

# “Authority and Natural Kind Essence”

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## Abstract:

If natural kinds have microstructural essences they have them independently of rules for the application of kind terms. This suggests that what those rules are should make no difference to the essences being discoverable. I present two thought-experiments that suggest otherwise, however. Each shows an authority’s application of rules creates the appearance of there being kind essences; absent those rules, the appearance vanishes. This suggests natural kind essences are not independent of authority-sanctioned rules.

## 1. Introduction

Due largely to Kripke and Putnam there is a familiar story about the metaphysics of natural kinds, and the semantics and meta-semantics of natural kind terms.<sup>1</sup> The central tenets of this story include the following:

- (i) Discovering the essential properties of natural kinds is one goal of scientific investigation.
- (ii) The essential properties of natural kinds are microstructural.<sup>2</sup>
- (iii) Vernacular speakers are often acquainted with natural kinds long before their essences are discovered.
- (iv) Vernacular speakers typically apply natural kind terms on the basis of the readily observable but nonessential qualities of those kinds.
- (v) Because natural kinds have microstructural essences, natural kind terms apply only to microstructurally uniform entities.

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<sup>1</sup> Although there are good reasons to distinguish Kripke from Putnam on these matters (Williams 2011; Hacking 2007a), I will follow orthodoxy in referring to a “Kripke-Putnam” account.

<sup>2</sup> ‘Microstructure’ and ‘microstructural’ have become catchall terms, encompassing both *microcomposition*, e.g. the chemical formula H<sub>2</sub>O, and *microstructure*, i.e. the specific spatial arrangement of atoms or molecules, of which there may be several for any given compound (Needham 2002). Even so, I will follow custom and use ‘microstructure’ to cover both.

- (vi) Consequently, if vernacular speakers apply a kind term to apparently similar yet microstructurally distinct entities, then speakers either *misuse* a natural kind term or else that kind term is not a *natural* kind term after all.

Though many of these tenets are logically independent, the whole package— which I will call ‘the discovery account’—has of course been very attractive to many philosophers. And it is not hard to see why. For one, the discovery account seamlessly interweaves science, metaphysics, and language. According to the discovery account linguistic rules are ultimately justified or grounded by a realist metaphysics of natural kinds and their essences, and a robust scientific realism more generally. Tenet (v) in particular spells out this connection: because natural kinds are objectively a certain way (namely, microstructurally individuated), natural kind terms thereby function in a certain way (by applying only to microstructurally similar entities). And this in turn suggests something like (vi): to use kind terms without regard for microstructural boundaries would not only violate a linguistic rule but would also constitute a metaphysical (or scientific) mistake. To employ a now prevalent metaphor it would constitute a failure to “carve at nature’s joints”.

Suppose that such mistakes were made, however, or speakers simply followed alternate rules. This should make no difference to the essence of a natural kind being discernible: for on the discovery account’s realism natural kinds have essences independently of (and prior to) the rules for applying natural kind terms. In this paper, however, I present two thought-experiments that suggest otherwise. Each isolates the contribution of linguistic rules from the mind-independent world with the following result: natural kinds only appear to have microstructural essences when one is already committed to an authority-sanctioned linguistic rule- in particular, a rule that instructs speakers to apply kind terms on the basis of microstructure. Absent that rule or commitment the appearance vanishes. I conclude from

this that kinds having microstructural essences is mere appearance. I thus defend a version of conventionalism: rather than there being deep or substantive facts about kind essences, there are only appearances due to accepting a conventional rule.<sup>3</sup>

Of course, if kinds do not actually have microstructural essences then *a fortiori* science cannot discover their microstructural essences. So my arguments also suggest the discovery account is mistaken regarding at least one of the aims of science. I must emphasize, though, that my position is not tantamount or otherwise committed to anti-realism about the posits of science. The conventionalism I advocate is quite targeted, and is perfectly compatible with scientific realism more generally. In this paper I take for granted the reality of the individuals and kinds posited by science, as well as the reality of micro/macro “giving rise” relations, such as H<sub>2</sub>O giving rise to water-ish properties, and nephrite and jadeite giving rise to jade-ish properties. What my conventionalism denies is a further step- that water is a natural kind but jade is not because water has a microstructural essence but jade does not. So while my arguments imply that Kripke-Putnam style reasons are not sufficient for rejecting conventionalism about essences and naturalness, they also imply that (scientific) realism about kinds can—and should—be disentangled from the faulty (micro)essentialism that constitutes the discovery account.<sup>4</sup>

## 2. Jade and the division of linguistic labour

Before presenting the thought-experiments some background is necessary. In one of the discovery account’s founding documents Putnam (1975a, p. 10) claims that ‘water’ refers

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<sup>3</sup> A clarification: as we will see in section 7, I do not deny trivial essences such as ‘H<sub>2</sub>O is (essentially) H<sub>2</sub>O’. Instead, it is deep or substantive essences (in need of discovery) that I deny.

<sup>4</sup> Space forbids examining how my arguments against microstructuralist realism apply to anti-microstructuralist realisms about kind essence, such as that defended by Oderberg (2007).

only to H<sub>2</sub>O even if the word is uttered in the year 1750, when the typical speaker had no conception of hydrogen or oxygen. So when the constitution (or essence) of a substance is discovered, on this account, it is also discovered what a vernacular kind term had referred to all along.

It is also possible that speakers had been misapplying a term all along (principle vi), however. For if natural kinds are individuated microstructurally (principle ii) the discovery that speakers had applied a kind term to substances with distinct microstructures is also the discovery that speakers misspoke. This suggests that in such a case, speakers would—or should—stop applying the same term to both substances. LaPorte (2004) argues that the history of jade in China falsifies this aspect of the discovery account, however. LaPorte describes that history as follows. For millennia the Chinese referred to nephrite (a compound of calcium, magnesium, and iron) by the word ‘yü’. (English speakers have used the word ‘jade’, though not for so long.) For much of this period yü had a status and function in Chinese culture similar to that which gold has had in the West (p. 95). But in the 18<sup>th</sup> and 19<sup>th</sup> centuries a stone appearing highly similar to nephrite but with an unrelated composition was imported to China. The French scientist who eventually discerned its composition dubbed the stone ‘jadeite’.<sup>5</sup> Even absent sophisticated chemical analyses to the skilled Chinese artisan jadeite was subtly yet perceptibly different than nephrite. Nonetheless jadeite was treated just as nephrite had always been because the two compounds have highly similar artisanal and aesthetic qualities. So jadeite was accepted as jade, and the term ‘yü’ was applied to both.

LaPorte contends it was not a mistake to apply ‘yü’ to both jadeite and nephrite. For LaPorte argues that “speakers’ dispositions indicate the proper use of a term”, and “Chinese

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<sup>5</sup> The scientist was Alexis Damour, and jadeite is a compound of sodium, aluminium, silicon, and oxygen.

speakers have indicated by their actions *what they would say* [i.e. their dispositions] were they presented with a new substance like what they had called ‘jade’ except in its microstructure” (p. 97-8, original emphasis). So the “moral of jade”, LaPorte writes, is that “the proper extension of a term is determined not only by microstructure but also by relatively easily observed properties like colour, texture, weight, hardness, taste, and so on” (p. 100). And if ‘jade’ may legitimately refer to both nephrite and jadeite then it obviously cannot be discovered that jadeite is not jade, nor that the essence of jade is nephrite (on analogy with the essence of water being discovered to be H<sub>2</sub>O).

Bird (2007; 2010) rejects LaPorte’s conclusion. Because jade is a gemstone, Bird argues, parties with commercial or artistic (i.e. non-scientific) interests have a stake in how ‘jade’ is used. As a result these parties may use the term as it suits their interests, regardless of what scientists discover. But this does not entail that such uses are not misuses, Bird claims.

Recall that LaPorte contends that “speakers’ dispositions indicate the proper use of a term”. But which speakers? For LaPorte it is any and all Chinese speakers, including the jewellers and artisans who trade in jade. Bird on the other hand is keen to privilege the scientists: thus, if the chemist restricts her use of ‘jade’ to nephrite then only nephrite is included in the extension of ‘jade’. For Bird the non-scientist’s application of the term is simply irrelevant to its meaning.<sup>6</sup>

Bird’s argument speaks for the discovery account, as it leans on Putnam’s notion of a division of linguistic labour. According to Putnam only a “special subclass of speakers” have the job of determining whether something is an instance of a kind (1975a, p. 13).

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<sup>6</sup> In order to “distinguish the competing interests” of the scientist and non-scientist, Bird proposes the following counterfactual: “how would the scientists have ordered their classifications in the absence of such [non-scientific] pressures?” (2007, p. 12). For Bird answering this question will (probably) lead to “the view that jade really was just nephrite but that the makers of jade artefacts decided that it was in their interests to allow jadeite also to count as jade” (ibid., p. 4).

Consequently only these experts may fix how kind terms are to be applied. And for Putnam and Bird, of course, these experts are scientists. Even so it may be misleading to say that scientists fix the rules for applying kind terms. For if kinds have microstructural boundaries then using kind terms irrespective of these boundaries constitutes a mistake (i.e. a failure of joint-carving). Consequently, it is no more up to the scientist than the artisan to choose some other application rule, based on however she happens to be disposed. Instead the world itself fixes the correct rule, to which the scientist as much as the artisan is subject. So according to the discovery account the scientist's role in the division of labour is not to create or construct application rules for 'is gold' or 'is the same kind as', say. Instead it is to discover the rules and then disseminate the knowledge. The scientist is thus taken to be an intermediary between mind-independent essences and the laypeople who lack the expertise to discover these for themselves. Put another way, the scientist discerns the joints of nature, and, *qua* expert, instructs the folk to talk accordingly. And from this Bird's criticism can be seen to follow: if artisans apply 'jade' to both nephrite and jadeite for commercial or artistic reasons they ignore the rules and thereby fail to carve at the joints. They either misuse a natural kind term or else 'jade', being a commercial or artistic term, is not a natural kind term at all (Bird 2007, p. 4).

But is it the case that the scientists' role is intermediary or passive? Fortunately this is testable. For if scientists merely report the facts about kind essences, altering the (social) role of scientists should not affect the appearance of there being such essences. But as the following thought-experiment shows, this is not born out. For in a hypothetical society

wherein scientists do not have the social authority to fix the use of kind terms the discovery account's plausibility is greatly diminished.<sup>7</sup>

### 3. An alternate division of linguistic labour

Consider a hypothetical planet upon which there is an aristocratic society. Suppose these aristocrats are fond of a sparkling red liqueur which just so happens to occur naturally. This liqueur was first imbibed by the society's founder and subsequently by his many descendents exclusively for commemoration. Because the drink is associated with aristocratic status peasants are also prohibited from drinking it. Aristocrats call it 'intensahol'.

Suppose an aristocrat employed a scientist to analyze the drink, which revealed an XYZ microstructure. Though the scientist believed he had thereby discovered the essence of intensahol to be XYZ he had another scientist corroborate. Upon testing, though, the second scientist detected an ABC microstructure. So which was the essence of intensahol? Had they discovered that it was really ABC or XYZ? Or, perhaps, had they discovered that a kind of natural drink was not really a natural kind after all?

Further testing seemed required. Yet each test reconfirmed that exactly half of each sample contained ABC and half XYZ.<sup>8</sup> Suspecting there was a hidden indexical component to natural kind terms, one scientist suggested that if the stuff first ostensibly dubbed 'intensahol' was found, it could be discovered whether ABC or XYZ bears the *is the same kind as* relation to *that*. Luckily the goblet from which intensahol was first imbibed was

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<sup>7</sup> In response to Bird, LaPorte (2010, p. 109-110) suggests that even in his own Anglo-American society, scientific interests need not trump "cultural interests" vis-à-vis the application of terms.

<sup>8</sup> A note on the plausibility of this story: it is highly unlikely if not physically impossible that different compounds would be entirely indistinguishable except in having different micro-structures. So I do not suppose that all macroscopic samples of ABC and XYZ are exactly similar in their superficial or observable qualities. All that is required for the story is that nobody has yet noticed, or taken an interest in, the observable differences among samples.

accessible and still contained microscopic remains. Even so the scientists found ABC and XYZ in exactly equal proportions. Nonetheless the scientists realized some good might come of this. For although peasants were forbidden to drink intensahol, either XYZ or ABC must not be intensahol, in which case the peasants would be able to drink *that*. But the aristocrat thought otherwise: “In our division of linguistic labour”, he said, “it is the aristocrats who decide how words are applied. So even though distinct microstructures give rise to intensaholic properties the word ‘intensahol’ is defined by the stereotypical properties associated with the drink, and, as a result, the role it plays in our community. So any red sparkling liqueur which signifies status just *is* intensahol. Moreover, you cannot have discovered its essence, for its essence is to be sparkly, red and intoxicating. And we’ve known that ever since we first pointed at a sparkly, red beverage and said “this is to be called ‘intensahol’”.”

#### **4. Authority, rules, and the appearance of essence**

Part of what the thought-experiment is intended to bring out (however dramatically) is that authority may play a significant role in establishing rules for the use of kind terms. Nonetheless one might resist attributing any philosophically important role to authority in such scenarios. For as we saw earlier the discovery account has it that kind essences (and principles of individuation) determine how kind terms ought to be used (if they are to carve at reality’s joints), regardless of how they are used. So it seems to follow that who claims to have linguistic authority is irrelevant.

It is worth noting that a thesis about priority is built into this line of thought. On the discovery account kind essences are prior to application rules for kind terms: it is because kinds have microstructural essences that a given kind term ought to only apply to

microstructurally similar entities. And being prior, the essence of a kind does not depend on application rules, and neither do application rules depend on any supposed linguistic authority. As described above the scientist is an intermediary who reports the facts- not someone who invents them or establishes them by fiat. So it seems the discovery account may safely ignore issues of social authority such as those illustrated in the thought-experiment.

But this is a mistake. For an opposing priority thesis is not only coherent, but as we will see summarily, better supported. On this opposing view it is the application rules that determine what counts as the essence of a kind. For example, it is because one accepts the rule <only apply 'water' to H<sub>2</sub>O> that anything that is not H<sub>2</sub>O fails to count as water. And as nothing else can be called 'water' (without equivocation) it is the rule that creates the necessity of all water being H<sub>2</sub>O. If one were to accept the rule that 'jade' is to be applied to any hard, green, ornamental stone, however, then as a result it would be necessary only that every instance of jade have those macroscopic features, not that they have any particular microstructure. On this view more generally the *necessity* of a kind's possessing any particular feature is due to the linguistic rule. As such, kind essences depend on which (conventional) linguistic rules speakers follow.

So of course the important question becomes how to decide whether essence determines application rules or vice versa. Fortunately this is testable. If the discovery account is right then kinds have microstructural essences independently of what rules language-users actually follow, or who has (or claims to have) the authority to set these rules. So if the discovery account is right then even if one alters the division of labour, or speakers follow alternate application rules, the microstructural essences of natural kinds should still be discernible (assuming they ever are). If (apparent) essence is dependent on authority or

application rules, however, then (apparent) essences will shift as rules are shifted, or, if all such rules are removed, one will find a scenario bereft of necessity or essence.

And it is exactly this latter scenario that one finds in the thought-experiment. Given a division of linguistic labour in which the scientist does not have authority it simply does not appear that intensahol has a microstructural essence. Rather, because 'intensahol' is said to apply to anything with certain macroscopic properties it appears that the essence of intensahol is just those macroscopic properties (as the aristocrat claims is the case). So the thought-experiment suggests it is the linguistic rules that determine essence and not vice versa. Contra the discovery account.

There is an obvious objection here: this result only occurs because ABC and XYZ exist in equal proportion. And if such a distribution rarely if ever occurs in nature, then one could argue that in most (if not all) actual cases there is a plainly observable microstructural essence of any given natural kind. But the equal proportion of ABC and XYZ is a red herring: whether (or how often) microstructurally distinct compounds exist in the same place in equal quantities is irrelevant. For if a kind's microstructure (and thus its essence) is intrinsic to it, as I take the discovery account to suppose, then a kind's essence does not depend on the actual or possible distribution of nearby stuff. And if the application rules for kind terms are determined by these (intrinsic) kind essences then whether a kind term may legitimately apply on the basis of macroscopic qualities should not itself be affected by something extrinsic to a kind, such as its instances being located near something macroscopically similar. So what the equal distribution of stuffs in the thought-experiment serves to show is that whether a kind even *appears* to have a microstructural essence is contingent on there being no macroscopically similar stuff nearby. Perhaps ironically,

Putnam himself comes to this very conclusion when considering his famous Twin Earth case. According to Putnam,

if H<sub>2</sub>O and XYZ had both been plentiful on Earth, then.. it would have been correct to say that there were *two kinds of 'water'*. And instead of saying that 'the stuff on Twin Earth turned out not to really be water', we would have to say 'it turned out to be the *XYZ kind of water'* (1975a, p. 162, original emphases).

So even Putnam concedes that the appearance of water necessarily being H<sub>2</sub>O is itself contingent on there being no (Twin Earth) XYZ nearby. But even in a case where there is no such stuff and it does *appear* that a kind has a microstructural essence the possibility still remains that this appearance is due to the linguistic rule, as described above.

Of course one might just insist that a kind's microstructural essence is directly— and reliably—observable. Now, a naïve appeal to observation is not in general a sound strategy for rebutting the charge that some observation is mere appearance. But even putting this aside the view is simply false. For thinking that two macroscopically similar yet microstructurally different substances are instances of the same natural kind is not a reason to get one's eyes checked; this thought is not the result of defective vision or less-than-stellar observational skills. Put another way, the anti-microstructuralist will accept the same observable data as the microstructuralist: namely, that two distinct microstructural compounds give rise to similar macroscopic properties. Even so they disagree over the essence of a kind, and, relatedly, how kind terms are to be applied. So which is right is simply underdetermined by what is directly observable.

So the defender of the discovery account might try an indirect approach. For example, one might argue that the supposition that kinds have microstructural essences yields inductive, explanatory, or predictive success, and that this is reason to adopt the thesis. But the same success is available to the anti-microstructuralist. Applying 'jade' to both nephrite and jadeite (or 'intensahol' to both ABC and XYZ) does not preclude one from

explaining why nephrite and jadeite yield similar properties (or why ABC and XYZ yield intensaholic properties), nor from making successful predictions or inductions over nephrite, jadeite, or jade in general.<sup>9</sup> So this move fails as well.

I thus conclude that if one drops the microstructuralist application rule (and the concomitant authority of the scientist) the appearance that kinds have microstructural essences simply vanishes. By contrast, when one is committed to a particular application rule one perceives exactly the sort of necessity consistent with the rule. So the discovery account gets it backwards: it is not that kinds have microstructural essences which in turn determine application rules, but rather it is the (conventional) rule that determines an apparent essence. Accordingly it cannot be discovered that kinds have microstructural essences, and a key plank in the discovery account is false.

## 5. Objection: but these are not *natural* kinds

Another likely line of objection is that kinds such as jade are not *natural* kinds at all (Bird 2010; Contessa 2007, p. 235; Braddon-Mitchell 2005, p. 866). More generally one might adopt what I will call *the rarefied conception* of natural kinds, according to which only kinds that meet a certain rarefied standard—such as microstructural uniformity—count as natural.<sup>10</sup> By adopting this conception one might suppose the discovery account avoids the problems discussed above.

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<sup>9</sup> One might object that predictions or inductions over intensahol may be faulty because what one may predict or induce on the basis of ABC may differ from what one may predict or induce on the basis of XYZ. But this is true any time one makes inductions over general kinds that have instances that differ specifically. For example, inducing over leptons may lead one astray in cases where the specific differences between electrons and muons are relevant. But it's not clear that this has anything to do with essences *per se*—but rather with a lack of specificity.

<sup>10</sup> Hacking (2007b) contrasts rarefied kinds with “mundane kinds”. For what I take to be a similar distinction (from the linguistic rather than metaphysical side), see Wikforss (2010).

The problem, though, is the proponent of the discovery account may not be able to avail herself of the rarefied conception of kinds. For on the discovery account vernacular speakers are supposed to be acquainted with natural kinds prior to scientific inquiry. But few rarefied kinds are identified prior to scientific investigation; kinds such as *electron* and *quark* are obviously not dubbed by non-scientists first and then investigated later (cf. Beebee and Sabbarton-Leary 2010). By contrast, the preponderance of kinds identified prior to scientific investigation, such as mud, air, oil, or rain turn out not to be microstructurally uniform (Zemach 1976, p. 121). Moreover, on the Putnam-Kripke account the meaning of kind terms is established by acquaintance with a kind and a concurrent act of ostension.<sup>11</sup> But as most instances of rarefied kinds are unobservable *qua* rarefied kind (at least to the naked eye) they are not objects of acquaintance or ostension, except indirectly through whatever superficial macro properties they yield. But then we're back into non-rarefied territory, i.e., kinds grouped by observable macro properties rather than microstructural uniformity. So the rarefied conception makes the Putnam-Kripke meta-semantics of kind terms largely irrelevant to the meta-semantics of *natural* kind terms (cf. Sabbarton-Leary 2010).<sup>12</sup>

Furthermore, moving to the rarefied conception due to jade-inspired difficulties is arguably *ad hoc* (LaPorte 2004, p. 97). Now, Bird does attempt to rebut this charge, claiming that “chemists regard it as a necessary condition of being the same substance that two samples share the same or very similar composition and engage in the same reactions” (2010, p. 129). But the beliefs of practising chemists need not be decisive. For if chemists adopt this rule because it is pragmatic or customary then discounting kinds such as jade from counting

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<sup>11</sup> Putnam’s idea of a hidden indexical component and Kripke’s (1980) idea of an initial baptism each invoke non-scientists fixing the reference of kind terms ostensively.

<sup>12</sup> Moreover, it is precisely this acquaintance-cum-ostension (and the causal chains subsequent to it) that ground claims such as Putnam’s that ‘water’ referred to H<sub>2</sub>O even in the year 1750, when the typical speaker had no conception of hydrogen or oxygen.

as natural, even if not *ad hoc*, would still lack epistemic import; i.e., chemists following the rule would be not indicative of a corresponding reality.

Of course there may be more substantive ways of defending the realist construal. For instance one might think that if the sameness-of-composition rule is conventional then the scientist's judgement regarding kind membership is not ultimately justified (or is at least no more justified than a rule imposed by an arbitrary social aristocracy). But as the scientist's judgements *are* justified (in a way the aristocrat's are not) it follows that the rule is not conventional. Instead the judgment must be grounded by the way the world really is. But this reply does not succeed either. For the conventionalist can grant that the scientist has epistemic superiority or expertise. To see this suppose part of the scientist's expertise-cum-epistemic-authority consists in her being uniquely placed to discover how and why certain microstructures give rise to certain macroproperties- such as why H<sub>2</sub>O gives rise to the familiar properties of water. But of course jadeite gives rise to jade-ish properties just as H<sub>2</sub>O gives rise to water-ish properties- even if only water is thought a natural kind. More generally these sorts of micro/macro "giving rise" relations obtain regardless of the supposed naturalness of the kinds in question. So the issue between the microstructuralist and conventionalist is not whether scientists have *any* genuine epistemic authority or expertise in contrast to an aristocrat, say; of course they do. Instead the issue is whether the epistemic authority regarding micro/macro giving rise relations extends to judgements about which kinds are genuinely natural.

I contend that it does not- even if the scientist's (other) judgments are better justified than anyone else's. For mind-independent facts about essences (or carving at the joints) are not the only grounds for justifying a taxonomy (no matter what practicing chemists think). Pragmatic criteria (such as simplicity or specificity) might also justify the classificatory

practice. Now, it is at this point in the argument that the microstructuralist might wish to play a favourite trump card- the famous “no miracles” argument (due to Putnam 1975b, p. 73). In this context that argument runs as follows. Because science is successful and science classifies by microstructure, one is warranted in inferring that the best—and perhaps only—explanation of this success is that microstructural classifications carve at the joints, i.e. that natural kinds really do have microstructural essences. Otherwise, the reasoning goes, the success of science would be a miracle. Even if generally effective<sup>13</sup> the “no miracles” argument is too coarse-grained to carry weight in this context, however. For the question here is not a global scientific realism or the existence of unobservables, say, but something far more specific. As indicated at the outset, the conventionalism I am defending is perfectly happy to grant that the individuals and kinds posited by science are objectively real, and that H<sub>2</sub>O really does give rise to water-ish properties, and that nephrite and jadeite really do give rise to jade-ish properties. What this conventionalism denies is a further step- that water is a natural kind but jade is not, because water has a microstructural essence but jade does not. And for a “no miracles” style of argument to apply *here*, kinds having microstructural *essences* would have to be a crucial (if not essential) factor in the success of science regarding H<sub>2</sub>O, nephrite, and jadeite, *inter alia*. But it is not. For as argued above, all the predictive, inductive, and explanatory success available to the realist about micro/macro relations is available to the sort of conventionalist I have in mind- because she too is a realist about the basic entities of which science’s success is made. Whether positing essences in addition would make science even *more* successful than it would be without them is far from clear.

So what the discovery account needs is not the authority of scientists nor their received judgment. Instead what is needed is something in the world as such that would

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<sup>13</sup> Though see Frost-Arnold (2010) and Mizrahi (2012) for criticism.

make it metaphysically incorrect—and not just less practical—to consider kinds constituted by macroscopic similarity to be natural. That is, they would need to show that holding jade to be a natural kind fails to carve at the joints. (Moreover, this would have to be done by appealing to something other than what the conventionalist described above can also accept, such as instrumental success). My argument has been that there is no such thing, however—as evidenced by the fact that when one takes away the linguistic application rules one is left with a scenario bereft of facts about which kinds are natural.

## 7. Are modal facts born before baptisms?

Turn now to the second thought-experiment. Suppose a new compound with DEF microstructure is discovered. Does this reveal anything about the essence of the kind DEF? Granted, it is necessary that DEF is DEF. And insofar as DEF could not exist without being DEF one might be inclined to say the essence of DEF is DEF. But this is certainly not an essence that is discovered, nor are these modal facts substantive or interesting. That DEF is (necessarily or essentially) DEF is an entirely trivial fact, and hardly a paradigm of what science seeks to discover.

Next suppose that DEF is later named ‘setium’. Then by substitution from ‘DEF is DEF’ we get ‘setium is DEF’. And assuming ‘setium’ and ‘DEF’ are both rigid designators the result is (presumably) a necessary identity. So we now have two necessary identities: DEF is DEF, and setium is DEF. Of course, the former identity is trivial. What about the latter? ‘Setium is DEF’ certainly looks like it expresses a substantive or deep fact—just as ‘water is H<sub>2</sub>O’ does. So, does ‘setium is DEF’ express the sort of substantive or deep essentialist fact that science is said to seek?

Although only ‘DEF is DEF’ is an instance of ‘P is P’ (and is thus obviously trivial), that setium is DEF is not any more substantive; after all, ‘setium’ was knowingly introduced as a name for DEF.<sup>14</sup> Moreover, there is obviously no *discovery* that setium is DEF. Yet ‘setium is DEF’ is of the same form as ‘water is H<sub>2</sub>O’. And of course, water’s being H<sub>2</sub>O (and gold’s being the element with atomic number 79, etc.) are supposed to be substantive and deep facts, paradigms of what science seeks to discover according to the discovery account. So whence the difference between ‘setium is DEF’ and ‘water is H<sub>2</sub>O’? The only difference appears to be when they were named: ‘water’ (and ‘gold’) are (and were) vernacular terms, in use prior to the scientific investigation of the kinds they picked out. ‘Setium’, by contrast, is introduced afterwards.

Order shouldn’t matter, however: a fact’s being substantive or trivial should not depend on how or when it is picked out. Moreover, the facts in question are supposed to be mind-independent (and joint-carving) facts, out there waiting to be discovered. So the facts expressed by ‘setium is DEF’ and ‘water is H<sub>2</sub>O’ should not differ with respect to being substantive or trivial. Consequently either ‘setium is DEF’ expresses a substantive and deep fact (despite ‘setium’ being explicitly introduced as another word for ‘DEF’), or else water’s being H<sub>2</sub>O is not as substantive or deep a fact as is typically assumed. I find the latter far more plausible.

Consider a variant on the second thought-experiment. Suppose two compounds are discovered: one with microstructure GHI, the other with JKL. Further suppose both are suited to play the R role. Now, suppose that someone points to GHI and says “I name this kind ‘rollium’”. The question is this: is the essence of rollium GHI?

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<sup>14</sup> See Beebe and Sabbarton-Leary (2010, p. 164-5) for nonfictional examples of this kind.

A ‘yes’ answer is too quick. For although one might think ‘rollium’ rigidly designates GHI, it could instead be a name for the functional kind R: for pointing at GHI is also pointing at (one kind of) R. And if ‘rollium’ picks out R, then JKL is also rollium, which suggests the essence of rollium is not GHI. So in order to know whether the essence of rollium is GHI, we need to know whether ‘rollium’ picks out GHI or R. But what experiment could show this? What information is missing? We know JKL is not GHI, and we know both are R. It seems the only missing element is a decision: namely, the decision to use ‘rollium’ as a functional-kind term as opposed to a microstructural-kind term. But then either the essence of rollium being GHI is conventional, or, if it is nonconventional, it is no more substantive than GHI is GHI, or setium is DEF.

## 8. Conclusion

In this paper I presented two thought-experiments. Each isolates language from the attempt to discern the essences of natural kinds. In the first, rules for the application of kind terms are declared to apply on the basis of macroscopic similarity, independently of what is discovered to give rise to these similarities. In the second, physical structures are discovered first and only later are kind names introduced. In neither case, I argued, can one discern a (nontrivial) essence that would determine the correct way to use the relevant terms. Instead I argued that what appears as the (nontrivial) essence of a kind varies as one alters (or introduces) the application rules for kind terms. So microstructural essences are conventional, or else real but trivial.

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