1.The problem of natural kinds

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Introduction

> The problem of natural kinds forms the busy crossroads where a number of larger problems meet: the problem of universals, the problem of induction and projectibility, the problem of natural laws and de re modalities, the problem of meaning and reference, the problem of inter-theoretic reduction, the question of the aim of science, and the problem of scientific realism in general. Nor do these exhaust the list. (Churchland 1985, 1)

Does *the* problem of natural kinds exist? Given the importance of natural kinds for various other philosophical issues, different philosophers have definitely *conceived* of the problem in many different ways, depending on what other philosophical issue led them to think about the naturalness of kinds (Churchland 1985). Given that there is such a variety of philosophical approaches to natural kinds, how can we get a clear view on the problem that is to be the subject of this dissertation?

I propose that we turn to the tradition of thinking about natural kinds for a first answer. Both Kripke and Putnam are to be credited for making natural kinds the center of much philosophical attention since the 1970's, but even before them philosophers had been thinking about the naturalness of kinds. Whewell and Mill started talking about Kinds (capital-K for Mill) as a philosophical term of art around 1840, and simultaneously also talked about Natural Groups and Classes (Whewell 1847; Mill 1843). According to Hacking, Venn was the first to comment on their work using the phrase 'Natural Kind' (Hacking 1991). Although used occasionally by others as well, the phrase was only properly introduced into modern philosophy when both Russell and Quine published their views on natural kinds (Russell 1984; Quine 1969). Eventually 'natural kinds' found their way into almost all areas of philosophy when Kripke and Putnam wrote about the semantics of natural kind terms (Kripke 1972; Putnam 1975b). Perhaps then, we will be able to get a clear view on the nature of the problem of natural kinds by looking at the problem at stake in this philosophical tradition.

Although of course, we might also just find that there is as much diachronic multiplicity as there is synchronic multiplicity. In a recent article, P.D. Magus (2014) has argued as much by claiming that the standard narrative about the history of natural kinds is mistaken. According to Magnus, the idea – largely due to the work of Hacking (1991) – that there is a *tradition* of thinking about natural kinds that starts with the work of Mill and culminates in that of Kripke and Putnam, is false. Of course, there are some connections to be found between all philosophers who spoke of 'natural kinds', but they had very different things in mind when they used that phrase. Magnus is right to stress the historical differences within this tradition, because there are many and they cut deep. To some extent I agree with his view that "there is no use trying to analyze 'natural kind'", since different philosophers were using different concepts (Magnus 2014, 9). In fact, Putnam had already taught us this same lesson:

> Even if we could define 'natural kind' — say, 'a natural kind is a class which is the extension of a term P which plays such-and-such a methodological role in some well-confirmed theory' — the definition would obviously embody a theory of the world, at least in part. [. . .]; what really distinguishes the classes we count as natural kinds is itself a matter of (high level and very abstract) scientific investigation and not just meaning analysis. (Putnam 1975a, 141)

Putnam is right to stress that the problem of natural kinds is a problem about the world (and our investigation of it), as is Magnus correct to stress that different philosophers have conceptualized natural kinds very differently. Nevertheless, I contend that there is much to be learned from the history of thinking about natural kinds. First of all, there is at least enough historical continuity to present a general outline of this issue that has been called the problem of natural kinds. Even though such an outline will necessarily be quite broad, it will prove useful to spell out some historic commonalities. Secondly, it will be helpful to present some historically important answers, as they will act as foils when developing my own preferred view on the naturalness of kinds.

Here is how I will proceed. First, I will spell out some broad commonalities that will help us to delineate the problem at stake. Secondly, I will give an overview of the historically most important answers to this problem. This history will also reveal how it came to be that natural kinds were conceived of as existing only in the most fundamental sciences. In a third section, I discuss how Boyd's theory of natural kinds builds on some of these theories, while also explaining why natural kinds can just as well be found in the special sciences.

1.1 Outline of a problem

In essence, the problem of natural kinds is the following. We use categories to classify particular things into *kinds* of things. Is there any way in which this classification is not just determined by our anthropocentric interests and preferences, but also determined by the world itself? If so, what is there in the world that makes our classifications objectively better or worse? We theorize about kinds like *gold, hydrogen* and *water*, and the samples of this stuff do appear to be of the same kind. It seems that when we classify them as belonging to the same *kind* of thing, we are making a distinction that is present in the world itself rather than only in our minds and our theories. Is this more than an illusion? Is it true that these classifications are more than the result of us, humans, imposing order on the world as we see fit, given our interest and capacities? Beyond this general characterization, many philosophers have also tended to agree on three extra qualifications of the problem at hand.

First of all, the problem of natural kinds has often been considered as a problem for the philosophy of science. This is significant in two ways. First, it is an extra delineation of the problem. What we should aim to find out is whether *scientific* classification results in natural kind categories. The stock examples that have been discussed clearly show this interest in scientific classification. Whewell and Mill started the debate on 'Natural Groups' by discussing biological taxonomy. Kripke and Putnam selected scientific categories that are also folk categories, like *water*, *gold* and *tiger*.

Characterizing the naturalness of kinds as a question for the philosophy of science is significant in a second way as well. Traditionally, philosophers who have discussed the naturalness of kinds have often maintained that whatever there is in the world that allows us to make natural classifications, is also what makes these classifications scientifically fruitful. Natural kind categories, it appears, are not only the objective ways of classifying the world into kinds of things, but identifying these kinds is also crucial for expanding our knowledge of this world. Natural kinds have often been considered as the natural grounds of inductive generalizations. A satisfactory answer to the problem of natural kinds must explain not only how their naturalness is metaphysically grounded, but also how the metaphysical nature of kinds explains why they are (and need to be) the center of much scientific exploration.[1]

Secondly, the traditional problem of natural kinds concerns only *kinds* of things, not *properties* of things. Traditionally, philosophers have used the phrase 'natural kind' to discuss whether 'gold, 'water' and 'tiger' are natural categories, but not whether 'greenness' or 'negatively charged' are natural categories. Although *being negatively charged* might be a natural property, grouping all objects that are negatively charged – a class that would include all electrons but also some balloons –, is not a way of grouping things that belong to the same *kind*. This corresponds to the intuitive difference that properties are *instantiated* by particular things, whereas particular things are *members* of kinds. The problem of natural kinds concerns only the naturalness of kind-membership.

Yet our intuitions about this difference might be deceiving us. Consider the ontological debate about the existence of properties. Are properties, say *whiteness*, really entities that are instantiated by particular (white) things? According to realists, properties are

[1] Although, as we will see, Russell and Quine thought that kind-thinking was the sign of immature sciences.

universals and hence what accounts for the similarity of particulars is that they instantiate the same universal. Nominalists object to mysterious entities like universals and hold that similarity can be explained solely in terms of particulars, like tropes or resemblance classes of particulars. We need not go into the details of this debate (Armstrong 1989). The point is, however, that most realists and nominalists see no ontological difference between properties and kinds. After all, if *whiteness* is a universal instantiated in white particulars, it seems *tiger* would also just be a universal instantiated in all tigers. And if *whiteness* were actually a resemblance class of all white things, then *tiger* would also seem to be a resemblance class consisting of all tigers.

Why is this important? Because the tradition of natural kinds might have started with thinking about the naturalness of *kinds* of things, but we can of course also ask about the naturalness of properties. Provided that we accept that every predicate refers to a property, including a gerrymandered predicate like 'standing three meters from the Eiffel Tower' or a disjunctive one like 'red or round', we can ask how we should distinguish natural properties from unnatural properties. In fact, today the term 'natural kind' is often used as a synonym of 'natural property'. After all, if there is no ontological difference between kinds and properties, perhaps accounting for the naturalness of kinds and the naturalness of properties are just one and the same problem.

I do not think they are the same problem. But given what has been said before, I also do not think we can distinguish both problems ontologically. The distinction between the problem of natural kinds and the problem of natural properties is not that they deal with different entities, since it is at least possible that there is no ontological difference. In this dissertation I remain uncommitted to any view on the ontology of properties and kinds. However, we can still distinguish the two problems as being two different questions, both related to the naturalness of our categories. The concept of natural (or sparse) properties was introduced by Lewis not only to account for the objectivity of our categories, but also to account for the fact that only some properties make objects similar and ground their causal powers (Lewis 1983). Whereas all things that are green are similar in this respect, all things that fit the predicate 'green or round' are not thereby all similar to each other.

This notion of natural properties has become common in analytic metaphysics and constitutes a question both for nominalists and realists alike.

The problem of natural kinds, however, only starts where the problem of natural properties ends. It concerns an additional naturalness question that applies only to some categories. Even after we have discovered what it is that makes things similar to each other and grounds their causal powers, we can ask how these similarities and causal powers make things belong to the same *kind*. Not just any shared (natural) property results in particular things belonging to the same kind. As we have already mentioned, *being negatively charged* would seem to be a natural property, grounding both the similarity and the causal powers of objects that have this property, yet all negatively charged objects are therefore not also members of a single *kind* of thing. One might suggest, rather, that things only belong to the same kind when they also share very *many* properties or when some relationship connects their properties. It is these extra conditions of kind-membership that theories about natural kinds have aimed to discover.

We find support for this view on the difference between the problem of natural properties and natural kinds, when we consider the most fundamental level about which we can actually ask both these questions. When Lewis gives examples of the most perfectly natural properties that exist, he mentions “the charges and masses of particles, also their so-called ‘spins’, ‘colours’ and ‘flavours’” (Lewis 1986, 60). It surely makes sense to ask what it is that makes the spin of a particle a natural property. But although the spin of a particle seems to be one of the most fundamental properties that exist, it does not make sense to ask what makes individual particles belong to a kind based only on the fact that they have the same spin. Even one of the most fundamental properties does not thereby also define a kind. What we can ask, however, is how *several* of these fundamental natural properties together constitute the kinds that we call fundamental particles. The question we should therefore ask is how individuals become members of a kind, *given* their natural properties. Or as Chakravartty has phrased it: “how to make kinds with properties” (Chakravartty 2007, 168).

A third and last characterization of the (traditional) problem of natural kinds is that it is not a *deep* metaphysical problem. That is because theories of natural kinds have almost always aimed to spell out the metaphysical criteria that allow us to determine which kinds are natural kinds and which are not. This problem would not be solved by claiming that only natural kind categories refer to real universals, or any other deep metaphysical criterion. The problem of natural kinds is therefore also independent of that about realism or nominalism about universals. The problem of natural kinds is rather to give an account of the metaphysical criteria that determine the kind-membership of individuals and which thereby ground both the naturalness and epistemic fertility of some kind categories.

Beyond the general description that we started out with, we now have three extra ways of qualifying the traditional problem of natural kinds. It appears that a successful theory of natural kinds should be one that defines the *metaphysical* criteria that determine whether particulars belong to the same *kind*, given the properties they possess, and that explain the *scientific fertility* of natural kind categories. Even given these general requirements, however, philosophers have thought about natural kinds in very different ways and have proposed very different theories as to what they are. I will present some of the most interesting theories in the following sections.

1.2 A very short introduction to the history of natural kinds

1.2.1 Whewell and Mill: Kinds and Natural Groups

When philosophers tell the story about the origin of the phrase 'natural kind', it is common to trace this history back to the work of Mill, more specifically to *A System of Logic* (1843). Mill, however, never spoke of 'natural kinds'. He did introduce the notion of a Kind (capital-K) as a piece of philosophical jargon and he also presented a theory that fits the general requirements we have just laid out. But Mill himself was responding to Whewell's previous work on scientific classification. Both Whewell and Mill aimed to explain what it is that constitutes the naturalness of scientific classifications, and interestingly, both did so by objecting to the existence of *essences*.

In his monumental *Philosophy of the Inductive Sciences* (1847), Whewell extensively discusses the "philosophy of the classificatory sciences" (Whewell 1847, Book VIII, 466-542). Although he only speaks of Natural Classes, not natural kinds, he did consider Classes, Sorts and Kinds as synonyms (Whewell 1847, 474). Furthermore, in his discussion of scientific classifications, he is worried about an issue that should by now sound familiar:

> In framing a Classification of objects we must attend to their resemblances and differences. But here the question occurs, to what resemblances and differences? for a different selection of the points of resemblance would give different results: a plant frequently agrees in leaves with one group of plants, in flowers with another. Which set of characters are we to take as our guide? The view already given of the regulative principle of all classification, namely, that it must enable us to assert true and general propositions, will obviously occur as applicable here. The object of a scientific Classification is to enable us to enunciate scientific truths: we must therefore classify according to those resemblances of objects (plants or any others,) which bring to light such truths. (Whewell 1847, 486)

Whewell and Mill agreed on this much that the regulative principle of classification is to allow for true generalizations about classes of objects. However, before spelling out what Whewell considered Natural Classes to be, it is interesting to note that he also presented an additional problem. It might be surprising for those of us who have become accustomed to the historical picture of a time before Darwin wherein all biologists were essentialists, but Whewell was well aware of the fact that biological taxonomists were frequently confronted with taxa that are not definable in terms of traits that all and only members of the taxon possess.[2] He thus realized that although generalization might be the goal of classification, many classes do not allow for *defining* generalizations. And this, he argues, might suggest that there are no Natural Classes in biology, as without defining

[2] The origin of the myth that before Darwin biologists were all essentialists has recently been explored by McOuat (2009) and by Wilkins (2009).

properties it would seem every classification depends on "the arbitrary choice of the botanist" (Whewell 1847, 493). Whewell assures us, however, that there is no need to worry about the naturalness of biological classifications in this regard. Even without definitions, natural classifications are possible. To explain how, he introduces his theory of Types:

> Though in a Natural group of objects a definition can no longer be of any use as a regulative principle, classes are not therefore left quite loose, without any certain standard or guide. The class is steadily fixed, though not precisely limited; it is given, though not circumscribed; it is determined, not by a boundary line without, but by a central point within; not by what it strictly excludes, but by what it eminently includes; by an example, not by a precept; in short, instead of a Definition we have a Type for our director.[3] (Whewell 1847, 494)

Scientific classification, Whewell argues, is not based on definitions but on *types*, which are examples of a class "eminently possessing the characters" of it (Whewell 1848, 494). In biology, every taxon that is a family has a type-genus, as does every genus have a type-species that possesses "all the characters and properties of the genus in a marked and prominent manner" (Whewell 1848, 495). Kind-membership is determined by resemblance to such central types. Whewell, then, is opposed to a view of kinds, groups, or classes, being defined in terms of *essences*. Although classifying based on exemplars will not result in a precise limit of the class, it does not only allow for natural classification, it is also a *sign* of naturalness. After all, are Artificial Classes not those that are characterized by defining properties where in fact there are none to be found in nature itself? Furthermore, in relation to this lack of defining generalizations, Whewell makes the following remark:

[3] Whewell only proposes a theory of Types for genera and higher taxa, but not for species. That is because species cannot just be classified based on similarity, as Whewell acknowledges that the same species can tend to look different in various environments. The defining principle of species should therefore include that con-specifics should be of 'the same stock'.

> Thus in the family of the Rose-tree, we are told that the ovules are very rarely erect, the stigmata are usually simple. Of what use, it might be asked, can such loose accounts be? To which the answer is, that they are not inserted in order to distinguish the species, but in order to describe the family, and the total relations of the ovules and of the stigmata of the family are better known by this general statement. (Whewell 1848, 493).

I draw attention to this passage because I will later argue that the point Whewell discusses here is a crucial problem for every theory of natural kinds that grounds this naturalness in the similarity of its members. As will be discussed shortly, Mill disagreed with Whewell's theory of types, and everybody since Mill has followed suit in proposing a theory with at least this necessary criterion that members of a kind have many properties in common. The similarity of kind-members and the regular co-occurrence of their many traits is a standard component of most theories of natural kinds. But here is the problem: kinds, and certainly those in the special sciences, apparently also sustain true generalizations that only apply 'very rarely' or only 'usually'. Do those generalizations also make for natural classifications? While the regulative ideal of classification is to formulate generalizations, it seems that not all generalizations are grounded in most or all of the members of a kind sharing a property. I will address this problem again in chapter three, and it will eventually be one of the main arguments for my Causal Unification Theory.

First, however, we should aim to define Whewell's Natural Classes. In fact, the theory of naturalness Whewell proposes does not really satisfy the requirements we have presented in the previous section. Whewell maintains that Natural Classes are just those classes that are defined *a posteriori*, which is not a metaphysical but an epistemic characterization. Whewell does not say what it is that metaphysically accounts for the naturalness of kind-membership or for their epistemic fertility. In terms of their metaphysics, Natural Classes are just those that are defined based on the rules that actually govern the natural system (Whewell 1848, 488). This *a posteriori* condition excludes classifications founded on "casual, indefinite, and unconnected considerations", like a division of plants into "*aromatic*, *esculent*, *medicinal*, and *vinous*" or one using classes like "*trees, shrubs*, and *herbs*" (Whewell 1848, 487). The *a posteriori* criterion also excludes what Whewell calls Artificial Systems (Whewell 1848, 490). Say, for example, that we discover some characters that are helpful for dividing the members of a few particular classes, like the number of stamens and pistils when distinguishing particular flowers. An Artificial System would then take these properties as "absolute and imperative" (Whewell 1848, 489) and use them to build a complete classification system, including (higher) categories of which we do not know whether they can be so characterized.

Although Whewell defines the naturalness of scientific classifications with his *a posteriori* criterion, he does not present a general theory of the metaphysical nature of natural classes. We should keep in mind, however, Whewell's very correct observation that natural classes often also support generalizations that apply only to some members. Every theory that defines the naturalness of kinds in terms of the similarity of its members, will therefore have to formulate an answer to the question whether this type of generalizations also attests to the naturalness of a kind.

In *A System of Logic* (1843), Mill argues against several points made by Whewell, most importantly his theory of types. However, Mill does concur with Whewell on at least this point that objects should be grouped such that they allow for "a great number of generalizations" to be made (Mill 1843, Book IV, 714). In a section titled "Theory of natural groups" Mill adds the following to this general view:

> The properties, therefore, according to which objects are classified, should, if possible, be those which are causes of many other properties: or at any rate, which are sure marks of them, and as being themselves the properties on which it is of most use that our attention should be strongly fixed. But the property which is the cause of the chief peculiarities of a class is unfortunately seldom fitted to serve also as the diagnostic of the class. Instead of the cause, we must generally select some of its more prominent effects, which may serve as marks of the other effects and of the cause. A classification thus formed is properly scientific or philosophical, and is commonly called a Natural, in contradistinction to a Technical or Artificial, classification or arrangement. (Mill 1843, 714)

Thus, Mill appears to argue that Natural Classifications are those that are defined in terms of *causal* properties that result in many other properties being shared by members of the group. But why causal properties? In Mill's theory, causality is a means to a very specific end, as natural classifications are just those that allow for the largest amount of true generalizations, and "the test of its scientific character is the number and importance of the properties which can be asserted in common of all objects included in the group" (Mill 1843, 715). Important in what sense? Causal properties are only important because they result in kind-members sharing many other properties as well. Mill does not really allow for any other sense of importance. Of course, *we* might consider some properties more important than others given certain practical goals, for example when we would classify whales as fish when dealing with the economic industry of whale fishery. When making a *scientific* classification, however, "we must consider as the most important attributes, those who contribute most, either by themselves or by their effect, to render things like one another" (Mill 1843, 716). Hence the naturalness of groups is grounded in the *similarity* of its members, which in turn requires us to classify based on those causal properties that result in several other similarities.

Yet not all Natural Groups are grounded in a small set of causal properties that result in many other shared properties. In fact, the best examples of natural distinctions are "distinctions not consisting in a given number of definite properties, *plus* the effects which follow from those properties, but running through the whole nature […] of the things so distinguished" (Mill 1843, 719). These are what Mill calls distinctions in *Kind*. In a section titled 'Kinds have a real existence in nature', he introduces the notion of a real Kind to distinguish classes like 'white' and 'red' from classes like 'sulphur' and 'phosphorus'. The first classes are characterized only by what is connoted by their name, and perhaps some other properties that are connected with this one property "by some law of causation" (Mill 1843, 122). Members of the same Kind, however, are characterized by an *inexhaustible* number of similarities. The similarities of Kind-members are infinite, says Mill, and are not the effects of a definite amount of causal properties.

This notion of real Kinds is meant to recuperate an insight by Aristotle, namely that there is a difference in saying of someone that he is an 'animal' or a 'man', rather than saying of a man that he is 'biped'. According to Aristotle, the difference lies herein that the first two terms predicate the genus and species, which are of the *essence of an individual*, while biped is not an essential property of man. Mill found talk of *individuals* having essences too mysterious, and thought that the essence of an individual was just the "essence of the class to which that individual was most familiarly referred"(Mill 1843, 121). But he did agree that there was a difference between the two types of classes, as he argued that 'animal' and 'man' constitute a real Kind, whereas 'biped' does not. We can now connect this theory of Kinds with his theory of Natural Groups by noting that while all Kinds are Natural Groups, not all Natural Groups need to be Kinds. After all, "[v]ery few of the genera of plants, or even of the families, can be pronounced with certainty to be Kinds", yet they can certainly be naturally classified. Only some divisions between classes, most notably those dividing species, go "through the whole nature of plants" (Mill 1843, 720), which we now understand to mean that they are similar in an inexhaustible number of ways. These Kind classifications are the most Natural Groups because one cannot replace a distinction in Kind with a more definite distinction without opting for a class that has lesser properties in common. The naturalness of all classes is grounded in their similarity, but only some groups of similar objects are also Kinds. Even for Mill, Kinds seem to have a reality beyond that of other Natural Groups.

This distinction between Kinds and other Natural Groups is mistaken in two ways. First, as will become clear in the next chapters, I do not think there is any deeper sense of naturalness than that which is contained in his theory of Natural Groups. Secondly, even if there were Kinds beyond Natural Groups, making the distinction based on the absence of a definite amount of causal relations is wrongheaded. To see why, consider the following example of Mill. Although we can classify someone as Christian, Englishman, or Mathematician, these do not constitute Kinds. After all, Mill argues that Christians only differ from other human beings in the attribute expressed by the word, and in some properties that are effects of it. Yet Mill does not exclude the possibility of Kinds of people altogether: "[t]he various races and temperaments, the two sexes, and even the various ages, may be differences of kinds, within our meaning of the term" (Mill 1843, 124). However, if we were to discover that the differences between the races actually follow as consequences "under laws of nature, from a small number of primary differences which can be precisely determined, and which, as the phrase is, *account for* all the rest", in that case they would not be Kinds (Mill 1843, 124). The fact that we are able to *explain* the differences between classes of individuals as the result of a few more fundamental properties, shows us that we were mistaken all along in thinking we had distinguished real Kinds of things. If that were the case, however, then scientific progress would have revealed many of our categories as being 'merely' Natural Groups, even those classes that would seem to be the most likely candidates for being real Kinds. Mill himself considers chemical kinds like *sulphur* as being prime examples of real Kinds, but we can now *account for* many of their common properties based on a few, more fundamental, causal properties. This notion of Kinds being the most Natural Groups because their similarities are not causally grounded thereby loses its plausibility, and is perhaps only applicable to the most fundamental physical particles. After all, the fundamental properties of elementary particles – their spin, charge, mass, etc. – are properties that co-occur lawfully although there seems to be no causal relation between them (Chakravartty 2007). But again, if we were to find that we can in fact account for their co-occurrence in a causal manner, would we then discover that elementary particles are not the Kinds we thought they were? I think the opposite would be the case instead. When we discover that some properties that we take to be characteristic of a kind are causally related, either because one property causes the others or because they are common effects of a different property, then this would surely increase our confidence that we have distinguished a real distinction in kind rather than a merely conventional one. As will be elaborated upon in further chapters, the surest sign of a conventional classification is that it is defined based on properties that are (causally) unrelated to each other.

We might ask ourselves the question, however, *why* Mill thought it necessary to stress that only some Natural Groups are Kinds, beyond the fact that he shares some intuitions with Aristotle about different types of classes. Interestingly, contrary to what has inspired many others to take an interest in natural kinds, Mill does not propose the notion of a Kind to *solve* a problem of induction, but rather to *propose* one. As we have already mentioned, Mill stresses that natural classifications, both Kinds and other Natural Groups, allow for many true generalizations and are scientifically useful. *Water*, for example, is such a fertile Kind because once we have recognized something as being water, based on some of its properties, "we are able to affirm of it innumerable other properties; which we could not do unless it were a general truth […] that the set of properties by which we identify the substance as water, always have those other properties conjoined with them"(Mill 1843, 579). But Mill also wonders how it is that we can inductively confirm these generalizations. To explain the inductive confirmation of *causal* generalizations, he introduces his famous Method of Agreement and his Method of Disagreement, based on the Law of Universal Causation. The properties of Kinds, however, are "uniformities of co-existence", some of which are co-existent without being causally related (Mill 1843, 578). Because not all common properties of Kinds are the result of a definite set of causal properties, some will be *ultimate properties* rather than derivative ones. Mill argues, for example, that the blackness of crows might very well be a fundamental property of the Kind *crow*, namely if it is not caused by any other common property.

> But in an inquiry whether some kind (as crow) universally possesses a certain property (as blackness), there is no room for any assumption analogous to this [Law of Universal Causation]. We have no previous certainty that the property must have something which constantly coexists with it; must have an invariable coexistent, in the same manner as an event must have an invariable antecedent. When we feel pain, we must be in some circumstances under which if exactly repeated we should always feel pain. But when we are conscious of blackness, it does not follow that there is something else present of which blackness is a constant accompaniment. There is, therefore, no room for elimination; no Method of Agreement or Difference, or of Concomitant Variations […]. We therefore inquire into the truth of a proposition like "All crows are black," under the same disadvantage as if, in our inquiries into causation, we were compelled to let in, as one of the possibilities, that

> the effect may in that particular instance have arisen without any cause at all. (Mill 1843, 585)

Generalizations about the fundamental properties of Kinds can only be confirmed by simple enumerative induction only. Mill concludes that such generalizations can therefore never be more than *empirical* laws, which are those laws of which we do not know *why* they exist, and hence on which we cannot rely "in cases varying much from those which have been actually observed" (Mill 1843, 516). I will not discuss this problem of confirmation any further. The point is, however, that apart from some Aristotelian intuitions (which I do not share), Mill seems to introduce the notion of a Kind – as distinct from other Natural Groups – primarily to *address* a problem of induction. Even provided that there would be such a problem, there is no reason why we should also take this epistemic problem as a defining criterion of kinds when developing our own view.

Having developed his theory of Kinds and Natural Groups, Mill objected to Whewell's theory of Types. According to Mill, membership in a Natural Group is always determined by properties that members have in common. When classifying Kinds, which are supposed to have "an impassable barrier" between them, classification proceeds by finding a few properties that allow one to determine on which side of the barrier an individual belongs. Species membership, for example, is therefore not determined by resemblance to one particular exemplar, but by those characters "that are selected as marks by which we might recognize the possibility of a common parentage" (Mill 1843, 720). When classifying other Natural Groups, like Genera, things are more difficult, as Mill agrees that types often help us to *group* objects. Nevertheless, common characters, rather than resemblance to a type, still *determine* the fact that individuals belong to a class. After all, if genera did not have properties in common, then "what general assertion would be possible respecting it? Except that they all resemble each other more than they resemble anything else, nothing whatever could be predicated of the class" (Mill 1843, 721). I agree with Mill on this point. It could very well be that exemplars are often used to identify to which kind an individual belongs, but the fact that individuals belong to the same kind is determined by the properties they share, even if this is a cluster of properties of which certain exemplars instantiate the most. After all, as I will argue, the naturalness of kinds lies in the fact that they allow for multiple generalizations.

Before we move on with this history of thinking about natural kinds, let us review some of the most interesting aspects of Whewell and Mill on natural classifications. We have already encountered many ingredients that will remain important for the debate on natural kinds: similarity, causality, and generalization. Whewell, however, did not really present a metaphysical theory of natural classes, but he did make the interesting observation that natural classes also sustain generalizations that only apply to some members. In later chapters we will revisit this issue. Mill on the other hand, did not just present one account of the metaphysical ground of natural classes, but two. Natural Groups allow for generalizations because their members are similar as a result of a definite amount of causal properties, whereas real Kinds contain members that share an inexhaustible number of properties that do not result from a shared number of causal properties. I have argued that we should not account for the most natural groups by assuming that they are not causally grounded, because this is essentially a criterion of ignorance and does not seem to affect the *naturalness* of kind classifications. My preferred view of natural kinds will eventually be very close to Mill's theory of Natural Groups, although to account for Whewell's observation I will argue that we should qualify the need for particulars to be similar. Shared by Whewell and Mill, however, is the view that natural classifications are scientifically important because they support many generalizations. Yet when Russell and Quine introduced the notion of natural kinds into modern philosophy, they both doubted the scientific importance of kind-thinking based on their use of a Millean concept of Kinds. In this respect both Russell and Quine are outliers to the general tradition of theorizing about natural kinds. I will discuss their reasons in the following section.

1.2.2 Russell, Goodman and Quine: projectible kinds

Mill's theory of Kinds was commented on by, amongst others, John Venn (using the phrase Natural Kind), Broad and Peirce (Hacking 1991). It was Russell, however, who properly introduced the notion of a 'natural kind' as philosophical jargon (Russell 1948). He did so, furthermore, by worrying about the rationality of induction, like Mill did, and with a view of kinds very similar to that of Mill.

> The existence of natural kinds underlies most pre-scientific generalizations, such as 'dogs bark' or 'wood floats'. The essence of a 'natural kind' is that it is a class of objects all of which possess a number of properties that are not known to be logically interconnected. Dogs bark and growl and wag their tails, while cats mew and purr and lick themselves. We do not know why all the members of an animal species should share so many common qualities, but we observe that they do, and base our expectations on what we observe. We should be amazed if a cat began to bark. Natural kinds are not only of biological importance. Atoms and molecules are natural kinds; so are electrons, positrons, and neutrons. [4] (Russell 1948, 335)

Like Mill, Russell thinks of natural kinds as having members that are very similar. He speaks of kinds as being 'intensional neighbourhoods' determined by many properties that are usually conjoined (although we do not know why). Cats are, for example, "not all in one intensional place, but most of them are crowded together close to an intensional centre" (Russell 1948, 390). This is to allow for the fact that some cats will be members of the same kind, yet lack a few of the properties that are common to cats in general. Manx cats, for example, are cats despite having no tail (Russell 1948, 390). But as Hacking already argued: "Russell should have noted that his account is too generous. Cats form a Russellian natural kind, but so do white cats and myriad other subset" (Hacking 1991,

[4] "A natural kind is like what in topology is called a neighbourhood, but an intensional, not an extensional, neighbourhood. Cats, for example, are like a star cluster: they are not all in one intensional place, but most of them are crowded together close to an intensional centre."(Russell 1948, 390)

112-113). I can only concur with Hacking that Russell's theory of natural kinds is too promiscuous.

Russell's theory of natural kinds is most interesting, however, for the conclusion he draws from it. Russell did not, after all, think that natural kinds are fundamental for science. It is interesting to see why he did not. Russell defines natural kinds very similar to Mill, namely as classes of objects that are similar but of which we cannot explain their similarity. The question Russell asks is whether we should accept the existence of natural kinds *like that* as a fundamental principle of scientific inference. That is, should we reason using a 'postulate of natural kinds', namely by assuming that there are *kinds* of things of which the members tend to be similar, and which will therefore support our inductive inferences, although we do not know *why* this is the case. He argues that we should not, because we can actually know the more fundamental laws that describe the co-occurrence of these properties. When we discover these laws there will be no remaining use for a notion of 'being alike in kind' and its presumed implication that we do not know why kind-members tend to have the same properties. Hence Russell, as I did when discussing Mill's proposal, objects to the criterion of ignorance that is part of the Millean view of natural kinds.

> There are still natural kinds—at the moment there are electrons, positrons, neutrons, and protons—but it is hoped that these are not ultimate, and may be reduced to differences of structure. Already in quantum theory their existence is somewhat shadowy and unsubstantial. This suggests that in physics, as in biology since Darwin, the doctrine of natural kinds may prove to have been only a temporary phase. I conclude that the doctrine of natural kinds, though useful in establishing such pre-scientific inductions as "dogs bark" and "cats mew", is only an approximate and transitional assumption on the road towards fundamental laws of a different kind. Both on this ground and because of its arbitrary character, I cannot accept it as one of the postulates of scientific inference. (Russell 1948, 460-461)

According to Russell, the notion of natural kinds is only useful to the extent that we do not know the fundamental laws that make properties tend to co-occur. Once we do discover these laws, mature sciences have no need for the notion that things can be alike in kind anymore. As will become clearer in the last section, I disagree with Russell's conclusion. It is correct that a notion of kinds that implies our ignorance of more fundamental laws is not a necessary postulate for scientific inferences. But that is not how we should think of kinds. After all, even when it is discovered why properties tend to co-occur, scientists still theorize about *kinds* of particulars. Why do they do so, even when they know many of the laws or mechanisms that cause properties to co-occur? The beginning of the answer to this question is the fact that very many properties of (many, not all) kinds tend to co-occur, making them 'hotspots' of inductive inferences. Kinds might not be needed as a principle of inductive inference, but rather as a way of grouping those things that support multiple inductive inferences. In that view a notion of *kinds* of things is not used to *justify* inductive inferences, but rather to account for the fact that some groups of things are *apt* to support them.

The notion of natural kinds being apt for inductive inferences was introduced by Quine and it is perhaps the criterion of naturalness that the most philosophers have been able to agree on. Nevertheless, Quine agreed with Russell that mature sciences do not need a notion of kinds. How can that be? To understand Quine's view we need a few more historical notes. After Russell introduced the notion of natural kinds into modern philosophy, others had associated it with attempts to define objective resemblance classes. Resemblance classes are what resemblance nominalists propose exist instead of universals to account for properties (Rodriguez-Pereyra 2002). These nominalists argue that what makes things *green* is not that all green things instantiate the universal *greenness*, but that all green things resemble each other. That is, a particular thing (a leaf) is green because it resembles other green things (bottles of wine, the statue of liberty, etc.). Properties, then, are just sets of particulars that resemble each other.[5]

Because the resemblance of particulars is an objective notion, it was also used to characterize the naturalness of our categories. As such, H.H. Price, a prominent

[5] Notice that resemblance nominalists object to the existence of universals, but not to the existence of abstract objects, since they accept 'classes' in their ontology.

resemblance nominalist, argued for a realist view of natural kinds (or classes) without accepting the existence of universals (Price 1953). Price maintained that individuals objectively belong to the same kind if they are all more like each other than any of them is like a non-member, and that this is an observable and objective fact. According to this view, we can *discover* that whales are not fish, as all whales are more similar to other mammals than they are to other fish (Price 1953). Note, however, that for a resemblance nominalist there will be no real difference between properties and kinds, as both are resemblance classes.

Resemblance nominalism has been heavily criticized. Most decisive was Goodman's objection of the imperfect community, in response to Carnap. The objection can be summarized as follows. Imagine three objects: object a is red, round and hot, object b is red, square and cold, and object c is blue, square and hot (Goodman 1966; Rodriguez-Pereyra 2002, 143). These three objects constitute a resemblance class (if they also resemble each other more than anything else), yet it is not clear what common property they would thereby have. They each resemble the other particulars of the class in some (one or two) ways, but they do not all share a single property.

Others objected mainly to the objectivity of resemblance classes like 'mammal' and 'fish'. Given the innumerable many ways in which things can be alike or different, it seems that it is always our interests in some similarities that determines the boundaries of classes. As Resnick argues against Price, it would not be *objectively* wrong for some imaginary scientists to take whales as both being fish and mammals. After all, it is *our* rule, says Resnick, that we consider having lungs as a criterion for being a mammal. "My claim is that the rules which tell us whether or not two objectively different creatures are different in kind are *conventional* and that therefore my imaginary zoologists, if they are mistaken in any sense at all, are clearly not mistaken about any objective facts" (Resnick 1960, 557).

Quine was sympathetic to resemblance nominalism, but also convinced by Goodman's objection to Carnap. In his article *Natural Kinds*, Quine explains that there is no way of defining kinds in terms of resemblance/similarity, or the other way around either, that would stand to philosophical scrutiny. "Definition of similarity in terms of kinds is halting, and definition of kind in terms of similarity is unknown"(Quine 1969, 45). Note that Quine considers similarity and kinds as just two adaptations of a single notion: particulars belong to the same kind when they are similar, and they are similar when they belong to the same kind. And while both notions are very fundamental to our thinking, "they are alien to logic and set theory"(Quine 1969, 45). Nevertheless, Quine does not think that our judgments on particulars being alike in kind are merely conventional. After all, Goodman's New Riddle of Induction showed that they cannot be, and according to Quine, Goodman thereby hinted at the non-logical roots of our concept of similarity.

Remember that Goodman's Riddle challenged the instantiation theory of inductive confirmation, according to which generalizations are confirmed by positive instances of them (Goodman 1966). Inductively confirming the generalization that 'all F's are G' requires instances of F that are G. Yet Goodman asks us to consider the predicate 'grue' that is defined as applying to all things examined before time T that are green and to all things examined after T that are blue. Given this predicate, observed instances of green emeralds a, b, etc., confirm the generalization that all emeralds are green equally well as the generalization that all emeralds are grue. The two generalizations, however, make quite different predictions about future emeralds. Goodman's New Riddle of Induction, then, is to account for the difference between projectible predicates, like 'green', and non-projectible predicates, like 'grue'. Projectible predicates are just those that can be used for inductive inferences. Thus, 'F' and 'G' are projectible if finding an 'F' that is 'G' gives us some justification to expect that the next 'F' will be 'G'.[6]

According to Quine, the notions of similarity and of kinds have their origin in humans learning which predicates are projectible and which are not. In that view, 'grue' is not projectible while 'green' is, because grue things are not similar to each other and hence

[6] This is only a very rough characterization of projectibility. For the intricacies of defining projectibility, see Earman (1985). Furthermore, 'projectibility' is normally only said of predicates. In following the literature on natural kinds, however, I will also speak of properties and kinds as being projectible themselves.

not of the same kind. Projectibility is the answer to the question why our similarity judgment cannot be just conventionally determined. Although there is no successful philosophical way of spelling out this similarity relation, the fact that 'green' is projectible, while 'grue' is not, shows us that to some extent this similarity is natural and not just conventional. It is the *success* of our inductive inferences that reveals that our similarity standards (we take green things to be more alike than grue things) are to some extent in line with those of the world itself. How did they get to be so aligned? "Darwin's natural selection is a plausible partial explanation" (Quine 1969, 49). Because color is helpful at the food-gathering level (Quine 1969, 49), it has become an important aspect of our innate similarity standards. But clearly we also note other similarities that do not have this same type of evolutionary origin:

> Credit is due to man's inveterate ingenuity, or human sapience, for having worked around the blinding dazzle of color vision and found the more significant regularities elsewhere. Evidently natural selection has dealt with the conflict by endowing man doubly: with both a color-slanted quality space and the ingenuity to rise above it. He has risen above it by developing modified systems of kinds, hence modified similarity standards for scientific purposes. By the trial-and-error process of theorizing he has regrouped things into new kinds which prove to lend themselves to many inductions better than the old. […] We revise our standards of similarity or natural kinds on the strength […] of second-order inductions. New groupings, hypothetically adopted at the suggestion of a growing theory, prove favorable to inductions of some predicate, to our satisfaction, by successfully trying to project it. In induction nothing succeeds like success. (Quine 1969, 49-50)

And yet, given all of this, Quine still thinks that mature sciences do not need the intuitive notions of 'similarity' and 'kind'. His reasoning is similar to that of Russell. As we have seen, Quine thinks of kinds as being resemblance classes, yet he also thinks that this resemblance relation cannot be properly defined. According to the resemblance view, molecules of the chemical kind *gold* all belong to the same kind because they are more similar to each other than to any other molecules. Quine argues that chemists do not need such a notion of 'overall' similarity, or a notion of kind based on it, because they are now capable of defining the comparative similarity that matters for chemists in chemical terms only. That is, molecules "*match* if they contain atoms of the same elements in the same topological combinations" (Quine 1969, 53). Hence, to know to what extent two samples of stuff are similar requires just 'counting' how many matching molecules they have. An acceptable notion of kind can then be defined based on this 'chemical similarity' (rather than overall similarity). What chemists do not need, according to Quine, is a notion of molecules, like gold, being of the same kind just because they are *similar* to other molecules. Mature sciences like chemistry can define similarity (or kind-membership) more specifically in their own terms.

> In general we can take it as a very specific mark of the maturity of a branch of science that it no longer needs an irreducible notion of similarity and kind. It is that final stage where the animal vestige is wholly absorbed into the theory. In this career of the similarity notion, starting in its innate phase, developing over the years in the light of accumulated experience, passing then from the intuitive phase into theoretical similarity, and finally disappearing altogether, we have a paradigm of the evolution of unreason into science. (Quine 1969, 55)

I agree with Quine that *this* notion of kinds as objective (overall) resemblance classes is not needed in mature sciences. I will argue, however, that there is a different notion of natural kinds that is important for (the philosophy of) science and that is not rooted in scientists' ignorance of more specific similarities (Quine) or of more fundamental laws (Russell). Yet Quine's view that the naturalness of a class is guaranteed by the fact that their predicates are projectible is a crucial one and will be retained in my view as well. The fact that kinds allow for the properties that are associated with them to be projected over their members is a powerful sign of their naturalness rather than conventionality.

Yet, we should also note that Quine introduces projectibility as a sign of the naturalness of classes, but that he does not distinguish between classes that are properties and classes that are kinds (in the sense introduced above). After all, projectibility applies to all classes that do not undermine inductive inferences, most famously to 'green'. To introduce a distinction between natural properties and natural kinds based on this projectibility criterion we might suggest that kinds are what properties are projected over. While that is correct, it is not sufficient. After all, one can also project properties over all negatively charged objects, namely that they all attract positively charged objects, and yet we argued that negatively charged objects do not constitute a *kind* of thing. To a first approximation, we could say that natural kinds support *many* inductive inferences. I will further flesh out this view below. However, in chapter two, I will also argue that natural kinds can never be fully accounted for in epistemic terms only. Although projectibility is a sign of naturalness, to distinguish between the naturalness of properties and the naturalness of kinds requires a *metaphysical* account of the nature of kinds. In the next section I will discuss the work of Kripke and Putnam, which has led to a renewed popularity of much more deeply (too deeply) metaphysical views of natural kinds.

1.2.3 Kripke and Putnam: natural kinds with essences

Both Kripke and Putnam came to the subject of natural kinds by thinking about the semantics of natural kind *terms*. They do have a somewhat different approach, however. Kripke primarily aimed to argue that there are necessary *a posteriori* truths. Theoretical identity statements, like 'water is H_2O', 'heat is molecular motion', and 'lightning is electricity', are, according to Kripke, necessary *a posteriori* truths (Kripke 1972, 116). Putnam on the other hand, was more interested in developing an externalist theory of meaning, one that could support scientific realism (Putnam 1975b). Although there are many differences between their views, and an increasing tendency of Putnam to distance himself from the all-too-metaphysical Kripkean view of essences and metaphysical necessity, their work on the semantics of natural kind terms tends to be referred to as the Kripke/Putnam theory.[7] I will do so here as well, keeping in mind that there are differences. Most important for our purposes is to present the way in which this

[7] For a discussion of the many differences between Kripke and Putnam on the subject of natural kinds, see Hacking (2007).

Kripke/Putnam theory made talk of *essences* popular again, and why we should not accept this.

Kripke and Putnam share a common target; the descriptivist theory of reference. According to this theory, proper names and general terms refer to the world because they are associated with a description that *applies* to something in the world. In this view, a proper name like 'Albert Einstein' refers to *Einstein* because this name is associated with a description like 'the man who invented the relativity theory'. Similarly, a general term like 'water' would succeed in referring to *water* because this term is associated with a description that applies to water, like the 'odorless and colorless liquid that falls from the sky and fills our oceans and rivers'. Kripke and Putnam argue, however, that this cannot be how proper names and natural kind terms refer. A proper name like 'Einstein' does not seem to refer via a description associated with it, because even when we suppose that Einstein never had invented the relativity theory, 'Einstein' would still refer to him. Furthermore, it seems that people can refer to Einstein even if they do not actually know any uniquely identifying description (Kripke 1972). The same is true for natural kind terms, say Kripke and Putnam. About the natural kind term 'gold', Kripke argues that this term would still refer to gold even if we would all be wrong in believing that gold is yellow, say because of an optical illusion (Kripke 1972, 118). Neither does 'gold' refer to everything that satisfies the description that is associated with it, since it does not refer to pyrite, or fool's gold (Kripke 1972, 119). Putnam's famous Twin-Earth thought experiment aims to convince us of the same point (Putnam 1975b). Imagine, says Putnam, a planet that is exactly like our own in every respect, except that where on Earth we find H_20, on Twin Earth we find a substance with a different chemical structure, abbreviated as XYZ, that is in every other respect like the H_2O that we find on Earth. It is a colorless and odorless liquid that fills the oceans and lakes of Twin Earth. Furthermore, this substance on Twin-Earth is also called 'water' by Twin-Earthlings. If we now consider a time before the chemical structure of water was discovered, would the term 'water' on Earth refer to both H_2O on Earth and XYZ on Twin Earth? That is to say, would the term 'water' when used on Earth refer to everything that satisfies its associated description? According to

Putnam, it should be intuitive to say that it does not. The natural kind term 'water' only refers to H_2O on Earth, not to XYZ. Thus, it cannot be the description associated with this term that determines its reference.

If proper names and general terms do not refer via a description, how do we achieve reference to the world? For proper names, Kripke and Putnam argue that the reference of a name is initially fixed by a *baptism*. When Einstein was born, someone said something like 'I call this baby Albert Einstein' and in that way the reference of the name was fixed. Subsequent reference is achieved by a causal chain of reference that links all later reference to Einstein to this original baptism. According to Kripke and Putnam, a similar story applies to natural kind terms. In the case of gold, for example, reference is also initially fixed by a baptism, which Kripke imagines would go something like "gold is the substance instantiated by the items over here, or at any rate, by almost all of them" (Kripke 1972, 135). This *ostensive* definition determines the reference of water as the *same substance* (what Putnam calls the same_L relation) as *this stuff* in the actual world. This reference is then passed on to subsequent users of the term through a causal historical link. The question that remains to be answered, however, is what it is that determines whether something is the 'same substance' as the stuff that was thus baptized, if it is not our description of that substance. According to the Kripke/Putnam theory, membership in natural kinds, like *gold* and *water*, is not determined by our description but by the *essence* of these substances themselves. Which stuff is water and which is not, is not determined by how we describe water but by the *very nature* of what it is to be water, namely being H_2O. Scientific research "attempts, by investigating basic structural traits, to find the nature, and thus the essence (in the philosophical sense) of the kind"(Kripke 1972, 138). It is the essence of a natural kind, rather than our description of it, that determines which particulars belong to the kind and which do not. Thus, the existence of natural kinds with essences allows Kripke and Putnam to make sense of the semantic intuition that the extension of a natural kind term is not determined by the description that is associated with it.

The importance of the Kripke/Putnam theory can hardly be overstated. Not only did they make an interesting case for a causal-historical theory of reference, they also re-introduced a deeply metaphysical view of natural kinds into contemporary philosophy. The idea that natural kinds have essences that metaphysically determine the kind-membership of individuals is central to this view. Other associated tenets of this metaphysical view are the claim that there is only one uniquely correct way of dividing particulars into kinds, and that kind-membership is determined by intrinsic and microstructural properties. In this view, the difference between natural and conventional kind categories lies herein that only membership in natural kinds is *metaphysically* determined by the instantiation of a kind's essence.

This metaphysical picture of natural kinds seems to have become the standard view, at least amongst those philosophers who hold that there are natural kinds (Ellis 2001, Wilkerson 1988). The intuitive appeal of this metaphysical view lies in the fact that it seems well-suited to explain the naturalness of kinds in the 'basic sciences', by which I mean physics and chemistry here. The classification of fundamental particles and of chemical elements appears to be the uniquely correct way of classifying, based on the essential properties that metaphysically determine kind-membership. However, this essentialist view of natural kinds will not apply to many kind categories in the special sciences, like biology, geology, or meteorology. Kind categories in the special sciences are often polythetic rather monothetic categories. They are not defined by necessary and sufficient properties that determine kind-membership, but rather by a cluster of properties none of which are together necessary and sufficient to define the kind. It should not be surprising that there are many polythetic kinds in the special sciences, as the high-level regularities they study depend on very complex mechanisms with many causally relevant variables (Boyd 1999b). Kinds that support such regularities will therefore often be defined in terms of many causally relevant properties, none of which are necessary and sufficient. This type of complexity is, for instance, one of the reasons why kinds of mental disorders tend to be defined as polythetic kinds, as they do not seem to have essential properties (Kendler et al. 2011).

Especially within biology it is now commonly accepted that the kinds they study do not have essences. Kripke and Putnam, however, expected biological species to have an essential 'genetic structure' much in the same way as chemical kinds are characterized by underlying structural properties. Philosophers of biology, however, have stressed the need for population thinking about species and the importance of variation within populations. Okasha (2002) defends the majority view among philosophers of biology, using both an empirical and a conceptual argument against species essentialism. First of all, it is empirically false that species can be defined in terms of a set of morphological or physiological traits that all and only members of that species have. This is also true on the genetic level. Although there are many commonalities between con-specifics, it is not the case that species have genetic traits that are possessed by all and only members of that species (Ridley 1993). Secondly, Okasha defends the conceptual point that even if there would be such (a set of) traits that are shared by all and only the members of a particular species, these traits would still not constitute the essence of the species. "For if a member of the species produced an offspring which lacked one of the characteristics, say because of a mutation, it would very likely be classed as con-specific with its parents" (Okasha 2002, 197). Hence, based on both empirical and conceptual grounds, essentialism about biological species appears to be flawed.

Some metaphysicians who defend an essentialist view of natural kinds accept this conclusion and therefore maintain that biological species cannot be natural kinds (Ellis 2001). Others are not willing to give up on essentialism about species that easily. Instead they argue that biological species do have essences, but that these will be somewhat different than expected. Okasha (2002), for instance, argues that species are defined by *extrinsic* essences, like breeding relations according to the biological species concept, rather than by intrinsic essences. He accepts, however, that these extrinsic essences will not play the causal-explanatory role that intrinsic essences do, but will only play a defining role.[8] Devitt on the other hand, maintains that there *must* be some intrinsic properties that are causally responsible for the many generalizations that hold about species, but he accepts that "*an intrinsic essence does not have to be 'neat and tidy'*", as they are in the basic sciences (2008, 371).

Rather than to discuss these proposals in detail, I will argue that essentialism about natural kinds is false. First of all, I will argue that there are no essences that *metaphysically* determine kind-membership. There is no good reason to accept that there exists such a thing as the *very nature* of a kind. Secondly, I will argue that even in a metaphysically more innocent way, kinds do not need to have defining properties to be natural kinds.

The view that I aim to argue against is that natural kinds are characterized by defining properties that constitute the very nature of a kind such that kind-membership is determined metaphysically, completely independent of our epistemic practices or interests. In this view we could be theoretically (or economically) interested in many of the macro-properties of gold and its lawful behavior, our interests in these properties in no way determine which particulars are to be classified as gold and which are not. There is only one property that is metaphysically necessary and sufficient for something to be gold, namely having atomic number 79. Having this property just constitutes what it is to be a member of the kind *gold*, whatever we might believe about it. Hence, when we gained knowledge of this property, we discovered the essence of this kind and, if one accepts the causal theory of reference, we also discovered what we had been talking about all along when using the term 'gold'.

There are two different issues at stake in this essentialist view of natural kinds. First, there is the semantic claim that the extension of our kind terms is not *just* determined by what is in our head, but also by the world itself and possibly by other language users.

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[8] "In some areas of science this idea is no doubt correct – the value of the periodic table does stem from the fact that when you know what atomic number something has, you can predict a lot about how it will behave. But in other areas of science, such as biology, this account of what makes a classification a good one is not necessarily appropriate." (Okasha 2002, 208-209)

Secondly, there is the metaphysically deep claim that kind-membership is determined by properties that constitute the very essence of the kind, independently of any human interests or theories. While I am sympathetic to the first claim, I think we have no good reasons to believe that there exist such things as essences of natural kinds.

First of all, metaphysical essentialism does not and could not follow from the semantic views of Kripke and Putnam. Many philosophers have already made this point, and I think it is a correct one. Most decisive, perhaps, was Salmon (1981), who convincingly showed that no non-trivial essentialism could be inferred from the Kripke/Putnam theory on the semantics of natural kind terms. More specifically, in the above described case, the *necessity* of the statement that 'water is H_20' crucially depends on our previously assumed premise that to be the *same substance* ($same_L$) as *that stuff* requires having the same chemical microstructure. Rather than that we can deduce the truth of microstructural essentialism from externalist semantics of natural kinds terms, they require that we first accept that the microstructure of a substance determines its kind-membership. The claim that microstructures constitute the essence of chemical kinds is a required premise if one wants to conclude that necessarily, everything that is the *same substance* as this stuff we call 'water', is H_20.

Furthermore, even when kinds *de facto* have necessary and sufficient properties, as chemical elements do, these properties do not determine kind-membership independently of our epistemic interests and practices. The empirical situation is more complex, making the metaphysical interpretation of essential properties less likely. Consider, for example, that the choice of which microstructural properties are considered definitive of chemical kinds has been partly determined by the *theoretical interests* of chemists. Chemists did not just *discover* the very nature of what it is to be a chemical element or compound. According to the IUPAC, chemical elements are to be defined as substances that have the same nuclear charge.[9] But there are also different isotopes of chemical elements, which differ in neutron number (atomic weight) but not in atomic

[9] The IUPAC is the International Union of Pure and Applied Chemistry.

number (nuclear charge). Both, however, would presumably allow for a chemical classification, that is, a classification based on a (nuclear) property that can survive chemical change (Hendry 2013, 588). Nevertheless, as Hendry argues, it was more natural to choose a classification based on nuclear charge, as this is "the overwhelming determinant of an element's chemical behavior, while nuclear mass is a negligible factor" (Hendry 2013, 588). Chemists have *decided* to maintain the periodic classification system based on nuclear charges, because *for the domain of chemistry*, these properties are causal-explanatory more important than the atomic weight of elements. The differences between isotopes were instead delegated to the domain of physics (Van Brakel 2000). While the nuclear charge of an element determines much of its chemical properties, the fact that some of these properties are also determined by differences between isotopes, suggests that this nuclear charge cannot be considered to be the very nature of what it is to be a chemical element.[10] Hence neither the Kripke/Putnam theory of natural kind terms, nor the actual classification of chemical elements, provides us with good reasons to suppose that metaphysical essences exist.

Perhaps, however, natural kinds do need to have essential properties in a more metaphysically innocent way. Is it not necessary for natural kinds to have sharp boundaries? I can think of two reasons why one might suppose that this would be the case, both of which are mistaken. First of all, one might think that there cannot be a *real difference* between members and non-members of a kind if there is no discrete boundary between them. But this is clearly false. Even a category referring to a single property, like 'being bald', can trace a real difference between people while allowing for vague boundary cases. Ask any man who is slowly losing his hair whether there is a real difference between being bald and having a head full of hair, and the answer will be

[10] The same point was made by De Sousa when he discusses the different isotopes of water: "So, if we discover that water consists of several isotopes of H_2O, what then should we say about the very nature of water? Chemists maintain that different isotopes of H_2O constitute one natural kind, physicists find several different natural kinds. I would suggest that both the chemist and the physicist are right, and that classification is partly determined by their theoretical interests." (De Sousa 1984, 571)

unanimously 'yes', even though the difference is a gradual one. The claim that natural kinds require sharp boundaries might gain in plausibility, however, if one holds that there cannot be a distinction *in kind* if there is only a gradual boundary between members and non-members. This intuition seems to be what inspired Mill to maintain that real Kinds have an impassible barrier between them. But science falsifies this intuition, as there are many categories that do appear to trace real distinctions in kind, even though their definitions are to some extent vague. Hendry (2006), for example, discusses the classification of chemical isomers, which are compounds that have many distinct chemical and physical properties although they contain the same elements in the same proportions (Hendry 2006, 869). Discussing the complex issue whether these compounds can be defined in terms of their microstructure, Hendry notes that "[m]olecular structure is defined in terms of continuous variables like internuclear distances and angles between bonds. Therefore the similarity of one molecular geometry to another is a vague, and so an intransitive relation" (Hendry 2006, 869). Even isomers, which surely seem to be different in kind from each other, are nevertheless defined in terms of interatomic geometry which will "group molecules into vague-bounded, overlapping clusters of similar structures" (Hendry 2006, 869).

A second reason to maintain that natural kinds need to have sharp boundaries is that we cannot possibly naturally *distinguish* kind-members from non-kind-members without such sharp boundaries. After all, how could we decide in a natural way to what kind particulars belong, if there is no cut-off point to be found in nature? Or as Ellis has it:

> Hence, when we are dealing with natural kinds, there cannot be any gradual merging of one kind into another, so that it becomes indeterminate to which kind a thing belongs. For if there were any such merging, we should have to draw a line somewhere if we wished to make a distinction. But if we have to draw a line anywhere, then it becomes our distinction, not nature's. Natural kinds must be ontologically distinguishable from each other. (Ellis 2001, 19-20)

Why should we suppose, however, that we *must* decide on a cut-off point? If kind-membership *is* vague and gradual, it seems perfectly fine for our kind categories to be vague as well. According to essentialists like Ellis, there cannot be natural polythetic kinds. After all, it seems that when defining such kinds *we* must decide how many of the properties an individual must possess to be a member of the kind (and perhaps how properties in the cluster should be weighed). But this is not true. The natural way of classifying a polythetic kind is by using a cluster concept that allows for vague boundaries. It is quite common for scientific kinds to allow for such indeterminate boundary cases (Hendry 2006). Of course, for practical reasons one might decide on a cut-off point and this will to some extent be conventional, but such a cut-off point is not necessary. *We* do not have to draw a discrete line anywhere. Rather, the natural way of categorizing members of kinds without essential properties is to relinquish the need for sharp boundaries.

Hence I do not think that natural kinds *require* sharp boundaries, nor do I think that there is any good reason to accept that even the most natural of scientific kinds are characterized by *metaphysically* necessary and sufficient properties. Kripke and Putnam assume that there are essential properties that can metaphysically determine the membership of kinds independently of our epistemic interests and practices. Although this microstructural essentialism helps them to account for our intuitions about natural kind *terms*, essentialism does not follow from semantic externalism and is not necessary for it either (Haslanger 2012). Furthermore, necessary and sufficient properties also appeared to be unnecessary for natural kinds. There is, however, one remaining reason for thinking that kind-members need to share essential properties. After all, if kinds do not all share the same (set of) causal properties, then what would explain the fact that they often tend to be so similar? Boyd's theory provides the solution.

1.3 Boyd's theory of natural kinds

Boyd's theory of natural kinds is an elegant combination of much of what has been discussed before (Boyd 1980, 1991, 1999a, 1999b, 2000). Like Mill, Boyd maintains that kinds are characterized by similarities and therefore allow for many generalizations. Like Quine, however, he also recognizes that only some categories are appropriate for successful inductive generalizations and that it is exactly in this success that we find their naturalness. Furthermore, Boyd is a scientific realist and agrees with Putnam that natural kinds should and can be defined in terms of (perhaps unobservable) causal properties or mechanisms that help us explain their observable properties. He is not a metaphysical essentialist, however, as he argues that the naturalness of kinds is never independent of human (epistemic) practices, and that natural kinds need not have unchanging, ahistorical, and intrinsic defining properties. Nor do natural kinds need to be referred to in fundamental, exceptionless, eternal and ahistorical laws. Boyd's view of natural kinds has become very popular, especially with philosophers of the special sciences, who find in it a way of conceiving of the naturalness of kinds that applies in the same way to both the basic and special sciences (Griffiths 1999; Wilson et al. 2007; Kornblith 1993). Let us look closer at the different tenets of his view.

Boyd shares with Quine the more general philosophical view that an epistemology of science needs to be a naturalistic epistemology. To understand how scientific knowledge is achieved, says Boyd, we need "a conception according to which not only scientific knowledge, but the language and methods of the sciences as well, represent hard-won victories in a continuing struggle to accommodate our intellectual practices to the structure of an independently existing world" (Boyd 1980, 613). According to Boyd's view then, both our scientific terminology and scientific methods, like our principles of inductive inference, have been developed by successively better accommodation to the actual causal structure of our world.[11] Like Quine, Boyd has also learned from Goodman that inductive projections require categories that are naturally rather than conventionally defined. Moreover, Boyd contends that the naturalness of categories lies exactly in the fact that they allow us to trace the causal structure of the world that grounds our inductive generalizations. This is what Boyd calls his *accommodation thesis*: "[k]inds useful for induction or explanation must always 'cut the world at its joints' in this sense: successful induction and explanation always require that we accommodate our categories to the causal structure of the world" (Boyd 1991, 139). We do not have to accept a full-blown naturalistic epistemology to agree with the view of Quine and Boyd that only some kind categories are apt for inductive projections, and that successful inductive inferences require that we define our scientific categories in deference to nature.

Unlike Quine, however, Boyd does not believe that this accommodation is achieved by mere "second-order inductions" (Quine 1969, 50). Instead, he aims to *integrate* Quine's view on the naturalness of categories with Putnam's claim that we can classify things according to their causal-explanatory essences (Boyd 1980, 640).

> Here then is the basis for an integration of the two naturalistic conceptions of kinds. Kinds characterized by 'explanatory essences' are also kinds from the point of view of inductive generalizations; indeed, in mature sciences, kinds which are explicitly characterized in terms of explanatory essences are the overwhelmingly typical cases of inductively natural kinds. Kinds natural from the point of view of successful induction need not always be explanatorily natural kinds, but they must correspond in relevant respects to the (perhaps unobservable) properties and mechanisms which causally determine the observable properties of the subjects of empirical generalizations. Moreover, an understanding of such determining properties and mechanisms is the procedure for identifying inductively natural kinds which is characteristic of scientific inquiry. (Boyd 1980, 642)

[11] According to Boyd, principles of inductive inference can therefore never possess *a priori* justification, as there are always possible worlds imaginable in which they would not be appropriate.

There are two aspects to this integration. First of all, Boyd argues that projectible generalizations are in fact *causally sustained* generalizations. Projectible categories are those that allow us to make generalizations of the form 'F are G', where "for any instantiation of it that makes the antecedent true, the state of affairs described by the antecedent will (in the relevant environment) cause the effect described by the consequent" (Boyd 1999b, 147).[12] Natural kind categories are therefore associated with properties that causally sustain generalizations over the kind. It seems that Mill's Natural Groups are back.

Secondly, in line with his broader abductive argument for scientific realism, Boyd maintains that *theoretical knowledge* of these (perhaps unobservable) causal properties is required to define scientific categories that allow for generalizations with the scope and precision typical of the mature sciences (Boyd 1980, 642). Theoretical knowledge of causal properties and mechanisms, rather than mere second-order induction, is what allows scientists to accommodate their categories to the causal structure of the world. However, we do not need to accept Boyd's broader abductive argument for scientific realism to agree that knowledge of causal properties and mechanisms will often help scientists in defining the kinds that will support inductive generalizations and explanations.

How should we picture natural kinds given this integration of Quine and Putnam? Metaphysically, Boyd considers them to be families (F) of properties that tend to co-occur, and for which this co-occurrence "may be metaphorically (sometimes literally) described as *homeostasis*. Either the presence of some of the properties in F tends (under appropriate conditions) to favor the presence of the others, or there are underlying mechanisms or processes that tend to maintain the presence of the properties in F, or both" (Boyd 1999b,

[12] "Things may be hairier than they are in our example: perhaps the truth makers for the antecedents of true instantiations are symptomatic effects of causes of the states of affairs described by the consequents. Perhaps the generalizations speak of causal powers and propensities rather than of determinate effects so that it is the causal sustenance of propensities rather than the causation of effects that is relevant. Perhaps the generalizations have a more complex logical form. And so forth." (Boyd 1999b, 147)

143). Kinds do not always have defining properties, but they are always characterized by a cluster of co-occurring properties that is causally sustained.

Yet natural kinds can never be completely accounted for in metaphysical terms only, as the naturalness of a kind is co-determined by our human epistemic practices. The *naturalness* of kinds lies in the contribution that referring to a causally sustained cluster of properties makes to the accommodation demands of a particular scientific discipline. Boyd imagines a 'bicameral legislation' according to which kinds are as much the workmanship of the causal structure of the world as of our understanding (Boyd 1999a, 89). In Boyd's view there is no place for metaphysical essences that determine kind-membership independently of our epistemological practices. This epistemic-cum-metaphysical approach to natural kinds is one of the most interesting aspects of Boyd's theory of natural kinds, and will be retained in my view as well.

Boyd's theory does not require natural kinds to have defining properties. After all, even when kinds do have defining properties their naturalness is still "a reflection of a wider sort of property correlation" (Boyd 1991, 141). Kinds with defining properties are also characterized by a cluster of properties, and we could just as well defer to nature more fully and take the kind to be defined by all these co-occurring properties (Boyd 1991, 141). And if that is the case, it is "reasonable to inquire whether there may be kinds so defined except that the relevant property correlations are not perfect, so that the set of correlated properties functions as a property-cluster" (Boyd 1991, 141). According to Boyd, many (not all) scientifically important kinds are cluster kinds without defining properties, which he calls *homeostatic property cluster kinds* (HPC). These kinds are naturally defined in terms of both their co-occurring properties and the mechanisms that explain this co-occurrence, such that any "refinement would require either that we treat as important distinctions which are irrelevant to causal explanation or to induction, or that we ignore similarities which are important in just these ways" (Boyd 1999, 144). Biological species are the best examples of such HPCs:

> It is, I take it, uncontroversial that biological species […] exhibit something like the sort of property homeostasis that defines homeostatic property cluster natural kinds. A variety of homeostatic mechanisms—gene exchange between certain populations and reproductive isolation from others, effects of common selective factors, coadapted gene complexes and other limitations on heritable variation, developmental constraints, the effects of the organism-caused features of evolutionary niches, and so on—act to establish the patterns of evolutionary stasis that we recognize as manifestations of biological species (Boyd 1999b, 164-165).

According to Boyd, biological species are to be defined both in terms of the recurring intrinsic properties of kind-members and in terms of the extrinsic mechanisms that produce this co-occurrence. More generally, Boyd sees no reason to think that natural kinds cannot be defined in terms of extrinsic properties. A biological kind like *alpha male* will at least in part be relationally defined, yet it does contribute to the accommodation demands of ethology. Kinds need not be ahistoric either, as a category like *feudal economy* can represent "a real achievement in the accommodation of explanatory practices in European history to relevant causal factors" (Boyd 1999b, 155). The fact that the definition of what constitutes a feudal economy might have to change when the social structures themselves change is just a "perfectly ordinary phenomenon in disciplinary matrices concerned with the history of complex phenomena" (Boyd 1999b, 157). Hence, it is clear that Boyd finds natural kinds not only in the basic but also in the "Inexact, Messy and Parochial Sciences" (Boyd 1999b, 151). All sciences are interested in discovering generalizations that are causally maintained.

> The problem of projectability and the associated accommodation demands are no less real in geology, biology and the social sciences than in (philosophers' idealization of) basic physics. What requires explanation, and what the theory of natural kinds helps to explain, is how we are able to identify causally sustained regularities that go beyond actually available data and how we are able to offer accurate causal explanations of particular phenomena and of such causally sustained regularities. These regularities need not be eternal, exceptionless, or spatiotemporally universal in order for our epistemic success with them to require the sort of explanation provided by the theory of natural kinds. Whatever philosophical importance (if any) there may be to the distinction between, on the one hand, causally sustained regularities and the statements that describe them, and,

> on the other, LAWS (Ta!Ta!), it is not reflected in the proper theory of natural kinds. (Boyd 1999b, 152)

In general, Boyd makes a convincing case for a theory of natural kinds that appears to account for the naturalness of kinds in both the basic sciences and the special sciences. The metaphysical nature of kinds does not require essential properties nor fundamental laws. Rather, natural kinds are characterized by clusters of properties that are sustained by causal properties and mechanisms. Yet kinds can never just be defined in metaphysical terms, as their naturalness lies exactly in the contribution reference to these metaphysical phenomena makes to the epistemic practices of a scientific discipline. To conclude, let us review some of the connections between this view and the previous views we have discussed.

Conclusion

Mill, as we have seen, distinguished real Kinds from other Natural Groups. The criterion for this distinction is the absence of a definite causal ground for the many similarities shared by Kind-members. As we have argued, however, this is a criterion of ignorance and one that would exclude many kinds that appear to be natural. In Boyd's view this distinction disappears. Although he retains Mill's focus on the similarity of kind-members, this similarity needs to be causally grounded. After all, the fact that generalizations over kinds are causally sustained is exactly what makes natural kinds epistemically so useful. However, recall that Whewell made the interesting observation that some generalizations supported by kinds do not apply to most of its members. Like Mill, however, Boyd holds a similarity view of natural kinds, as they are all characterized by a cluster of shared properties. In the third chapter we will revisit this issue, which will lead us to qualify the need for kind-members to be similar.

Like Quine, Boyd holds that the naturalness of kinds is revealed in their projectibility. Yet Quine and Russell also considered kind-thinking the sign of immature sciences. Not so for Boyd, as he presents a metaphysical view of the phenomenon that makes kinds so particularly useful for science, namely the causally sustained clustering of properties. But projectibility is the epistemic counterpart of (causally sustained) similarity, and hence in chapter three we will also qualify this criterion of projetibility.

Furthermore, Boyd integrates the view of Quine with that of Putnam, according to which natural kind terms refer to kinds with causal-explanatory essences. While Boyd stresses that natural kinds do not require essential properties, he does retain the view that kinds are causally grounded. In chapter three, I will aim to defend the need for natural kinds to be causally grounded against one interesting challenge.

Although Boyd's theory of natural kinds is one of the best to date, several philosophers have noted that it cannot account for all kinds that appear to be natural kinds (Chakravartty 2007; Ereshefsky & Reydon 2015; Khalidi 2013; Magnus 2012, 2014; Slater 2013). Some of them, however, have also argued that this mismatch can be solved without having to present a different view on the *metaphysics* of natural kinds. They contend that to define the naturalness of all scientific natural kinds, we should just define them in epistemic terms only. In the next chapter I discuss several such proposals and argue that a successful theory of natural kinds must be an epistemic-cum-metaphysical one.

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