Reduction, Emergence and Other Recent Options on the Mind/Body Problem: A Philosophical Overview", *Journal of Consciousness Studies*, vol. 8, no. 9-10, pp. 1-34.
Van Inwagen, Peter (1990): *Material Beings*. Ithaca: Cornell University Press.
-------------------- (1998): "Metaontology", *Erkenntnis*, vol. 48: 233–50.
-------------------- (2009): "Being, Existence and Ontological Commitment" in D. Chalmers & R.Wasserman, and David Manley (eds.), pp. 472–506.
Van Riel, Raphael (2013): "Identity, Asymmetry, and the Relevance of Meanings for Models", *The British Journal for the Philosophy of Science*, vol. 64, pp. 747-761.
-------------------- (2014): *The Concept of Reduction*. Dordrecht: Springer.
Varzi, Achille C. (2011): "Boundaries, Conventions, and Realism", in Campbell, O'Rourke & Slater (eds.), pp. 155-174.
Waters, C. Kenneth (2017): "No General Structure" in M. Slater & Z. Yudell (eds.) *Metaphysics in Philosophy of Science*. Oxford: Oxford University Press, pp. 81-107.
------------------- (2018): "Ask not 'What is an Individual?'" in O. Bueno, R. Chen & M. Fagan (eds.), pp. 91-113.
Weber, E., Van Bouwel, J., & Vanderbeeken, R. (2005): "Forms of Causal Explanation. *Foundations of Science*, vol. 10, pp. 437–454.

Wilkerson, Terence E. (1995): *Natural Kinds*. Aldershot: Avebury Press.
Wilkins, John S. (2018): *Species: The Evolution of the Idea* (2nd edition). Boca Raton: CRC Press.
Wilson, Alastair (2018): "Metaphysical Causation", *Noûs*, vol. 52, no. 4, pp. 723-751.
Wilson, Jessica (2014): "No Work for a Theory of Grounding", *Inquiry,* vol. 65, no. 5-6, pp. 535-579.
Wilson, Robert A. (1996): "Promiscuous Realism"*, British Journal of Philosophy*, vol. 47, pp. 303-316.
------------------- (2005) *Genes and the Agents of Life: The Individual in the Fragile Sciences, Biology*. Cambridge: Cambridge University Press.
Wilson, Robert A. and Barker, Matthew (2013): "The Biological Notion of Individual" in Edward N. Zalta (ed.) *The Stanford Encyclopedia of Philosophy* (Summer 2018 Edition), URL = <https://plato.stanford.edu/archives/sum2018/entries/biology-individual/>.
Wilson, Robert, M. J. Barker & I. Brigandt (2007): "When Traditional Essentialism Fails: Biological Natural Kinds", *Philosophical Topics*, 35, pp. 189–215.
Wimsatt, William C. (1972): "Complexity and Organization", *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association*, vol. 1972, pp. 67-86.
------------------- (2006): "Reductionism and its Heuristics: Making Methodological Reductionism Honest", *Synthese*, vol. 151, no. 3, pp. 445-475.
------------------- (2007): *Re-Engineering Philosophy for Limited Beings: Piecewise Approximations to Reality*. Cambridge, MA: Harvard University Press.
Woody, Andrea I. (2012): "Concept Amalgamation and Representation in Quantum Chemistry" in R. Hendry *et al.* (eds.) *Handbook of the Philosophy of Science, vol. 6: Philosophy of Chemistry*, pp. 427-466. Amsterdam: Elsevier.
--------------------- (2015): "Chemistry's Periodic Law: Rethinking Representation and Explanation after the Turn to Practice" in L. Soler *et al.* (eds.), *Science after the Practice Turn in the Philosophy, History, and Social Studies of Science*, pp. 123-150. London: Routledge.
Zachos, Frank E. (2018): "(New) Species Concepts, Species Delimitation and the Inherent Limitations of Taxonomy", *Journal of Genetics*, pp. 1-5.