

# Messy Chemical Kinds

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## Abstract

Following Kripke ([1972/1980]) and Putnam ([1973], [1975]), the received view of chemical kinds has been a microstructuralist one. To be a microstructuralist about chemical kinds is to think that membership in said kinds is conferred by microstructural properties. Recently, the received microstructuralist view has been elaborated and defended (e.g., Hendry [2006], [2012]); but it has also been attacked on the basis of complexities both chemical (e.g., Needham [2011]) and ontological (e.g., LaPorte [2004]). Here, I look at which complexities really challenge the microstructuralist view; at how the view itself might be made more complicated in order to accommodate such challenges; and finally, at what this increasingly complicated picture implies for our standard assessment of chemical kindhood—primarily, for the widespread assumption that chemical kinds in general are more neat and tidy than those messy biological ones.

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## 1 The Received (Microstructuralist) View

Once upon a time both biological and chemical kinds seemed like neat and tidy natural kinds. Clear-cut and simple definitions of these kinds abounded, like ‘human = rational animal’ and ‘water =  $H_2O$ ’. But it is now common knowledge in the philosophy of science that biological kinds are messy.<sup>1</sup> There are no simple and clear-cut definitions of these kinds. Philosophers can’t even agree on what kind of kinds biological kinds are.

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<sup>1</sup> I picked this term up from Slater ([2009]), but Slater ([2015]) prefers the term ‘unruly’.

Take biological species, for instance: once thought to be paradigmatic examples of natural kinds, they are generally considered natural kinds no longer. At least, they're no longer thought of as straightforwardly natural kinds. If these kinds are still 'natural', whatever that means, then their naturalness must be complicated in some way. Perhaps biological kinds are cluster kinds (e.g., Boyd [1991], [1999]) or historical kinds (e.g., Millikan [1999]); maybe they're kinds with historical essences (e.g., Griffiths [1999]) or dispositional ones (e.g., Ellis [2001]); perhaps they're sociable kinds (e.g., Chakravartty [2007]) or just promiscuous, pluralist ones (e.g., Dupré [1993], [2001]).

Whatever your preferred view of biological kindhood, the general consensus is that the situation with respect to biological kinds isn't a neat and tidy one; rather, it's a very messy one. In contrast, chemical kinds remain purportedly stalwart, neat and tidy natural kinds. Clear-cut and simple definitions are still the order of the day. Just consider the endurance, frequency, and stature of statements like 'gold is the element with atomic number 79' and 'water is H<sub>2</sub>O' in philosophical discourse.<sup>2</sup> Now one might ask, 'why is gold atomic number 79' or 'why is water H<sub>2</sub>O?' The standard answer to both these questions is 'microstructure'. More generally, 'why are chemical kinds so neat and tidy, whereas biological kinds so messy?' The common reply is 'a certain kind of microstructure, or lack thereof'.

Here I argue that chemical kindhood, like biological kindhood, is actually quite messy—*contra* the standard assessment that chemical kinds are uniformly neat and tidy microstructuralist kinds. I do not claim that chemical kinds are messy in the same way as biological kinds. Rather, I claim that clear-cut definitions of chemical kinds aren't actually that easy to find, and that giving a complete and robust account of chemical kindhood is not nearly so simple as has often been supposed. This assessment follows from two observations: one, that the classic cases of microstructuralist chemical kinds such as elements (like gold) and compounds (like water) are themselves rather messy; and two, that the foregoing philosophical picture of chemical kindhood has been based on an extremely tenuous inference from an oversimplified discussion of these sparse paradigm cases to chemical kinds much more generally.

In what follows, I'll first situate the classic cases in a standard philosophical taxonomy of chemical kinds (Section 2) before taking another look at microstructuralist accounts of elements (Section 3) and compounds (Section 4). Then I'll reassess the received microstructuralist view, developing a more complicated version capable of accommodating a revised understanding of the paradigm cases (Section 5). After that

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<sup>2</sup> Primarily thanks to Kripke [(1972/1980)] and Putnam [(1973), (1975)], examples of these kinds (ha, ha) are simply too numerous to survey.

I'll look at neglected parts of the taxonomy, showing how even a complicated microstructuralist account faces difficulty applying to other examples of complex chemical kinds, such as mixtures (Section 6) and macromolecules (Section 7). In the penultimate section (Section 8), I'll discuss what the microstructuralist must give up in order to save their account of chemical kindhood, and consider what else they must do in order to sustain it. Finally, I conclude that there is very little support for the widespread notion that biology is a messy, anti-microstructuralist junkyard whereas chemistry is a neat and tidy microstructuralist paradise (Section 9).

## 2 A Taxonomy of Chemical Kinds

The currently predominant account of chemical kinds is Robin Findlay Hendry's ([2006], [2012]).<sup>3</sup> Hendry is a microstructuralist with respect to chemical kinds. As Hendry defines it, 'microstructuralism about a natural kind is the thesis that membership of that kind is conferred by microstructural properties' ([2006], p. 865). Correspondingly, microstructuralism about a particular chemical kind (like an element or a compound) is the thesis that membership in that chemical kind is conferred by certain microstructural properties (like nuclear charge or molecular composition).

What Hendry offers is a rosy picture of chemistry as a place populated by the kinds of natural kinds that Saul A. Kripke<sup>4</sup> and Hilary Putnam<sup>5</sup> envisioned scientific experts were in the business of discovering the 'natures' of (at least when they were doing their jobs right). But Hendry's account extrapolates from just a few familiar cases, in order to generate a picture of chemistry overall. This extrapolation is also based on a particular taxonomy of chemical kinds in which the classic cases (elements and water) are situated along with other, less well-studied ones (like compounds and mixtures).<sup>6</sup>

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<sup>3</sup> Here I will focus on Hendry's view, as it is the most cited and comprehensive account of chemical kinds. However, I will also consider other microstructuralist perspectives (such as Goodwin's [2011]) along with anti-microstructuralist counterpoints (such as LaPorte's [2004] and Needham's [2011]).

<sup>4</sup> In Kripke's own words: 'When we have discovered [that heat is molecular motion], we've discovered an identification which gives us an essential property of this phenomenon. We have discovered a phenomenon which in all possible worlds will be molecular motion—which could not have failed to be molecular motion, because that's what the phenomenon *is*' ([1972/1980], p. 133; italics original).

<sup>5</sup> In Putnam's terms: 'Suppose, now, that I discover the microstructure of water—that water is H<sub>2</sub>O [...] In fact, once we have discovered the nature of water, nothing counts as a possible world in which water doesn't have that nature. Once we have discovered that water (in the actual world) is H<sub>2</sub>O, *nothing counts as a possible world in which water isn't H<sub>2</sub>O*' ([1973], p. 709; italics original).

<sup>6</sup> Hendry is more cautious about making the extrapolative case at some times rather than others. For instance, somewhat early in the [2006] paper he says that "Perhaps arguments for, or against, microstructuralism about compounds ought to be made case by case" and admits that "I have no general

Hendry uses the chemical substances as the basis for his taxonomy of chemical kinds, dividing the substances into three broad categories of elements, compounds, and mixtures (see Figure 1):

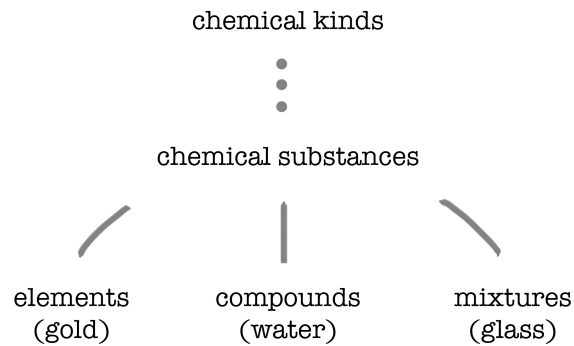


Figure 1: An illustration of the taxonomy of chemical kinds deployed by Hendry ([2006]). An example is provided (in parentheses) below each type of chemical substance.

Elements are chemical substances composed of only one kind of atom, such as helium, carbon, aluminum, or gold. Compounds are chemical substances composed of multiple types of atoms (i.e., containing different elements) that are chemically combined with one another via strong bonding interactions (i.e., covalent or ionic bonds). Examples of compounds include molecules like water, salts like ammonium, minerals like quartz, and macromolecules like proteins. Mixtures are chemical substances containing different kinds of elements and compounds that are materially or mechanically combined, but not chemically conjoined via strong bonding interactions. Examples of mixtures include materials like air and fog, cement and concrete, glass, granite, soil, or salt water.

Perhaps unsurprisingly, Hendry ([2006], [2012]) focuses on two classic cases, gold and water, which are of long-standing interest to philosophers. Gold is a really nice

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argument for microstructuralism about them" (p. 865).

But in that same paper he also ends his presentation of the microstructuralist account of water with the following: "Other compound substances present fewer problems to the microstructuralist. Some are simply more uniform as collections of molecules, while others are typically encountered as components of mixtures, rather than as (relatively) pure macroscopic samples. In either case there is less opportunity for the extensions of the substance terms to come apart from the extensions of the names of their characteristic molecular species" (p. 873). And Hendry concludes the paper by claiming that when the extension of a substance term "is not determined by microstructural similarities alone but also by various manifest properties... the term does not name a *chemical* substance" (p. 874, italics original).

Hendry's extrapolative case is made most explicitly in the introduction to the [2006] paper, when he says, "in most cases membership of [chemical] kinds is conferred by microstructural properties" (p. 865). There is more on how that case is laid out in Section 3, and a comprehensive response to it follows in Section 7.

case for the microstructuralist, actually—for at least two reasons. For one, gold is a nice case because, relative to the other cases, membership in the chemical kind gold is rather straightforwardly conferred by the possession of a pair of microstructural properties: that of having nuclear charge and number of protons both equal to 79. These properties are often jointly referred to as ‘having atomic number 79’. For another, gold is a nice case because it is perfectly representative of the rest of the chemical substances it is grouped with in Hendry’s taxonomy (i.e., with the rest of the elements). Perhaps a better way to put this point is by saying that any of the elements—gold, argon, mercury, boron, or whatever—would be, for these purposes, perfectly representative of the rest of the elements. This is because membership in the chemical kinds known as ‘elements’ is consistently conferred, throughout the group, by the general microstructural property of nuclear charge and/or number of protons.

But neither of these two things can be said about Hendry’s other main case, water. For one, it is not the case that membership in the chemical kind water is straightforwardly conferred by possession of a single microstructural property or pair of equivalent properties. For another, although water is of course a compound, it is not representative of the rest of the compounds it is grouped with. Whatever particular property (or properties) confer(s) membership in the chemical kind water—even if well characterized microstructurally—this property (or these properties) in a generalized form does not (do not) consistently confer membership throughout the group of chemical kinds known as ‘compounds’.<sup>7</sup> I’ll discuss this case more thoroughly in Section 4, after further discussion of the elements, in Section 3.

### **3 Atomic Number ( $Z = 79$ )**

Very briefly: to be an atom of the element gold is to be an atom with 79 protons and a nuclear charge of 79. Membership in the chemical kind known as ‘gold’ is conferred by having both a number of protons and a nuclear charge equal to 79, which is known as ‘having atomic number 79’. Most importantly for this discussion, a generalized version of this claim is true for all elements. Membership in element kinds is conferred by

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<sup>7</sup> Just to be clear, I am denying that membership in the (general) chemical kind known as a ‘compound’ is typically conferred by the (generalized) microstructural property of molecular composition. Although molecular composition may suffice to confer membership in some cases of (particular) compounds, there are many cases where it cannot. These cases include water, isomers, proteins, and others—so many, in fact, that this alternative class may represent the majority of currently known compounds. So I’m not simply fetishizing borderline cases here (or what Hendry calls ‘monsters’), by drawing attention to those cases where molecular composition does not confer membership in compound kinds.

atomic number, which tracks number of protons and nuclear charge. This is what makes elements such a great case for the microstructuralist: there is a pair of conjoined microstructural properties that clearly and consistently confers membership in these crucial chemical kinds, the elements.

This is important because Hendry ([2006], [2012]) uses the case of the elements to advance his microstructuralist view of chemical kinds in general. About elements, he says:

1. 'Long before any direct investigation of atomic structure, chemists used element names with determinate extension'
2. 'Membership of those extensions was conferred by having atoms with particular nuclear charges'
3. 'The chemical facts that make all this so were unknown until the twentieth century, so if they are known now they must have been discovered'<sup>8</sup>

Now, there are some complications surrounding these three claims already out there in the literature—complications having to do with things such as (i) how the case of elements historically unfolded, (ii) how determinate the extension of element names really is, and (iii) how apt it is to characterize that determination relation as 'discovered', or 'discoverable'. Joseph LaPorte ([2004]), for instance, makes something called the 'indeterminacy argument', which can be applied to some of the historical facts about the actual trajectory of the scientific investigation of the elements in a way that might seem to undermine Hendry's claims about them—especially claims 1 and 3.

But Hendry has responded to these and related worries convincingly in recent work (Hendry [2012]), by detailing the microstructuralist picture such that factors like the original intensions of chemical terms, the epistemic aims of early chemists like Lavoisier, the details of their scientific practice, and even historical contingencies of the context of discovery can all be fit within the frame. As he puts it:

In the specific case of chemistry the claim is that, since around the mid-eighteenth century, systematic thinking about the composition and behaviour of natural substances has been committed to three theoretical assumptions about elements as components of substances: that elements are actually, rather than merely potentially, present in their compounds; that they survive the specific kinds of chemical change (combustion, calcination, acid-base reactions) that were studied by the people among whom the discipline of chemistry emerged in the eighteenth century; and that the actual presence of an element is what explains (at least partially) the chemical and physical behaviour of its compounds... ([2012], pp. 59–60)

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<sup>8</sup> All direct quotes from Hendry ([2006], p. 864).

Take all these factors into account, and nuclear charge emerges as ‘the only serious candidate’ (Hendry [2012], p. 60) for the conferral of membership in the chemical kinds known as elements.

I can happily concede the microstructuralist case about elements to Hendry and still make my own case about the general messiness of chemical kinds. I said in the introduction that even the classic cases of microstructuralist chemical kinds are themselves rather messy—and this is borne out for elements in the complications that must be added to Hendry’s microstructuralist picture in light of, for instance, LaPorte’s indeterminacy argument. I also said that the inference from these classic cases to other chemical kinds is extremely tenuous. In other words, even granting that there is a strong microstructuralist case to be made for the elements, Hendry’s much more comprehensive claim about chemical kinds in general simply does not follow. Nor does his contrastive claim about the relative uniformity of classificatory interests with respect to chemical as opposed to biological kinds.

Here is how the extrapolation to other chemical kinds happens, along with the comparison between chemical and biological classification:

- A. ‘The three claims about elements [1–3 above] add up to a strong case for microstructuralism’
- B. ‘Chemistry [...] is different [from biology]: the interests that govern its classifications are more unified’
- C. ‘In most cases, membership of [chemical] kinds is conferred by microstructural properties’<sup>9</sup>

Claim A is true only if read as presenting ‘a strong case for microstructuralism’ *about elements*—and yet claims B and C are about chemical kinds *in general*. In order to judge the extrapolation from elements to all chemical kinds as well as the contrastive claim about the classificatory interests of chemistry and biology, we’ll have to survey at least a few other cases.

## 4 H<sub>2</sub>O, H<sub>3</sub>O<sup>+</sup>, OH<sup>-</sup>, and More

This brings us to Hendry’s other principal case. Actually, ‘water is H<sub>2</sub>O’ is many a philosopher’s paradigmatic example of a theoretical identity statement. As Hendry puts it: ‘being H<sub>2</sub>O is the only *chemical* requirement that is relevant to being water; it is the

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<sup>9</sup> Again, quoted in Hendry ([2006], p. 865).

only requirement of any kind that is necessary to being water' ([2012], p. 63; italics original). But I disagree with this characterization: I think that there are other chemical requirements relevant to and necessary for being water. And I think that Hendry himself has already admitted as much.

I'll separate these other chemical requirements into two sets, one of which consists of a single, general requirement that Hendry ([2006]) introduces as a blanket condition—one that he says applies to all chemical substances. I'll discuss that general requirement beginning in Section 6. Prior to that, here in Section 4, I'll introduce the other set of chemical requirements—as they are specific to water—and in Section 5 I'll discuss various attempts to accommodate these requirements. Though these requirements complicate giving a microstructuralist account of water, I don't think they are necessarily prohibitive. Again, I think that Hendry himself is well aware of these chemical complexities, and the goal here is to make explicit what the consequences of these complexities are for the supposedly simple microstructuralist account.

H<sub>2</sub>O is indisputably the water molecule. Here's a standard way of representing that molecule (shown in Figure 2a):

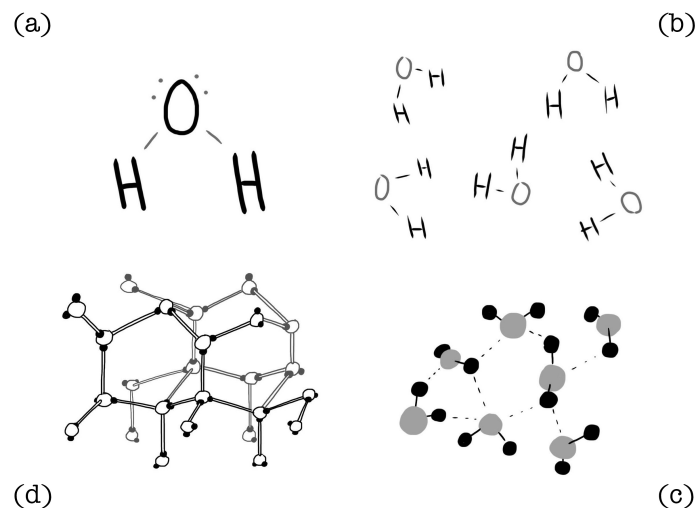


Figure 2: Illustrations of water in various forms. Clockwise from top left: (a) as molecule; (b) as vapor; (c) as liquid; (d) as ice. Drawings by Adam Streed.

But water, even in the chemical sense of the term, isn't a substance that is comprised only of H<sub>2</sub>O molecules. Much of the water we encounter, as well as the water chemists use and study, comes in groups of molecules commonly known as 'bodies of water'.



And bodies of water—even chemically ‘pure’ bodies of water—aren’t composed only of H<sub>2</sub>O molecules.

Figure 2 shows not just a standard chemical depiction of the water molecule; it also shows typical chemical depictions of bodies of water in three other familiar states. Figure 2b is a rather simplistic representation of one of those kinds of bodies of water: water vapor. This is water in its gaseous form, and it looks pretty much like individual H<sub>2</sub>O molecules hanging out near one another.

But Figure 2c is a representation of another kind of body of water: liquid water. This state is far more chemically complex than either of the previous two. Water in this form actually consists of an assortment of H<sub>2</sub>O, H<sub>3</sub>O<sup>+</sup> (hydronium), and OH<sup>-</sup> (hydroxide) molecules—some water molecules dissociate when in this kind of contact with one another. A characteristic percentage of hydronium (cation) and hydroxide (anion) ions form in the body of liquid water, and this disassociation is what gives this kind of water some of its most distinctive macroscopic chemical properties. For example: liquid water is a remarkable conductor when considered merely as a collection of individual H<sub>2</sub>O molecules, as it is the dissociation of these molecules that makes it such a good conductor. The disassociation allows for the formation of nascent structures, which can then transport protons in an especially efficient way, due to the Grotthuss mechanism.<sup>10</sup>

Water molecules also associate with one another, when in liquid form, and these associative rather than disassociative interactions generate other distinctive macroscopic chemical properties of liquid water. For example, liquid water has an unexpectedly high heat of vaporization, when considered merely as a collection of individual H<sub>2</sub>O molecules. It takes more energy than would be expected, on the basis of individual water molecule composition alone, to transform liquid water into steam. This is due to the formation of hydrogen bonds between individual H<sub>2</sub>O molecules in liquid water. Incidentally, these kinds of bonds occur not just between isolated pairs of H<sub>2</sub>O molecules, but also among whole groups of them, which form fluctuating aggregate structures throughout the liquid—structures that disperse and reform continuously in the presence of thermal and other forms of agitation. In order for a body of liquid water to transition to a gaseous state, the hydrogen bonds within these aggregates have to be broken, and this takes extra energy, which produces liquid water’s unexpectedly high heat of vaporization.<sup>11</sup>

The point of discussing both these cases—vaporization and conductivity—is just to show that liquid water is not well characterized simply as H<sub>2</sub>O. If liquid water was

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<sup>10</sup> Discovered by Theodor von Grotthuss in 1806.

<sup>11</sup> These complexities similarly apply to liquid water’s transition to ice.

merely a simple collection of non-interacting H<sub>2</sub>O molecules, then it would not be the chemical substance that scientists have been struggling to understand and accurately characterize for centuries now. Bodies of liquid water containing only non-interacting H<sub>2</sub>O molecules would not have the chemically characteristic macroscopic properties of liquid water. So, if we sufficiently take into account the long-standing chemical tradition and its epistemic investment in understanding bodies of water as well as (if ever) isolated water molecules, then liquid water is not necessarily H<sub>2</sub>O.<sup>12</sup> Rather, it's a population of interacting H<sub>2</sub>O, H<sub>3</sub>O<sup>+</sup>, and OH<sup>-</sup> molecules in characteristic proportions—at least according to the best scientific account that we have to date.

Not to belabor the point, but the third state in which bodies of water are commonly found—as frozen water, or ice—is not just a free-floating assortment of H<sub>2</sub>O molecules either. As Figure 2d illustrates, it has something which could very aptly be called 'microstructure', even in the literal sense of 'structure'. Finally, there are several other potentially problematic permutations of the water case—as there was with the elements. There's the possibility of a nuclear reaction that spontaneously turns an assortment of H<sub>2</sub>O molecules into isotopes of neon and fluorine, and there's the question of what to do about the status of heavy water. Complications abound.

## 5 Complicating the Microstructuralist Picture

The traditional, simple account is a familiar one:

Water = (H<sub>2</sub>O)

But clearly there is more to the case than the simple account implies. Hendry ([2006], [2012]) responds to the various chemical complexities of the case by, among other things, generating a disjunctivist account of water—although he doesn't explicitly mark it as such. Hendry's account is:

Water = ("individual H<sub>2</sub>O molecules") v  
("the substance formed by bringing together H<sub>2</sub>O molecules  
and allowing them to interact spontaneously")<sup>13</sup>

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<sup>12</sup> Hendry, at least, is committed to taking this long-standing chemical tradition and epistemic investment seriously, as these are just the sorts of considerations he invoked in his ([2012]) response to LaPorte's ([2004]) indeterminacy argument about element names (discussed in Section 3).

<sup>13</sup> Both quotes are from Hendry ([2006], p. 872). Hendry says there is one constraint on this account—and

Hendry is not the first to offer a two-part disjunctivist account of water. Paul Needham once offered ([2000]) a similarly microstructuralist account on which water was defined according to either a molecular condition or a compositional claim about bulk matter, although Needham has since articulated ([2011]) his preferred macroscopic account instead.<sup>14</sup>

Hendry's two-part disjunction accommodates the various gaseous, liquid, and solid states of water via the second arm—an arm that appeals to a pair of distinctions between simple and basic substances, components and ingredients. Simple substances include the free elements (like H<sub>2</sub> and N<sub>2</sub>) whereas basic substances are those which “occupy the places in the periodic table” (Hendry [2006], p. 867); components persist in the composites they form whereas ingredients are used up in the formation of those composites. On Hendry's account, liquid water contains certain basic substances (the constitutive elements hydrogen and oxygen) that persist as components, but water is also made out of certain ingredients (like the simple substances H<sub>2</sub> and O<sub>2</sub>, as well as H<sub>2</sub>O molecules) that may not necessarily persist through its component interactions (such as the dissociation of some H<sub>2</sub>O molecules into H<sub>3</sub>O<sup>+</sup> and OH<sup>-</sup> that occurs in liquid water).

One complication with this arm of Hendry's disjunction is that it creates the possibility, given a potential spontaneous nuclear reaction, that certain isotopes of neon

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that's the blanket condition I'll discuss in Section 6.

<sup>14</sup> Thanks to an anonymous reviewer for drawing my attention to Needham's earlier view. Of course, the existence of Needham's more recent and currently preferred account, a macroscopic one, does not preclude Hendry or anyone else from maintaining an opposing microstructuralist account. But Needham does argue against the viability of such alternatives. According to Needham, claiming something is necessarily true of a chemical substance (as the microstructuralist inescapably claims) entails claiming that substance can't be successfully characterized in an everyday sense, without reference to what's necessarily true of it. Yet, as Needham points out, such substances obviously and often are successfully characterized in an everyday sense without reference to what's necessarily true of it.

However: when Putnam says that 'once we have discovered the nature of water, nothing counts as a possible world in which water doesn't have that nature' ([1973], p. 709), I don't think he's saying that everyday speakers have to use the chemical understanding of 'the nature of water' to successfully refer to water or even to appropriately characterize it, in an everyday context. For a Putnam-style account, calling a substance like water a 'natural kind' is simply a way of saying that all other ways of referring to or characterizing members of that kind—ways that do *not* point to what is necessarily true of the kind—are necessarily proxies. As in, the successful use of non-essential properties as a way of characterizing or referring to members of a natural kind inevitably piggybacks on the good fortune of those non-essential properties successfully tracking the essential properties. Fodor ([1990]) calls this 'asymmetric dependence'. In sum, Needham's argument against microstructuralism in general fails. His ([2011]) presentation of the chemical complexities of water as well as his proposed macroscopic account leave open the possibility of a successful, albeit complicated, microstructuralist alternative.

and fluorine might count as water on Hendry's account. Hendry deals with this complication by positing a further constraint. This is the blanket condition that he says applies to all chemical substances, which I will discuss in detail in Section 6.

Before moving on to that discussion, however, I want to briefly note that there is another potential way for the microstructuralist to respond to the chemical complexities of water. The microstructuralist could simply build these complexities directly into an even more complicated disjunctivist account than Hendry's, rather than by appealing to his pair of distinctions (between simple and basic substances, components and ingredients).

Recall from section 4 that there is some expansiveness within the chemical sense of the term 'water'. There is 'water' in the sense of 'water molecule', yet there is also 'water' in the sense of 'body of water'. And that latter sense contains three more senses: first, there is 'body of water' in the sense of 'water vapor'; second, there is 'body of water' in the sense of 'liquid water'; and third, there is 'body of water' in the sense of 'frozen water'. Also note the various facts about the chemical complexities of water just discussed: being water in a liquid state requires not just  $\text{H}_2\text{O}$  but also  $\text{H}_3\text{O}^+$  and  $\text{OH}^-$  molecules, all in certain characteristic proportions; being water in a solid state requires  $\text{H}_2\text{O}$  molecules arranged in any one of several candidate lattices structure. Of course, the chemical details are even more complex than this. For instance, there isn't just one formation that the lattice structure of water molecules can take in ice. Rather, there are more than 10 of these structures currently known to chemistry.

But as long as the theoretical identity statement ('water is [fill in the blank]') doesn't contain any functional role attributions, the microstructuralist should in principle be able to make the right-hand side as complicated as they might want.

So, what is necessarily true of bodies of water? That they are groups of  $\text{H}_2\text{O}$  molecules in appreciable densities, or characteristic populations of  $\text{H}_2\text{O}$ ,  $\text{H}_3\text{O}^+$ , and  $\text{OH}^-$  molecules, or hydrogen-bonded  $\text{H}_2\text{O}$  molecules in various formations. The determined microstructuralist could even offer one comprehensive, disjunctive account of the basic chemical term 'water':

Water =  $(\text{H}_2\text{O}) \vee$   
           $(\text{H}_2\text{O} \text{ molecules in an appreciable density}) \vee$   
           $(\text{a characteristic population of } \text{H}_2\text{O}, \text{H}_3\text{O}^+, \text{ and } \text{OH}^- \text{ molecules}) \vee$   
           $(\text{hydrogen-bonded } \text{H}_2\text{O} \text{ molecules in one of 10 or more}$   
           $\text{particular formations})$

This is obviously a complicated account, but it is also a microstructuralist one. It successfully incorporates the current scientific understanding of the microstructural complexities of the chemical substance water, and it could act as a template for other candidate microstructural accounts. It is revisable in light of additional details and future information about the microstructural complexities of water, and it is applicable in form to alternative chemical substances of similar degrees of complexity.

This is not to say that such an account is necessarily free of philosophical difficulties. It is just to say that there is another provisional response to the chemical complexities detailed above available to the determined microstructuralist. And notice that, in offering even such a provisional response, the microstructuralist picture of water has become significantly more complicated—just as Hendry’s microstructuralist picture of elements had to be further complicated in order to accommodate the relevant chemical and historical complexities surrounding that case.

So, it’s the usual mix of good news and bad news for the microstructuralist. It looks like a microstructuralist account of water can be fashioned. But it’ll have to be a very complicated account. My goal here was simply to make explicit what the consequences of the chemical complexities of water are for the supposedly simple microstructuralist picture. Regardless of which microstructuralist option is preferred—Needham’s original view, Hendry’s received one, or my latest offering—all of these accounts are disjunctive, and this result significantly complicates the microstructuralist picture. Consider, for instance, any purported laws containing the term ‘water’: will they also split disjunctively, tracking the different terms on the right-hand side of the theoretical identity statement? They might need to. Will generalizations across the different arms of the disjunction hold? They might not.

## **6 Concrete and Other Mixtures**

Since it is the received view, let us now return to Hendry’s particular brand of complicated microstructuralism. Recall that Hendry had to introduce an additional constraint along with his two-part disjunctivist account, in order to deal with a certain problematic, potential spontaneous nuclear reaction. This constraint is called the ‘sameness of elemental composition’ condition (Hendry [2006], p. 872) and it means that in order for chemical substances to be members of the same kind of chemical substance, they have to have the same elemental composition as one another.<sup>15</sup>

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<sup>15</sup> Note that this is not the same as requiring sameness of molecular composition.

Hendry presumes this (necessary but not sufficient) condition applies to all chemical substances (Hendry [2006], pp. 872–3). In other words: Hendry thinks, along with many others, that this is a chemical requirement that acts as a blanket condition for sameness of chemical substance.

My interest in discussing this requirement is not to argue with Hendry about the application of this condition to the case of water. Rather, the problem with the sameness of elemental composition condition lies in its purported application to all chemical substances, as a blanket condition for sameness of chemical substance. It is now time for a shift in focus, from the first of my two observations about chemical kinds to the second. These are the observations on which my primary argument about the messiness of chemical kindhood is based. The first was that plausible microstructuralist accounts of the classic chemical kinds like elements and water turn out, on careful examination, to be rather messy themselves. I hope to have established that point already. The second point was that we don't actually have very much support for the notion that chemical kinds are generally microstructuralist kinds, merely given the fact that it is possible to give (albeit messy and complicated) microstructuralist accounts of so few (although classic) cases. I turn to that point now.

One effective way of undermining this inference is by demonstrating what a massive problem the seemingly innocuous sameness of elemental composition condition presents for the microstructuralist. I'll make this problem clear in a series of steps. First, I need to say something about mixtures, the as-yet-undiscussed third branch of Hendry's taxonomy of chemical kinds (shown back in Figure 1).

Mixtures are chemical substances whose components are combined, not via strong bonding interactions, but rather by weaker material or mechanical interactions. Paying attention to the way that the components in a mixture can be combined introduces some helpful distinctions within this extremely disparate category of chemical substances. For instance, suspension mixtures—which require only stirring to combine—can be distinguished from colloidal mixtures—which necessarily involve a solute and a solvent. Examples of suspension mixtures include single-state mixtures (like dirt and sand) as well as multiple-phase mixtures (like carbonated water). Fog (which is not just water vapor, as it turns out) is a nice example of a colloidal mixture. Another way to distinguish types of mixtures is to discriminate between heterogeneous mixtures (like ice water) and homogenous mixtures (like brass).

Standard chemistry textbooks readily admit things like “Unlike elements and compounds, **mixtures** can have variable compositions” (Brady & Senese [2004], p. 11; emphasis original). As an illustration of what this means for the microstructuralist,

consider concrete—a certain kind of chemical mixture primarily composed of water, aggregate, and cement. Note that the term ‘aggregate’ covers many different kinds of particulate material: many types of stone, as well as sand and gravel, among other things, can be used as the aggregate in concrete. Obviously these materials can have different elemental compositions; hence, many instances of the chemical kind commonly known as ‘concrete’ will themselves have different elemental composition. If sameness of elemental composition is a condition for sameness of chemical substance, then the term ‘concrete’ turns out not to refer to one chemical substance after all—it refers to many different chemical substances instead. That would be a distinct chemical substance for each sample of so-called ‘concrete’ with a unique elemental composition—potentially, one for every extant sample.

Perhaps Hendry would be happy to strike ‘concrete’ off the list of chemical substances. But concrete is not an anomalous case amongst mixtures. For one, it appears in not just modern textbooks (like the Brady & Senese quoted earlier) but also in sources as classic as Linus Pauling’s *General Chemistry*. For another, many if not most of the chemical substances known as mixtures—think for a second about air, clay, fog, glass, gravel, granite, mud, sand, and silt—will fail to meet the sameness of elemental composition condition and thus, every sample of these substances with distinct elemental composition will count as a distinct chemical substance.

That is, of course, if the sameness of elemental composition condition really ought to be taken as a blanket condition for sameness of chemical substance. On this reading, Hendry’s sameness of elemental composition condition contradicts his taxonomy: none of the usual mixtures (like concrete, glass, granite, and sand) actually qualify as chemical substances, as the members of these kinds fail to meet the basic requirement for sameness of chemical substance; and yet mixtures are purportedly one of the three fundamental kinds of chemical substance.

The microstructuralist can try and avoid this contradiction by restricting the scope of the sameness of elemental composition condition. The logical next step would be to consider limiting this supposedly general chemical requirement to whatever subset of the chemical substances happen to be the ‘true chemical kinds’, or some such. On this reading the sameness of elemental composition condition would only be a requirement for sameness of chemical kind, not for sameness of chemical substance. Unfortunately, this also generates a contradiction for Hendry since, on his suggested taxonomy, the candidate chemical kinds simply are the chemical substances.

Perhaps at this point it really is starting to seem like mixtures are an entirely hopeless case, and that they should simply be expunged from the list of candidate

chemical kinds. The microstructuralist could hold on to the sameness of elemental composition condition as a requirement for chemical kindhood, and simply concede that the chemical substances don't map onto chemical kinds quite as neatly as had originally been supposed. Sure, many of these mixtures might be standard chemical substances, but that doesn't mean they are 'true chemical kinds', in the strong sense of the term, like the elements.

## **7 Macromolecules, Especially Proteins**

In that case neither are many, if not most, of the compounds. This is because proteins—which are compounds, like water, rather than mixtures, like concrete—will often fail to meet the sameness of elemental composition condition as well. And these macromolecules make up a large portion of the chemical kinds currently known to modern science. In addition to the more than 95,364,853 organic and inorganic substances currently registered with the Chemical Abstracts Service (CAS),<sup>16</sup> there are also 66,063,437 sequences currently registered there—many of which are supposed to represent proteins, by tracking their amino acid sequence (aka primary structure).<sup>17</sup> More conservative estimates of the number of known proteins range over non-redundant sequence databases (from 548,208<sup>18</sup> to 46,714,516<sup>19</sup>), or even count only fully resolved structures (108,124<sup>20</sup>).

Obviously, it is quite difficult to say just how many known proteins there currently are, as the various databases contain anywhere from just over one hundred thousand to just under seventy million. Perhaps unsurprisingly, it is even harder to say how many actual proteins there are, as estimates of this number fluctuate wildly from one hundred million to one hundred billion, and these guesses are not exactly scientifically determined. Finally, because of the number of amino acids and the relatively few restrictions on the length and arrangements of sequences of amino acids in the formation of a protein, there are oodles and oodles more potential proteins than there are atoms in the universe.

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<sup>16</sup> Data obtained at 11:59pm CST on April 15, 2015. The CAS REGISTRY<sup>SM</sup> of inorganic and organic substances includes compounds, mixtures, alloys, salts, minerals, and more. You can watch substance registration happen in real time at: [www.cas.org](http://www.cas.org)

<sup>17</sup> Data obtained on April 15, 2015, when CAS RN 1684475-72-6 was the most recent entry.

<sup>18</sup> Data obtained on April 15, 2015 from SwissProt, which manually annotates and reviews sequences.

<sup>19</sup> Data obtained on April 15, 2015 from TrEMBL, which automatically annotates but does not review sequences.

<sup>20</sup> Data obtained on April 15, 2015 from the Protein Data Bank (PDB).



All this suffices to say that there are many, many, many proteins out there, which are standardly considered members of the same chemical kind—and many of those will fail the sameness of elemental composition condition, if that condition is applied. As it turns out, individuated (token) proteins even at the species-specific (type) level may have different primary structure or amino acid sequence and thus different elemental composition. This is because even these species-specific proteins (not paralogs or homologs) come in various forms with slightly distinct amino acid sequences, which are called ‘variants’. Variants in primary structure (amino acid sequence) that do not seem to affect higher-order structure or function are called ‘alleloforms’ of a protein; variants in primary structure that do seem to produce divergence in protein superstructure and function are called ‘isoforms’.<sup>21</sup>

The widespread existence of alleloforms and isoforms means that the sameness of elemental composition condition cannot be met for many of the proteins that are generally thought of as members of the same kind—by biologists, biochemists, and chemists alike. Though it may seem as though this discussion of proteins is occurring from a biological perspective, this is simply the way in which proteins are individuated throughout scientific practice. There is no alternative, chemical classification system that isn’t subject to these purportedly biological vagaries.

So, what is the resolute microstructuralist to do? Again, they could choose to strike proteins from the list of true chemical kinds, but then it starts to look like the microstructuralist account of chemical kinds really doesn’t fit a large chunk of the compounds as well as many mixtures. Recall that we were considering the sameness of elemental composition condition, its potential scope of applicability, and the relevance of this condition and its scope to the strength of the inference from microstructuralism about a few chemical kinds to chemical kindhood in general. We saw that imposing the sameness of elemental composition condition as a condition for sameness of chemical substance conflicted with the standard taxonomy of chemical substances; we saw how imposing the condition as a condition for sameness of chemical kind would mean that many of the classic chemical substances wouldn’t count as chemical kinds; and now it’s time to consider another restriction on the scope of the condition.

The microstructuralist faces a dilemma here: on the one hand, they could simply concede that perhaps the majority of chemical substances aren’t chemical kinds after all. But this poses a certain kind of risk that should be familiar to many philosophers of science. What started out as a supposedly descriptive project—that of characterizing

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<sup>21</sup> Check out the UniProt database of protein variants among humans, to see some of the isoforms and alleloforms that have already been identified just within our individuated, species-specific proteins.

what the chemical kinds are like, and positing that they are generally microstructuralist—threatens to become prescriptive instead, as the microstructuralist considers adopting the position that chemists are generally wrong about which are the ‘true chemical kinds’. Chemists can, of course, be wrong about this sort of thing, but the point is that in this case the microstructuralist was supposed to be testing a hypothesis about the character of chemical kinds, and they have instead ended up revising their class of test cases to include all and only those that fit with their hypothesis. This is not a good move, in science or philosophy. Unless they want to abandon the descriptive project altogether, and provide an independent line of argument for why only chemical substances that meet the sameness of elemental composition condition should count as ‘true chemical kinds’, this move is not open to the microstructuralist.

On the other hand, the microstructuralist could consider another restriction on the scope of the sameness of elemental composition condition, to just microstructuralist chemical kinds rather than chemical kinds in general. But this generates another tension for the received microstructuralist view—namely, for their broader extrapolative case. I presented Hendry’s ([2006]) version of the extrapolation at the end of Section 3, and drew attention to the tenuous link between claim A, which is about the strong microstructuralist case *for elements*, and claims B and C, which are about the unified, microstructuralist nature of chemical kinds *in general*. Here is how Hendry makes claims B and C in the text:

Now these arguments—against microstructuralism and for pluralism—are local to biology. Chemistry, I argue, is different: the interests that govern its classifications are more unified, and in most cases membership of its kinds is conferred by microstructural properties. ([2006], p. 865)

I’ve been referring to the claim that ‘chemistry, I argue, is different: the interests that govern its classifications are more unified’ as Hendry’s claim B, and to the claim that ‘in most cases membership of its kinds is conferred by microstructural properties’ as his claim C.

It should finally be clear why the generally accepted and presumably harmless sameness of elemental composition condition poses such a problem for the microstructuralist: even with a severely restricted scope, application of the condition threatens the whole extrapolative case; it makes both claims B and C false. The microstructuralist is left only with a significantly limited subset of the chemical kinds—those for which a (generally complicated rather than simple) microstructuralist account

can be given. They can't say that chemical kind membership is, 'in most cases', microstructuralist. Instead, microstructuralist chemical kinds are microstructuralist.

## **8 Abandoning Sameness of Elemental Composition**

There is one final alternative open to the microstructuralist. Instead of restricting the scope of the sameness of elemental composition condition to microstructuralist chemical kinds—abandoning the extrapolative case in the process—the microstructuralist could instead give up on the sameness of elemental composition condition as a blanket condition of any sort. On this deflationary account, sameness of elemental composition would simply be a requirement, albeit a microstructuralist one, of sameness of some chemical substances and not others. Whether it is or is not a requirement would have to be determined on a case-by-case basis; the condition would not be a general requirement of chemical substances on the microstructuralist account.<sup>22</sup>

Could this drastic move save the extrapolative case? I think that claim C, though not claim B, might be saved. Supporting this claim (claim C) would require showing that even the problem cases we've discussed here could still, though they fail to meet the sameness of elemental composition condition, be given a microstructuralist account of their membership conditions. It's hard to know what to say about mixtures, as they are such a disparate class of chemical substances. However, perhaps the microstructuralist could make a general appeal to the fact that many mixtures tend to have characteristic ingredients in certain proportions, despite not containing the same molecules in the same exact proportions in a way that satisfies sameness of elemental composition.<sup>23</sup>

Proteins are going to be an even tougher case. Some philosophers have responded to the evident complexity of proteins by arguing that they are not individuated microstructurally. Matthew Slater ([2009]), for instance, points out that enzymes and other proteins can, despite having the same structure, often perform different functions in different contexts. Slater infers from the fact of this one-to-many structure-to-function relationship that 'enzymes (and proteins more generally) are

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<sup>22</sup> Note that abandoning the sameness of elemental composition condition would not necessarily generate a problem for microstructuralist accounts of water. Although Hendry's disjunctivist account of water requires appealing to the sameness of elemental composition condition, mine does not.

<sup>23</sup> A similar appeal might be just the thing required for a microstructuralist account of alloys—a set of rather neglected substances in the philosophy of chemistry. Sadly, I will not be righting that wrong here. I won't discuss acids, either—another neglected chemical kind (c.f., Stanford & Kitcher [2000]).

individuated, at least in large measure, by their functions' (p. 861). But I disagree with Slater here: on inferential, descriptivist, and prescriptivist grounds.

Inferentially speaking, just because a protein type or even a token protein can do different things in different contexts—as is true in many cases of proteins—does not mean that we have to individuate that protein differently in those different contexts. Although I tend to cook in the kitchen, run outdoors, and I can fall asleep just about anywhere, we don't have to therefore say that I'm a different individual in each of these different contexts; I'm just one individual doing different things in different places.

Descriptively speaking, it's not only that we don't have to choose to individuate proteins by function; it's also that scientists don't, in actual scientific practice. Despite sometimes performing different functions in different contexts, proteins simply aren't individuated by function in the sciences that individuate them (enzymes included). All of the major protein databases were cited at the beginning of Section 7. None of these individuate proteins by function.<sup>24</sup> It's hard to say in philosophically precise terms how proteins are individuated, but the fact is that proteins are routinely and consistently individuated by scientists according to some combination of the properties of primary sequence, protein superstructure, and etiology.<sup>25</sup>

They simply are not, in practice, ever individuated by function. And so, speaking prescriptively now, I think that we should be wary of any philosophical account which says that they are. I also think that Slater should agree with me on this point, since he seems to genuinely care about philosophically capturing scientific practice. His ([2009]) paper, for instance, is filled with assessments of how well one view or another accords with such practice—e.g., 'there are several plausible contenders for the concept of a protein's structure, none of which accommodates biological practice' (p. 852), or "'dappled structuralism" coheres less well with scientific practice than it might initially seem' (p. 856). In sum, despite the fact that there is often a one-to-many relationship between protein structure and function, this fact doesn't require us to individuate proteins by function, and since scientists don't individuate proteins by function, I think it is a mistake for philosophers to claim that they are.

In another line of attack on the possibility of a microstructuralist account of protein individuation, Emma Tobin ([2010]) points to the existence of microstructural disorder within certain proteins: those which are intrinsically structurally disordered,

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<sup>24</sup> Once individuated, proteins are sometimes organized with respect to one another on the basis of function. But the individuation of token proteins into protein types is a different matter than that of how such protein types are then organized with respect to one another. More on the distinction between protein individuation and protein organization in (Havstad [2016]).

<sup>25</sup> This is a topic also pursued in (Havstad [2014]).

often known as Intrinsically Unstructured Proteins (IUPs). Like Slater, Tobin infers from a scientific fact about certain proteins (in this case, that they sometimes display disordered microstructure) to a conclusion about protein individuation (in this case, that it cannot be captured microstructurally). But the presence of microstructural disorder only matters for the purposes of individuation if the disorder occurs amongst those microstructural properties necessary for individuation.

Consider the case of elements: they too exhibit a certain kind of microstructural disorder. Who knows where any given electron in an atom of gold is at any one moment? Yet it is still possible to give a microstructuralist account of what kind of chemical element the atom is, because it is not all of an atom's microstructure, in its entirety, which confers membership in an element kind. It is just particular components of the microstructure—purportedly, the necessary ones. To require shared, ordered rather than disordered microstructure across all members of a chemical kind is much too strong an interpretation of the microstructuralist account. And in the particular case of proteins, I don't think that the kind of superstructural disorder discussed by Tobin is of a kind that threatens protein individuation. That's because, as I already mentioned, I think that proteins are individuated on the basis of a combination of factors pertaining to sequence, structure, and etiology—and having a rigid and determinate superstructure is not one of these factors.

So, neither Slater's [(2009)] nor Tobin's [(2010)] observations about the varying complexities of proteins entail their anti-microstructuralist conclusions. In contrast with these anti-microstructuralist accounts, there is one robustly microstructuralist account of the membership conditions of protein kinds already extant in the philosophical literature. William Goodwin [(2011)] appeals to the fundamentality of amino acid sequence (aka primary structure) in the individuation of proteins in actual laboratory practice. But I think that Goodwin has mistaken a common tool of ostension and partial characterization for one that straightforwardly confers membership in protein kinds. As was the case with water, this initial microstructuralist account turns out to be far too simple to meet the complex chemical requirements that confer membership in the kind.

I've already discussed the variability in primary structure that even species-specific protein kinds can display. Another problem for Goodwin is that there can also be identity of primary structure among proteins that are differentially individuated. This is because protein kinds are individuated not just according to primary structure or amino acid sequence but also according to their etiology—specifically, according to their transcriptional origin in the genome of specific species. There are segments known as 'ultraconserved elements' of genetic inheritance across various species, such that these

gene sequences are identical, producing proteins in their various species that therefore have identical primary structure.<sup>26</sup> But these proteins are still distinctly individuated in scientific practice.<sup>27</sup> I think this shows that any complete account of protein individuation in actual scientific practice must incorporate an etiological component, and cannot simply rely on sequence or structure. In philosophical terms, amino acid sequence, or primary structure, is not sufficient.

Again, as was the case with water, I'm not trying to say that a microstructuralist account of what confers membership in protein kinds is impossible to provide. Rather, I'm saying that doing so is at least going to be much more complicated than the microstructuralists have as yet admitted. It's going to have to be a disjunctive account, and somehow it's going to have to reconceive of at least one etiological constraint microstructurally. I would really, really like to see how that might work.<sup>28</sup>

## 9 Not So Different After All

I said at the outset that I would argue that chemical kindhood, like biological kindhood, is actually quite messy—*contra* the standard assessment that chemical kinds are uniformly neat and tidy microstructuralist kinds. My argument was based on two observations: that the classic cases of microstructuralist chemical kinds are actually rather messy themselves; and that the extrapolation from these few classic cases to a general microstructuralism about chemical kinds is seriously lacking in support. To support the first observation, I looked at the usual cases, showing how and why they are messy. To support the second observation, I looked at the not-so-usual cases, showing how and why they are even messier.

Despite the messiness, I did my best to save or charitably construct alternative,

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<sup>26</sup> For example, the Polypyrimidine Tract Binding Protein 2 (PTBP2) region in humans and marmosets is ultraconserved.

<sup>27</sup> As in, there is human PTBP2 and marmoset PTBP2, and though these proteins have identical sequence and structure, they are not the same kind of protein. They are related, of course, but not the same.

<sup>28</sup> The challenge of dealing with the etiological component of protein kinds is somewhat taken up in (Bartol [2014]), but the solution offered there is to deny microstructuralism when etiological considerations come into play. On this view, proteins are 'biochemical kinds' with dual natures: when treated as chemical kinds, proteins are microstructuralist; when treated biologically, proteins are historical kinds. As was the case with the view offered in (Slater [2009]), this philosophical account strays very far from scientific practice. There simply aren't separate ways of individuating proteins—chemical, biological, or otherwise. (Again: there are different ways of organizing proteins with respect to one another, post-individuation; but this is a separate issue.) Somehow, scientists are routinely and consistently individuating proteins according to some combination of their microstructural and etiological properties; I continue to hold out hope for a philosophical account that captures this feat.

albeit complicated microstructuralist accounts of membership conditions in both the usual and not-so-usual kinds of cases. I tried to preserve at least the possibility of a general microstructuralism about chemical kinds. In other words, I tried to save Hendry's claim C, the claim that membership in chemical kinds is, in most cases, conferred by microstructural properties. To do this, I had to: accept intensions, epistemic aims, details of scientific practice, and historical contingencies into the microstructuralist account of how membership in chemical kinds is determined; create complicated, disjunctive microstructuralist accounts of said membership conditions; abandon the sameness of elemental composition condition for sameness of chemical substance; and expand the concept of microstructural properties to include—among others—atomic number, appreciable molecular density, characteristically disassociated populations, multiple lattice structures, typical ingredients, standard proportions, variable amino acid sequences, flexible macromolecular superstructures, and etiology.<sup>29</sup>

And that expanded list of potential microstructural properties includes only those that have come up in our discussion of the chemical substances built into Hendry's own taxonomy of chemical kinds. But the chemical substances aren't necessarily the only or even the most likely candidate chemical kinds; there are other cases worth considering. For example, chemical bonds are another potentially fundamental kind of chemical kind (see Figure 3):

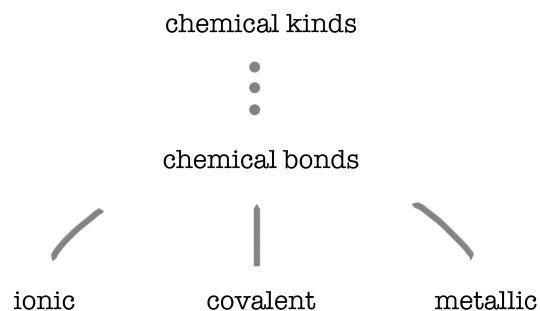


Figure 3: An alternative taxonomy of chemical kinds based on bonds rather than substances.

This taxonomy is woefully oversimplified; and yet an account of chemical kindhood based on it would produce an entirely different arrangement of the chemical entities into chemical kinds than does the standard chemical-substance-based taxonomy. Arrangement of chemical entities according to kind of chemical bond would presumably

<sup>29</sup> I expect this last one to be particularly troublesome, even for the determined microstructuralist.

be dictated by type of relation between chemical substances, rather than by just substance composition or structure. Incorporating these considerations would add things like electrostatic forces and quantum mechanical probabilities to the list of ‘microstructural’ properties. At the very least, the alternative taxonomy shows just how under-supported the general extrapolative case currently is: chemical substances not only aren’t easy to account for microstructurally, they’re also not the only candidate chemical kinds that need to be examined in order to make the extrapolative microstructuralist case.

This is all by way of saying that I haven’t actually saved claim C, I’ve merely kept the dream of it alive, and even doing only that has come at a very steep price. Among other costs, it requires junking Hendry’s claim B, the claim that the interests that govern chemical classification are more unified than those that govern biological classification. The argument goes like this: either chemical kinds are microstructuralist kinds, or they aren’t. Given all the foregoing complications, we might decide that chemical kinds just aren’t microstructuralist kinds after all. And if chemical kinds aren’t microstructuralist kinds, then they probably have that in common with biological kinds. Both kinds of kinds will be well characterized by a variety of classificatory considerations.

Alternatively, we could decide that chemical kinds are still microstructuralist kinds—despite all the complications—although only in an extremely permissive sense of the term. But if chemical kinds are microstructuralist kinds in this sense, then again they probably have that in common with biological kinds. Both kinds of kinds will be well characterized by a variety of ‘microstructural’ considerations.

We can see that this latter conditional is true when we consider just how permissive our interpretation of the term ‘microstructural’ has had to be, in order to cover chemical kindhood as just surveyed. The list of candidate ‘microstructural’ properties isn’t looking particularly unified; nor is it at all clear that, if the list can include all that, it can’t also include whatever might be required to cover biological kindhood. Whichever arm of the disjunction we choose to accept, we’ve collapsed the alleged distinction between the ‘neat’ chemical kinds and those ‘messy’ biological ones.

Just to be clear: I’m not saying that I think the list of candidate microstructural properties would be the same when giving a (necessarily permissive) microstructuralist account of biological kindhood, as it would be when giving a (similarly permissive) microstructuralist account of chemical kindhood. That would be making a first-order comparison of the interests that govern chemical and biological kindhood.

But Hendry’s claim B is a second-order claim: it says that whatever the first-order interests that govern chemical classification are, these are more unified than those that



govern biological classification. I'm disputing claim B, on the grounds that the interests that govern chemical classification do not, upon close examination, look at all unified (whether we characterize them as 'microstructural' or not). And I'm making an alternative second-order claim: since neither the interests that govern chemical classification nor the interests that govern biological classification appear to be especially unified, in this respect chemistry and biology are not so different after all.

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## References

- Bartol, J. [2014]: 'Biochemical Kinds', *British Journal for the Philosophy of Science*, **0**, pp. 1–21.
- Boyd, R. N. [1991]: 'Realism, Anti-foundationalism, and the Enthusiasm for Natural Kinds', *Philosophical Studies*, **61**, pp. 127–48.
- Boyd, R. N. [1999]: 'Kinds, Complexity and Multiple Realization', *Philosophical Studies*, **95**, pp. 67–98.
- Brady, J. E. and Senese, F. [2004]: *Chemistry: Matter and Its Changes* (4<sup>th</sup> ed.), Hoboken, NJ: Wiley.
- Chakravartty, A. [2007]: *A Metaphysics for Scientific Realism: Knowing the Unobservable*, Cambridge: Cambridge University Press.
- Dupré, J. [1993]: *The Disorder of Things: Metaphysical Foundations of the Disunity of Science*, Cambridge: Cambridge University Press.
- Dupré, J. [2001]: 'In Defense of Classification', *Studies in History and Philosophy of Biological and Biomedical Sciences*, **32**, pp. 203–19.
- Ellis, B. [2001]: *Scientific Essentialism*, Cambridge: Cambridge University Press.

- Fodor, J. A. [1990]: *A Theory of Content and Other Essays*, Cambridge, MA: MIT Press.
- Goodwin, W. [2011]: 'Structure, Function, and Protein Taxonomy', *Biology & Philosophy*, **26**, pp. 533–45.
- Griffiths, P. E. [1999]: 'Squaring the Circle: Natural Kinds with Historical Essences', in R. A. Wilson (ed.), *Species: New Interdisciplinary Essays*, Cambridge, MA: MIT Press, pp. 209–28.
- Havstad, J. C. [2014]: *Biochemical Kinds and Selective Naturalism* (doctoral dissertation, UC San Diego).
- Havstad, J. C. [2016]: 'Protein Tokens, Types, and Taxa', in C. Kendig (ed.), *Natural Kinds and Classification in Scientific Practice*, London: Routledge, pp. 74–86.
- Hendry, R. F. [2006]: 'Elements, Compounds, and Other Chemical Kinds', *Philosophy of Science*, **73**, pp. 864–75.
- Hendry, R. F. [2012]: 'Chemical Substances and the Limits of Pluralism', *Foundations of Chemistry*, **14**, pp. 55–68.
- Kripke, S. [1980]: *Naming and Necessity* (revised edition, originally published [1972]), Cambridge, MA: Harvard University Press.
- LaPorte, J. [2004]: *Natural Kinds and Conceptual Change*, Cambridge: Cambridge University Press.
- Millikan, R. G. [1999]: 'Historical Kinds and the "Special Sciences"', *Philosophical Studies*, **95**, pp. 45–65.
- Needham, P. [2000]: 'What is Water?', *Analysis*, **60**, pp. 13–21.
- Needham, P. [2011]: 'Microessentialism: What is the Argument?', *Nous*, **41**, pp. 1–21.
- Pauling, L. [1988]: *General Chemistry* (revised edition, originally published [1947]), New York: Dover.
- Putnam, H. [1973]: 'Meaning and Reference', *The Journal of Philosophy*, **70**, pp. 699–711.
- Putnam, H. [1975]: 'The Meaning of "Meaning"', in K. Gunderson (ed.), *Language, Mind, and Knowledge: Minnesota Studies in the Philosophy of Science*, Minneapolis: University of Minnesota Press, **7**, pp. 131–93.
- Slater, M. H. [2009]: 'Macromolecular Pluralism', *Philosophy of Science*, **76**, pp. 851–63.
- Slater, M. H. [2015]: 'Natural Kindness', *British Journal for the Philosophy of Science*, **66**, pp. 375–411.
- Stanford, K. and Kitcher, P. [2000]: 'Refining the Causal Theory of Reference for Natural Kind Terms', *Philosophical Studies*, **97**, pp. 99–129.
- Tobin, E. [2010]: 'Microstructuralism and Macromolecules: The Case of Moonlighting Proteins', *Foundations of Chemistry*, **12**, pp. 41–54.