

# Natural Kinds and Natural Kind Terms: Myth and Reality

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## **Abstract**

The paper examines the role of natural kinds in semantic theorizing, which has largely been conducted in isolation from relevant work in science, metaphysics and philosophy of science. We argue that the Kripke-Putnam account of natural kind terms, despite recent claims to the contrary, depends on a certain metaphysics of natural kinds; that the metaphysics usually assumed – microessentialism – is untenable even in a ‘placeholder’ version; and that the currently popular HPC theory of natural kinds is correct only to an extent which fails to vindicate the Kripke-Putnam account. This undermines the metasemantics required for anti-descriptivist semantics.

## **1. Introduction**

Philosophers invoke natural kinds for many purposes. Theories of natural kinds are said to explain the possibility of successful induction; to underpin externalist semantics; to warrant eliminativism with respect to psychological categories such as emotions and concepts.<sup>1</sup> Over the last decades, they have become a staple in the philosophers’ tool-kit. This paper concerns their role in semantic theorizing.

The background is familiar. Kripke and Putnam criticized traditional, descriptivist accounts according to which the meanings of kind terms are given by a set of associated descriptions

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<sup>1</sup> For the latter use, see (Griffiths [1997]) and (Machery [2009]).

(typically relating to easily available, observable features), determining the extensions of the terms. Against this, Kripke and Putnam argued that the natural kind terms stand apart from other kind terms in that associated descriptions fail to determine their extensions. Putnam presented us with Twin Earth, suggesting that were we to encounter a liquid superficially identical to Earth water but with a different chemical composition, we would not judge it to be water, and Kripke argued that natural kind terms, unlike definite descriptions, are rigid designators. As an alternative to descriptivism, they proposed a version of semantic externalism: the thesis that extensions of natural kind terms are determined, in the actual and possible worlds, by underlying, essential features of samples in the speaker's environment. We shall label this claim the 'KP-thesis'.<sup>2</sup>

This proposal came to have an enormous impact on the philosophical community. Within a few years the externalist account of natural kind terms had become orthodoxy. This is how things still stand. Although a variety of semantic theories, unavailable in the 1970's, are now on offer, and although various problematic implications of externalism have been hotly debated, the KP-thesis stands largely unchallenged in the philosophy of language.<sup>3</sup> The metaphysics of natural kinds on which Kripke and Putnam relied is, by contrast, far from unchallenged. Many philosophers of science have argued that micro-essentialism is deeply flawed, not just for biological but also for chemical kinds. But although some philosophers of language acknowledge 'limitations' of micro-essentialism, many still assume its basic (if limited) viability: the full impact of recent discussions of natural kinds in philosophy of science has yet to be fully absorbed. So we have a strange situation, where philosophers of

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<sup>2</sup> In this paper our concern is not with exegesis of Putnam or Kripke. We shall ignore certain differences between their accounts of natural kind terms (but see Hacking [2010]).

<sup>3</sup> Even two-dimensionalist theories that combine externalism with descriptivism, such as (Chalmers [1996]) and (Jackson [1998]), depend on the KP-thesis in their account of secondary intensions.

language take the KP-thesis to be beyond dispute, even though it depends on a theory of natural kinds that is widely rejected among philosophers of science.

Here, we argue that the correct moral is that the KP-thesis should be abandoned. We argue that in the case of natural kind terms semantic claims are more invested in metaphysical assumptions than is commonly granted (section 2); that the problems with micro-essentialism run so deep as to undermine even lingering support for the KP-thesis and that recent appeals to placeholder essentialism do not help (section 3); that a presently more popular account of natural kinds – Boyd’s HPC theory – faces related problems and that, as a result, it cannot be used to support the KP-thesis either (section 4). In section 5, we consider what the proper lesson is for semantics. We argue that semanticists face a choice. One option is to insist on some version of the KP-thesis at the price of admitting that many presumed kind terms have no extensions at all. We reject this as implausible, on reasonable assumptions of what the task of semantics is. Another option is to adapt the semantics in light of a scientifically reasonable metaphysics. We endorse this option, and argue that it entails resurrection of some version of descriptivism. The details of this cannot be spelled out within the space of this paper, but we suggest that the semantic theory that fits best with the metaphysics would be a sophisticated version of the cluster theory.

## **2. From Semantics to Metaphysics**

Why then does metaphysics matter to the semantics here? Let’s consider the precise connection between the KP-thesis and the anti-descriptivist arguments in Putnam (chiefly the Twin Earth case) and in Kripke (a variety of modal and epistemic arguments). The upshot of these arguments is that associated descriptions do not determine the reference of proper

names and do not determine the extensions of natural kind terms – not even a cluster of such description will do, according to Kripke and Putnam. What, then, determines reference and extension? To this question there is a trivial and a non-trivial answer, depending on whether the determination relation is understood semantically or meta-semantically.

In the semantic sense, the answer is that the meaning of a proper name determines its reference, and the meaning of a natural kind term determines its extension. ‘Determination’ here is simply understood in the mathematical sense of a function. The claim that meaning determines extension in this sense is close to trivial on any account, but in the Kripkean context obviously so, since the meaning of a proper name is said to be nothing over and above its reference, and the meaning of a natural kind term simply its extension across possible worlds, i.e. its intension.<sup>4</sup> More interesting is the *metasemantic* question where ‘determination’ is understood as a metaphysical, ‘in virtue of’-relation: In virtue of what do proper names and natural kind terms have the meanings that they do?

In the case of names, Kripke’s reply is, in familiar outline, as follows. It assumes an initial ‘baptism event’, serving to fix the name onto an object through an act of ostension. Although the speaker may have all sorts of descriptions associated with the object, and although these descriptions may help the speaker to home in on the object, they do not determine reference across possible worlds – that is done by the object itself. Consequently, associated descriptions do not go into the semantics of the term: they are all merely contingently true of the object, and the relevant modality is *de re*. When we think of what is possible and

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<sup>4</sup> In Putnam’s case, it is less trivial since he does not take meaning to be exhausted by extensions. However, he argues explicitly that we cannot give up the assumption that meaning determines extension in the case of natural kind terms (Putnam [1975], pp. 245-55).

necessary about a person (or object) we are thinking about this independently of how she (it) is described.<sup>5</sup>

Kripke then extends this picture to the case of natural kind terms. Just as a name is fixed onto an object, so a natural kind term is fixed onto a natural kind through a baptism event involving a set of samples. A scientifically ignorant speaker can do this through ostension, pointing to a set of items while intending the term to pick out the underlying, essential kind ‘instantiated by most or all of those items’ (Kripke [1980], pp. 135-6).<sup>6</sup> As a result, the actual underlying features of the samples determine the term’s extension across possible worlds, as claimed by the KP-thesis. The term’s modal profile is determined by the nature of the samples picked out, not by how they are described. In considering whether it is possible that tigers could have been white, for example, we can just stipulate a possible world where members of this kind are white. Kripke claims that, by extension, it is perfectly possible that tigers lacked all the properties we normally attribute to them, such as being tawny yellow, striped, fourlegged, and so on; conversely, something may have all these features and not be a tiger; inspection of its ‘internal structure’ reveals that it differs from tigers so much as to not belong to that species but rather to some sort of ‘very peculiar looking reptiles’ (Kripke [1980], p. 120).

The KP-thesis is thus brought in to underpin the negative arguments against the cluster theory. That is, the semantics depends on the metasemantics which, in turn, depends on

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<sup>5</sup> In the case of both proper names and natural kind terms the meta-semantic account is combined with the familiar story of ‘reference borrowing’: Speakers downstream from the baptism event use the name with the same reference/extension if they stand in an appropriate causal-historical chain to the initial baptism. Since reference/extension borrowing is parasitic on reference/extension fixing, our primary concern shall be with the latter.

<sup>6</sup> As stressed below, the ignorance condition is central since the extension fixing story is supposed to work prior to the development of modern science (Putnam mentions the year 1750).

assumptions about the nature of natural kinds. Unless there are underlying features of the required sort, serving to determine extensions across possible worlds, we would be right back with some version of descriptivism (or no extension would be determined in the first place).

This is where the metaphysics comes to matter for the semantics.

The central role of the metasemantic thesis has been stressed in the more recent literature.

This is partly a result of Soames's ([2002]) influential discussion concerning Kripke's claim that proper names and natural kind terms are semantically similar. *Prima facie*, the suggested kinship is surprising: natural kind terms are not typically used as singular terms, but as

predicates, and it is far from clear what it might mean for a predicate to be a rigid designator.

Soames mentions three adequacy conditions for a notion of rigidity for predicates, but argues that no account of rigidity for natural kind terms satisfies these ([2002], p. 263). However, he

does not conclude from this that Kripke's account is in trouble. Rather, he concludes that the interesting semantic feature that natural kind terms share with proper names is not rigidity,

but the account of 'reference fixing'. This, he argues, is the real source of the non-

descriptiveness of both proper names and natural kind terms and of the corresponding modal claims.<sup>7</sup> Unlike Soames, however, we think that Kripke's meta-semantic account of reference-

fixing fails for natural kind terms, precisely because of naïve or mistaken assumptions about natural kinds.

In the case of names, Kripke's meta-semantic story seems relatively straightforward, albeit not entirely trivial since even with a rigid name, the question could be raised precisely what

'that very object' is across possible worlds.<sup>8</sup> But in the case of kind terms, we are supposed to move from a set of samples to a kind. Here, this step is bolder. The problem is familiar: an

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<sup>7</sup> Similar ideas can be found in (Haukioja [2012]) and (LaPorte [2004]).

<sup>8</sup> For a good discussion of this, see (Sidelle [1992]).

individual object instantiates any number of kinds, including any number of natural kinds.

How, then, can pointing to the samples serve to fix the term to the relevant kind? This familiar problem is often called ‘the qua problem’ (Devitt and Sterelny [1987]). Though much discussed in the literature, the qua problem has not been taken sufficiently seriously.<sup>9</sup> Doing that involves taking a harder look at the metaphysics.

In response to the qua problem, Devitt and Sterelny emphasize the psychological element in reference-fixing. They suggest that something about the mental state of the speaker helps determine what is the relevant nature of the sample. The speaker associates with the term in question both descriptions that classify the term as a natural kind term (as opposed to, say, an artifact term) and descriptions that ‘determine which nature of the sample is relevant to the reference of the term’ (Devitt and Sterelny [1987], p. 92).<sup>10</sup> Yet, they suggest, the psychological component cannot be so strengthened as to require sophisticated scientific knowledge, and must not lead to a lapse back into the cluster theory. Their hope is that the metaphysical component can pick up the slack: the presence of underlying, essential features to latch on to (such as microstructural properties) ensuring that the associated descriptions do not do any extension-determining work.

This is still the favoured strategy used by contemporary externalists to overcome various metaphysical worries associated with the Kripke-Putnam account. The idea is that the relevant essentialism stems from speaker intentions. On this view, externalism has no need for any substantive metaphysical assumptions.

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<sup>9</sup> (Stanford and Kitcher [2000]) is an important exception.

<sup>10</sup> Similar ideas can be found in Jessica Brown’s discussion of the qua-problem. In particular, Brown argues, the speaker needs to appreciate that “whether an item is of some particular natural kind is determined by its fundamental properties” (Brown [1998], pp. 286-7). This stress on speaker intentions may arguably be found already in (Putnam [1975]).

We shall argue that this strategy fails. It is of course an empirical question whether (and to which degree) speakers have essentialist intentions – incidentally, this seems far from settled.<sup>11</sup> For the purposes of this paper however, we shall assume it to be true. Even so, the KP-thesis fails. To illustrate this, we will start by examining the metaphysics behind Putnam’s and Kripke’s original arguments: microessentialism.

### **3. Metaphysics part I: The Demise of Microessentialism**

On microessentialism, members or samples of natural kinds are unified by sharing a common microstructure which (i) explains their macroscopic properties, (ii) holds universally for those members, and (iii) is necessary throughout modal space – hence ‘essential’, in the classical modalist sense.

This comports with the KP-thesis. Devitt and Sterelny are representative:

In the paradigm cases, these [natural kind] terms are introduced into the language by perceptual contact with samples of the kind. The extension of the term is then all those objects, or all that stuff, having an internal structure of the same sort as the ostensively given samples. (Devitt and Sterelny [1987], p. 70)

The KP-thesis requires ‘underlying features’ which locate introductory samples within specific extensions. The qua problem was that samples themselves underdetermine (typically vastly) any specific extension. This is where adherents of the KP-thesis naturally turn to microstructural essences. Their job is to determine and unite specific extensions. But what are

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<sup>11</sup> See (Häggqvist and Wikforss [2015]).

they? In reply to this question, the KP-theorist may either offer specific candidates, or appeal to placeholder essences: whatever fulfills a certain role and meets certain demands (including the demand that it be recognizably a structure, but not including the demand that it be known). We will consider these in order.

### 3.1 Original microessentialism

Kripke suggested that water is essentially  $H_2O$ . Putnam suggested that since we have discovered that water is actually  $H_2O$ , '*nothing counts as a possible world in which water isn't  $H_2O$* ' (Putnam [1975], p. 233; italics in original), and that being  $H_2O$  provides the relevant same-liquid-relation determining extension of 'water' at other possible worlds. To many, Kripke and Putnam here appeared to offer a specific structure as relevant. But ' $H_2O$ ' is not a description of microstructure; it describes chemical composition, which here means just molar proportions.<sup>12</sup> Structural isomerism entails that different substances may share a single composition. A simple inorganic instance is  $C_3H_8O$ , which may be propanol, isopropanol, or methoxyethane (Leary [unpublished] pp. 4-5); for big organic molecules, the number of isomers of a single compound may run in the millions (Smith [2011]; Leslie [2013], p. 144). Composition just won't cut it.

Perhaps, then, same-making essences should be sought at the level at which structural isomers are distinguished? After all, water has no structural isomers, and essentialists often invoke 'molecular structure'.<sup>13</sup> This idea quickly runs into difficulties. Ice and steam are water, but

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<sup>12</sup> This has often been pointed out (van Brakel [1986]; Needham [2011]). Yet it is no mere gaffe.

<sup>13</sup> Water does, however, have many different isotopic isomers. Those composed of deuterium and tritium are different not just at an atomic level, but in readily observable properties (LaPorte [2004]; Weisberg [2005]). This raises the issue in what sense it is even true to say that water is  $H_2O$ . The point – stressed by Weisberg [2005] – is not that vernacular usage of 'water' comprises impure samples (as discussed e.g. by Brown [1998]), or is less precise than

they are not in general composed of molecules of H<sub>2</sub>O: ‘Water [...] is not molecular, except under certain conditions in the gas phase’ (Needham [2011], p. 16). Liquid water at any moment consists partly of H<sup>+</sup> and OH<sup>-</sup> ions, into which H-O-H molecules constantly dissociate, as well as of rapidly lengthening and shortening polymers of H-O-H molecules, ions, and combinations of these. Polymerization and ionization rates vary with temperature and pressure; hence, on any reasonable notion of ‘structure’ at the molecular level, the structure of liquid water varies with temperature and pressure. Surely it is best, if one wants to think of structure at this level, to avoid the singular definite altogether and grant that water has lots of different structures, many of them immensely complex.<sup>14</sup>

In view of water’s variability at the microstructural level, one may ask why chemistry sometimes counts water as a single substance at all. (It doesn’t always – see Weisberg [2005].) This question is broached in a number of papers by Paul Needham ([2000], [2002], [2011]). Its answer is instructively remote from both ‘H<sub>2</sub>O’ and any notion of molecular structure.

One criterion for individuating substances is based on Gibbs’s phase rule; this yields a value for the number of independent substances in a closed system and counts water as a single

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scientists’. It is that the isotopic isomers of H<sub>2</sub>O are clearly distinct substances both from the chemist’s and vernacular speaker’s points of view. Since, for instance, pure heavy water is both lethal to drink and different in its macroscopic properties from naturally occurring samples of water, the option of treating H<sub>2</sub>O simply as a generic kind encompassing every isomeric species (as considered in Tahko [2015]) seems unpromising: this would include stuff with very different properties from those considered stereotypical (in Putnam’s account) or reference-fixing (in Kripke’s). On the other hand, as Weisberg says, it would also be odd to count all amounts of isomers other than H<sub>2</sub><sup>16</sup>O as impurities in natural samples.

<sup>14</sup> Chang [2012] recommends naturalists to ‘follow the latest and best science as judged by scientists themselves, not some tired old approximation like “water is H<sub>2</sub>O”, which scientists who research on water have moved beyond a long time ago’. We concur, and would urge semanticists to heed Chang’s advice to naturalists: ‘it will not do to follow Putnam’s notion (1975, 224), in itself 40 years old and already outdated even then, that the extension of “water” is “the set of all wholes composed of H<sub>2</sub>O molecules”’ (Chang [2012], p. 248).

substance. Another is based on the entropy of mixing: if two separated samples are united (say, by lifting a shield in a closed container) and no increase in entropy results from their mixing, the samples are of the same substance. By this test, too, water will usually emerge as a single substance. But water has two spin isomers – parawater and orthowater (according to whether the nuclear spin of the hydrogen atoms in polymeric H-O-H links is opposed or aligned) – normally occurring in a 1:3 ratio. Though difficult, separation of ortho- and parawater has been achieved, and by the entropy of mixing test, they count as distinct substances (Leslie [2013], p. 149).

Now the entropy of mixing test is obviously very fine-grained. And the effects of isotopic variation are marked in the case of hydrogen compounds to an extent unique for them – in Hendry’s words, hydrogen is ‘a monster, not a paradigm’ (Hendry [2006], p. 868). So it might be urged that we simply dismiss criteria cutting so finely as to distinguish different isotopic and spin isomers. This is, in effect, Hendry’s suggestion. He offers a micro-structuralist criterion for elements based on nuclear charge (i.e. atomic number); for compounds, he grants that the similarity relations uniting a single structural isomer and distinguishing it from others ‘will need to be assessed case by case’ (Hendry [2006], p. 873).<sup>15</sup> His proposal may thus seem to hold some hope for a scientifically informed defense of microessentialism, and, by extension, the KP-thesis.

It doesn’t. Note, first, that Hendry’s criterion is only intended to work for chemical substances, and will not work generally for other items on Putnam’s and Kripke’s lists of natural kinds. Hendry’s criterion is avowedly ‘discipline-immanent’: motivated by the

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<sup>15</sup> Hendry also connects his proposal with the externalist’s claim that speaker intentions prior to scientific discovery warrant a construal of such discovery as finding out what the extension of a kind term was prior to discovery: ‘Lavoisier’s intentions uniquely pick out sameness of nuclear charge as the important similarity relation for elements’ (Hendry [2006], p. 869).

explanatory and taxonomic interests of chemists themselves. Second (and connectedly), it is, reasonably enough, modally unambitious and not intended to hold for every metaphysically possible world. Thus it does not seem apt for determining extensions at all worlds, as required by the KP-thesis. Third, the proposal takes care neither of those chemical kinds that resist characterization in microstructural terms (for instance, acids and bases) nor of those that actually depend on more finegrained criteria. In certain chemical contexts, these distinctions – between allotropes of elements, spin isomers of elements and compounds, and structural isomers – are made, after all. And these more finegrained differences are clearly every bit as objective as those indicated by the slots of Mendeleev’s table.

### 3.2 Placeholder essentialism

But perhaps discussion of actually proposed candidates for microessences, and of their shortcomings, misses the point. Perhaps the role of being an essential microstructure may fall where it may, as science progresses? This seems to be a common attitude among adherents of the KP-thesis, who, with some plausibility, regard ‘H<sub>2</sub>O’ as a mere placeholder in Putnam’s argument – just a bit less explicitly so than ‘XYZ’. After all, Putnam did warn that ‘whether something is or is not the same liquid as *this* [sample] may take an indefinite amount of scientific investigation to determine’ ([1975], p. 225; italics in original).

However, even granting that facets of the detailed structure of water may yet await discovery, the microessentialist’s problem is not that the essence-revealing science isn’t in yet. Her problem is rather that we know too much: there are way too many objective distinctions and sameness relations for any one of them to stand out as uniquely salient. Moreover, science doesn’t issue essence claims. While scientists may agree, at a certain time, to privilege some

individuation criterion, they are usually too aware both of the degree of decision involved and of the negotiability of such criteria to portray them as metaphysically necessary. Scientists may also disagree over such criteria for decades, without apparent damage to their communication or the progress of their research.<sup>16</sup>

For illustration, consider two recent proposals exploiting the placeholder idea. Haukioja ([2012], [2015]) suggests that the distinctive feature of natural kind terms is what he calls their actuality-dependence. A term is actuality-dependent iff its extension in non-actual possible worlds is determined by ‘some feature of the actual extension’ which realizes what Haukioja calls its ‘applicability role’: whatever criteria determine what the term applies to in the actual world (Haukioja [2012], p. 406). By contrast, an actuality-independent term’s extension at any world is determined simply by its applicability role. Whether a term is actuality-dependent or not depends only on how speakers are disposed to apply and interpret it (Haukioja [2015], pp. 2147-49). With natural kind terms such as ‘water’, he suggests, we are guided not by explicit intentions but ‘by a tacit assumption about underlying essences’ ([2015], p. 2149). Speakers need have no idea what the realizing property (essence) is: ‘the kinds of essentialist intuitions we have [...] are remarkably flexible’ ([2015], p. 2150 fn. 9); our dispositions just manifest ‘a preference for some explanatory unity’ ([2015], p. 2150 fn. 10). But in the case of ‘water’, Haukioja continues, we have made the empirical discovery that its actual applicability role realizer is ‘the chemical structure H<sub>2</sub>O’ ([2015], p. 2150). More generally, a realizing property is ‘an underlying property which is actually causally responsible for the observable characteristics of the stuff in the actual extension’ of the term ([2015], p. 2149).

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<sup>16</sup> For some historical cases, see (Cowie [2009]); for a contemporary illustration, see the resistance to Machery’s eliminativism about concepts evinced in the peer commentary on (Machery [2010]).

Similarly, Law ([2015]) suggests that the same-substance relation across worlds may be determined by whatever microstructural constitution is shared by most of a number of samples, different in a number of foils, and ‘plays a significant role in causally determining most of the’ observable properties by which samples are grouped (Law [2015], p. 8). He also suggests that although it is unlikely that this relation is due to explicit speaker stipulations, it ‘more or less accurately captures our linguistic intentions regarding “same substance”’ ([2015], p. 8). To emphasize the element of stipulation involved, however, he proposes that substance terms work largely in parallel to a toy example where sameness of ‘woozles’ is stipulated to come to sameness of whatever (under a magician’s spell) innards of certain boxes determine their colour (Law [2015], pp. 5-7). In the toy example, box colour is eventually found to depend on composition of coloured marbles; for water, Law takes science to have vindicated Putnam’s claim ‘that something is water if and only if it has the chemical structure  $H_2O$ ’ ([2015], p. 16).

Law and Haukioja are primarily concerned to rebut Salmon’s ([1982]) claim that a posteriori necessities follow only if semantic externalism is conjoined with a substantive metaphysical premise. As indicated, both also place great weight on speaker intentions or dispositions (respectively). And both stress the openness of such intentions and dispositions with respect to what may count as an essence: all that is required is ‘that the actual world cooperates ... sufficiently to make some theoretical identification statement, such as [that water is  $H_2O$ ] true’ (Haukioja [2015], p. 2150). It thus seems reasonable to read both as expressing placeholder essentialism conjoined with the claim that science has now substituted ‘the structure  $H_2O$ ’ in the slot left open by speaker intentions (Law) or dispositions (Haukioja).

If the latter claim is taken as an indication of how these authors envisage placeholder essences, however, we are just back where we started in this section: ‘H<sub>2</sub>O’ doesn’t designate any structure, and fails as a unity-conferring causal explanans of water. More generally, a drawback of Law’s and Haukioja’s proposals is, as indicated above, that the world often doesn’t meet even flexible essentialist expectations. To emphasize this, it is worth looking at some details of Law’s proposal. In his toy case, the operative parts are, first, speakers’ belief in the magician’s assertion that box (‘woozle’) colour causally depends on box innards except for a few ‘rogue boxes’ (Law [2015], p. 5); second, the grouping of boxes into sample groups by colour; and third, a stipulative definition of ‘same sort of wozle’ according to which two boxes, at the same or different worlds, stand in this relation iff they contain ‘the same variety of *dthat* (the inner combination of *these* [sample] boxes that is (i) the same in most of the same-coloured boxes, (ii) different in most of the differently-coloured boxes, and (iii) usually determines the box’s outward colour)’ (Law [2015], p. 6; italics in original).

For substances, Law suggests that a parallel definition may run as follows. Pointing to ‘a large number of samples’, we stipulate that

(SS) a sample *x* in possible world *w*<sub>1</sub> is a sample of the *same sort of substance* as sample *y* in possible world *w*<sub>2</sub> iff (for each of the below-mentioned levels of microstructural constitution that they share) *x* in *w*<sub>1</sub> possesses the same varieties of *dthat* (those levels of microstructural constitution of *these* samples that, where present in a given sample group, (i) are the same in most of the same-grouped samples, (ii) differ in most differently grouped samples, and (iii) play a significant role in causally determining most of the P properties of the the various samples) as does *y* in *w*<sub>2</sub>. (Law [2015], p. 8; italics in original)

‘P properties’ are those observational properties by which samples are grouped (Law [2015], p. 7). The ‘below-mentioned levels of micro-structural constitution’ are atomic number and molecular structure ([2015], p. 8) – speakers are not assumed to know this, of course, but they are assumed to assume that there is one or more such underlying levels of constitution which determine(s) P-properties ([2015], p. 7). From the context, it seems clear that Law intends ‘level of constitution’ to mean ‘(some determinate) constitution (at a certain level)’. Hence, what scientifically ignorant speakers are held to assume is – in parallel with the toy example – that there exists one specific unifying micro-constitution at each applicable level, which is shared by most same-grouped samples and which stipulatively determines the substance term’s extension across worlds.

But this just brings us back to the difficulty that water, for instance, is not a structural isomer, and so is unlikely to pass the (SS) test for sameness of substance. Law specifically addresses some of Needham’s objections to microessentialism. He suggests that samples are ‘likely to include multiple phases’ and that ‘condition (i) of (SS) will rule out phase-specific microstructural features’ ([2015], p. 15). But then what is left? Any structure at the levels where structure explains anything specific about water is phase-specific. There is no microstructural constitution common to steam, ice and liquid water, except at the level (useless for ‘water’-defining purposes) introduced by Law to allow for elements, namely proton numbers of the atoms involved.<sup>17</sup>

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<sup>17</sup> It is also worth recalling from our earlier discussion that not all liquid water shares the same (dynamic and extremely complex) structure. Incidentally, the suggestion that samples include different phases is not merely quite demanding, but also makes Law’s list of candidate P-properties puzzling: these includes ‘clarity, liquidity, thirst-quenching character’ ([2015], p. 7).

Law further suggests that (SS) explains why allotropes of the same element – in his example, oxygen – are counted as distinct substances by chemists, since it demands unity ‘at the level of both the atomic numbers of constituent atoms *and* (where present) molecular structure’ ([2015], p. 16; Law’s italics). But this demand also appears to rule out oxygen’s being a single substance, since natural samples of it tend to be molecular. In general, the expectation that there be a single unitary structural constitution underlying observable commonalities – however strongly held under conditions of ignorance, and however strongly demanded by one’s stipulations – need not be met by the world. (Even in the toy example, where Law has stacked the odds by stipulating that only a few boxes are ‘rogue’ and by assuming the magician’s reliability, the information speakers have before introducing the term ‘woozle’ is consistent with same-coloured boxes having their colour determined by very different contents.)

Let us turn briefly to biology in connection with placeholder essentialism. Even among essentialists, it is widely accepted that biological taxa do not comport with microessentialist assumptions. One exception is Devitt [2008]. He argues that since we have generalizations about species such as ‘African rhinos have two horns while Indian rhinos have one’, and since there has to be a proximal explanation why each rhino conforms to this generalization that partly appeals to intrinsic properties of rhinos, we can, indeed must, appeal to inner species-specific essences:

Some intrinsic underlying property of each Indian rhino causes it, in its environment, to grow just one horn. A different such property of each African rhino causes it, in its environment, to grow two horns. The intrinsic difference explains the physiological difference. If we put together each intrinsic underlying property that similarly explains a similar generalization about a species, then we have the intrinsic part of its essence (Devitt [2008], p. 352).

The crucial assumption operating here is that expressed by the quantifier order in the first sentence: that there is *an* intrinsic property common to all Indian rhinos which (contrastively but causally) explains why they grow one horn. This may be a tempting assumption. But, as Slater asks, why make it? (Slater [2015], p. 379) Nothing guarantees that the explanation be the same for each instance of the generalization.<sup>18</sup> For biological taxa, not even a sophisticated (and hence forcibly attenuated) placeholder version seems to work.<sup>19</sup>

At this point, a pattern should have begun to emerge. Semanticists assume introducers of a term to have not even a dim idea of what exactly underlies the common properties by which samples are grouped. Nevertheless, they grant them not just the assumption that there is something underlying (causing, explaining) these properties, but also the assumption that there is *some* unity at some underlying level, and that however unknown the underlying unity is, it may be caught by sufficiently clever stipulation. The problem is that not even very clever stipulation can impose such orderliness on the world, and so the world may fail to meet these assumptions.

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<sup>18</sup> Conversely, of course, insofar as the species generalization holds and Devitt's restriction to intrinsic explanantia is heeded, we may be fairly sure that for each Indian rhino, there is some intrinsic property that contrastively explains its one-hornedness; etc. Leslie ([2013], pp. 134-8) offers a useful catalogue of counterexamples to the assumption that a common phenotypic trait must have the same intrinsic explanation in each instance, even within a single species. Some of her examples turn on canalization of traits within a species, others on phenocopying across species. We submit that they succeed in rebutting Devitt.

<sup>19</sup> Note that within biology, there are seemingly good candidates, apart from the taxa, for natural kinds: '[p]redator may be one; sexuality may be another' (Sober [1984], p. 335). These kinds look immediately unfit for microessentialist unification. So do elementary particles (Lange [2011]; Slater [2015]), and natural phenomena such as light (Gray [2006]). Gray argues persuasively that, contrary to what Kripke ([1980]) suggested, even in the mouth of scientists, the extension of 'light' is determined by the effects on humans of certain electromagnetic radiation. To be fair to Kripke, in Lecture II of *Naming and Necessity*, he does suggest that light is to be identified with 'electromagnetic radiation between certain limits of wavelength' ([1980], p. 98), but in Lecture III the qualification (whose rationale, from an essentialist perspective, seems doubtful anyway) is dropped.

In conclusion, microessentialism fails as a metaphysics for natural kinds. This holds whether it is framed in terms of articulated candidate microstructures, or in a placeholder version.

#### **4. Metaphysics Part II: HPC Theory**

Outside of philosophy of language and some factions of metaphysics, many philosophers now embrace Boyd's Homeostatic Property Cluster Theory (henceforth, 'HPC theory'). HPC theory avoids some of the faults of microessentialism, but (we shall argue) not all of them. As a result, although HPC theory might in principle be invoked by externalists, it too fails to vindicate the KP-thesis. Additionally, as we'll note in passing, HPC theory as a general metaphysics for natural kinds faces grave problems of its own.<sup>20</sup>

Members of a natural kind not only share many properties, but share many properties yet to be discovered (as stressed by Mill [1843]). As emphasized by its 'PC' part, HPC theory holds such clustering of properties to be a necessary condition for natural kind-hood. But it also requires that the clustering result from an underlying mechanism (Boyd [1989], [1991], [1999]). The clustering may thus be 'metaphorically (sometimes literally) described as a sort of homeostasis' (Boyd [1999], p. 143).<sup>21</sup> In addition, HPC theory requires that in order to count as a natural kind, a property cluster must figure in important causal claims (especially generalizations); moreover, a kind mustn't assimilate causally relevant distinctions, nor make

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<sup>20</sup> For good overviews of HPC theory's problems as well as novel criticisms, see Slater (2015) and Ereshefsky and Reydon (2015).

<sup>21</sup> The emphasis on underlying mechanisms in (Boyd [1991], p. 141) is weaker in (Boyd [1999], p. 143): 'Either the presence of some of the properties [in the cluster] tends (under appropriate conditions) to favor the presence of others, or there are underlying mechanisms or processes which tend to maintain the presence of the properties in [the cluster], or both.'

causally irrelevant ones – the former would warrant what Craver [2009], using Darwin’s terms, calls ‘splitting’; the latter, ‘lumping’.<sup>22</sup>

While more liberal than micro-essentialism, HPC theory retains two of its crucial tenets: first, that natural kinds are united by something (causally) explanatory; second, that this explanans is located at a stratum or level indicated by ‘underlying’. Now what is causally or explanatorily prior need not, perhaps, be smaller than what it explains, intrinsic to it or concern its inner constitution.<sup>23</sup> But to many, Boyd’s phrasing has suggested that the explanatory direction is the same as that assumed by microessentialism, although the connection is looser.<sup>24</sup>

Given the affinities, it is not surprising that some writers have proposed assimilation. On the one hand, insofar as chemical substances or elements exhibit perfect homeostasis due to shared microstructures, they might be viewed as a special case of HPC kinds (Bird [2007]). On the other hand, mechanisms – even extrinsic ones – might be regarded as real essences: ‘The concept of causal homeostasis entails a very broad conception of the essence of a category. An essence can be any theoretical structure that accounts for the projectibility of a category.’ (Griffiths [1997], p. 188). There is no obvious need to choose between (or complain about) these ways of talking.

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<sup>22</sup> The last two conditions are often taken to be the core of Boyd’s so-called ‘accommodation demand’: the demand that the categories reckoned with by science be accommodated to the ‘structure of the world’ (Boyd [1989], p. 22). They are culled from the 11-clause list characterizing HPC kinds in (Boyd [1989], pp. 16-17). As Martínez ([2013], p. 443 fn. 11) notes, it ‘is unlikely that all of them are intended by Richard Boyd as necessary conditions’; unlike Martínez, however, we think that clauses (iv) and (x), from which the last two demands stem, are so intended.

<sup>23</sup> As noted by Griffiths ([1997], [1999]).

<sup>24</sup> For instance, Putnam, here offering a close-to-canonical statement of HPC theory *avant la lettre*: ‘*natural kinds* [are] classes of things that we regard as of explanatory importance; classes of things whose normal distinguishing characteristics are “held together” or even explained by deep-lying mechanisms’ (Putnam [1970], p. 187; italics in original).

Perhaps then, HPC theory might substitute for microessentialism as a grounding metaphysics for semantic externalists embracing the KP-thesis. This would require, first, that ‘essence’ be taken in its new, liberal sense to encompass clustering mechanisms; second, that these mechanisms be considered same-makers across worlds. Consider the term ‘tiger’ and the corresponding kind.<sup>25</sup> Matthen ([2013], p. 139) writes: ‘in the case of a natural kind terms such as “tiger” [...] understanding the term consists of (a) knowing (by acquaintance or by report) some tigers, and (b) knowing that these tigers belong to an HPC class’. Now as Matthen immediately notes, ‘the hypothesis of homeostasis could be wrong’ (in a given case). The assumption that there is an underlying mechanism responsible for the putative homeostasis could be wrong, too. Here the situation appears parallel to the microessentialist externalist’s fallibly assuming that there is a common micro-essence uniting the extension of a kind term.

But of course, just what the HPC externalist assumes here depends on what counts as a homeostatic mechanism. To continue the parallel, a meta-semantic theorist appealing to mechanisms may here either offer a general notion or invoke something antecedently identified by science as a mechanism.

Let’s consider the first option. It may here seem that the HPC theorist (unlike the microessentialist) has a plausible general notion within reach. Summarizing threads from the rapidly growing literature on mechanistic explanation, Craver suggests that a consensus view is that in general, mechanisms are ‘entities and activities, organized together such that they do

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<sup>25</sup> For present purposes it doesn’t matter whether we think of tigers as a species or some other taxon.

something' (Craver [2009], p. 582).<sup>26</sup> This is of course drastically generic and somewhat uninformative. But perhaps a very generic notion of mechanism is just what a liberal natural kind theory needs.<sup>27</sup> Moreover, it is obvious – though still very generic – what the 'something done' is here: it is the clustering of properties. Most HPC theorists are willing to accept the consequence that natural kinds, as a metaphysical category, will be highly heterogeneous.

More worryingly, even this very generic notion seems not to fit all instances actually invoked by HPC theorists. The phrase 'such that they *do* something' appear to require mechanisms to have causal agency, as does the motivating idea that mechanisms causally explain property clustering. But consider the popular idea that natural selection is the homeostatic mechanism uniting taxa. It is extremely controversial whether selection, even partitioned into distinctive selective regimes or selection pressures, is a cause at all (let alone of individual traits).<sup>28</sup> As Slater emphasizes: 'Denying a causal interpretation of natural selection requires (on any reasonable account of mechanism) denying that natural selection should be construed as a "mechanism".' ([2015], p. 392).

More worrying still are the fundamental questions pressed in particular in (Craver [2009]). What counts as one mechanism and what as distinct ones? When are two token mechanisms instances of the same mechanism type? These are blatantly questions about kinds: kinds of mechanisms. If mechanisms are carved into kinds by appeal to HPC theory, regress threatens.

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<sup>26</sup> Here Craver refers to (Bechtel and Abrahamsen [2005]), (Bechtel and Richardson [1993]), (Craver [2007]), (Darden [2006]), (Glennan [1996], [2002]), and to (Machamer *et al.* [2000]).

<sup>27</sup> Hacking toys with the thesis that 'there are exactly two natural kinds in the universe, *boson* and *fermion*' ([2007], p. 205; his emphases). HPC theorists are typically at the opposite pole from such austerity.

<sup>28</sup> See (Matthen and Ariew [2002]) and (Walsh *et al.* [2002]). For the debate over whether selection causes individual traits, see (Neander [1995]), (Sober [1995]) and (Walsh [1998]). For a general argument against the mechanistic credentials of the explanatory factors behind species homeostasis, see (Slater [2015], pp. 391-2).

One could attempt an answer in terms of sameness of constituent entities and activities. But this is still about kinds: if these kinds are themselves construed along the lines of HPC theory, regress looms again. (Craver [2009], pp. 586-7). It might be hoped that such a regress peters out eventually into kinds not requiring the HPC treatment, perhaps essentialist kinds. As Slater ([2015], p. 389) notes, such hope appears unsustainable in general. For reasons stated, we are also skeptical of essentialist kinds as even potential stopping blocks.

Consider now the other route, of counting as mechanisms only what are identified as such by various branches of science. One problem with this is that mechanisms underlying a set of clustering properties may be found at varying levels of abstraction, which in turn sanction differing extensions (Craver [2009], pp. 585-9). Another problem is how to distinguish different token mechanisms: even appeal to antecedently identified mechanisms will not always yield a satisfactory answer; and more general approaches will not cohere with scientific practices of individuation (Craver [2009], p. 590). A third sort of problem arises with the fact that both with the properties clustering and with the mechanisms explaining this, there are often no sharp boundaries warranting partitioning of phenomena into one kind rather than another, or issuing a verdict on whether two instances belong to the same kind or not – even when we know how the phenomena are caused. In biology and psychology, what Buckner ([2015]) calls ‘transitional gradation’ abounds: here,

nature does not provide us with any clear discontinuities that would recommend a particular metric of similarity for either kind (whether or not overlapping) as more or less natural than a host of other eligible alternatives. Metaphorically, we reach out to nature to feel for a joint and find only smooth continua. (Buckner [2015], p. 9)

These difficulties have prompted different reactions. Some writers apply the demand for a unitary underlying mechanism normatively, and argue from the (perceived) lack of such a mechanism to splitting or even eliminativism. Given the vagaries surrounding the notion of mechanism, this appears premature. Others insist on accepting as a natural kind whatever is united by an antecedently identified mechanism (at some right level). Magnus ([2011]) takes this line. But this seems seriously at odds with scientific practice (and, as Magnus notes, constitutes a departure from HPC theory as usually conceived). Still others suggest dropping the appeal to causal mechanisms, even liberally construed, altogether: Martínez ([2015]) proposes invoking only information-theoretic, probabilistic connections between properties as the basis for a graded notion of natural kind. This is clearly also a radical departure from Boyd's original idea. Recent refinements of HPC theory – offered largely in response to the problems catalogued above – thus seem to point in the direction of abandoning it.<sup>29</sup>

Craver ([2009]) suggests that a viable HPC theory must be construed pragmatically, since identification, individuation, and typing of pertinent mechanisms depend ineliminably on human interests. It may be held that a pragmatist seed was present at the outset: compare Boyd's demand that kinds be causally important – not every causal difference is important – and they not mask causally relevant, nor make causally irrelevant distinctions: these demands are, as Boyd recognized, interest-laden. But an avowedly pragmatist HPC theory is, of course, unlikely to warrant the KP-thesis, whose guiding idea is that once a sample (or set of samples) is fixed, the world accomplishes all the carving needed to determine extensions irrespective of human interests.<sup>30</sup>

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<sup>29</sup> Also see (Ereshfsky and Reydon [2015]).

<sup>30</sup> While acknowledging its pragmatic aspects makes HPC theory more plausible, it also tends to undermine the point of talk about 'underlying mechanisms'. If a category is recognized in a science because it exhibits close clustering of a set of properties (at various levels), it will be useful for that science regardless of whether there is a mechanistic (or other) explanation of

## 5. Prospects for Natural Kind Term Semantics

We have argued that extant theories of natural kinds fail to deliver ‘underlying, essential features’ of the sort required by the KP-thesis. The KP-theorist’s metaphysical assumption thus fails. Philosophers of language have taken for granted that it can be upheld, one way or the other, and do the work required by the KP-thesis. We propose, on the contrary, that it cannot be upheld and that this forces a choice. Semanticists may abandon the KP-thesis and admit that essentialist intentions fail to determine the semantics of natural kind terms. Or they may insist on the mechanism for extension determination encapsulated in the KP-thesis and accept the consequence that few, if any, natural kind terms have an extension.

We take the proper conclusion to be that the essentialist intuitions simply are mistaken and, correspondingly, that even if these intuitions show that speakers have the intention to pick out underlying essential properties, this does not determine the semantics.<sup>31</sup> That is, we take the proper conclusion to be that the KP-thesis should be abandoned and that the extensions of natural kind terms are not determined by underlying essential properties. Since, as argued above, the anti-descriptivist arguments depend on the KP-thesis, a further reasonable conclusion is that some version of descriptivism is true.

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why the clustering holds, much as we may want and seek such explanation. Hence a better alternative seems to be to drop the requirement of an underlying mechanism altogether, as argued in (Slater [2015]), and accept that natural kinds are just stable property clusters (as also suggested in Häggqvist [2005] and, perhaps, in Martínez [2015]). This would better accord with the practice of science.

<sup>31</sup> It is tempting to suspect with Leslie ([2013]) that the intuitive support for the Kripke/Putnam semantics results from a stubborn cognitive bias towards expecting essences where none exists: ‘The intuition that such essences *must* exist is no more than an expression of a belief set that is firmly in place by the middle of the preschool years, but this belief set does not comport with the complexities of actual science’ (Leslie [2013], p. 110; italics in original).

Which version? This question is in part empirical. That we question the decisive semantic role of essentialist intuitions does not mean that we deny that evidence from speaker use is relevant. The idea is this: instead of falling back on theoretically uninformed essentialist intuitions, we need to consider the overall function of these terms in ordinary classification, as well as how kinds are used and talked about in the sciences.<sup>32</sup> Sections 3 and 4 above reviewed some recent discussion in the literature on natural kinds, suggesting that micro-essentialism is discredited and that the HPC theory is less than satisfactory. The points raised in objection to these metaphysical accounts all seem to speak in favor of a property cluster account without much emphasis on underlying mechanisms as kind unifiers. Concordant with this, we propose that the most promising semantics for kind terms will be some sophisticated version of the cluster theory. If natural kinds are best understood as clusters of properties, then the extensions of natural kind terms are best understood as determined by clusters of descriptions.

It should be noted that although Kripke's main target was the cluster theory, many of his objections are in fact directed at definitionalist versions of descriptivism (such as the objection that gold need not be yellow), and can be handled by cluster theories. What a cluster theory cannot permit are the modal conclusions, in particular the claim that the totality of the associated descriptions may fail to hold of the kind in question. However, again, this seems a proper lesson of the metaphysical considerations above. As Slater notes, the tenability of the claim that we might be mistaken that a certain kind of stuff is correctly described by even a cluster of properties depends on the idea that we can 'keep our finger on the misrepresented

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<sup>32</sup> See (Häggqvist and Wikforss [2015]).

stuff in different circumstances (across possible worlds, and so on); in other words, there is some underlying essence of what it is to be stuff of this kind' ([2015], p. 378).

A sophisticated cluster theory would depart from standard versions of the theory in at least two respects: First, legitimate descriptions are not restricted to observable properties – the cluster theory in itself is not committed to ‘superficialism’.<sup>33</sup> Second, the theory would not commit to the idea that all descriptions are given equal weight. Although the number of properties satisfied is no doubt of importance in determining what a kind term picks out, it need not be the case that ‘most’ of the properties are present. For instance, we want to be able to say that people have had many mistaken beliefs about gold (think of alchemy), even if the number of mistaken beliefs were to outnumber the correct ones.<sup>34</sup> Of course, on such a semantics, there will no longer be a distinct semantic category of natural kind terms (see Wikforss [2010]). This harmonizes with the rejection of a sharp metaphysical demarcation between natural kinds and other kinds.

The only alternative option that we can see, for recalcitrant KP-theorists, is to persevere and insist that since speakers use these terms with the intention of picking out underlying essences, the semantics must reflect this. And they might add that Putnam and Kripke never claimed that a purported natural kind term actually succeeds in picking out a natural kind – there are well-known failures after all, such as ‘jade’.

The trouble is that the KP-thesis explicitly assumes that the metaphysics does matter to semantics. If, therefore, the semanticist insists that the essentialist intuitions of ordinary

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<sup>33</sup> See (Lewis [1994], p. 424).

<sup>34</sup> Indeed, the sheer number of properties cannot be what is decisive, since there may be an overlap in properties between two kinds such that most of the properties are shared. Compare (Ben-Yami [1999]).

speakers entail the KP-thesis, it follows that many natural kind terms will not have an extension determined at all, not even an empty one. On Kripke's view, at least, this would entail that they lack all semantic content.<sup>35</sup> However, with Soames ([2002], pp. 281-2), we take such an outcome to be unacceptable: clearly, many purported natural kind terms play a central role in our everyday practices and communication, as well as in science, and the suggestion that they must be construed as semantic failures is highly implausible.

In any case, as we suggested in section 3, there is little evidence that scientists do use these terms with essentialist intentions. If the recalcitrant KP-theorist admits this, but insists that lay speakers harbour such intentions, this would introduce a strange gap between ordinary usage and scientific usage. Although there may be differences between colloquial and scientists' uses of a term, these differences are best not construed in such a way that no extensions are determined for the colloquial uses. A gap of this sort would also seem contrary to the idea underlying the KP-thesis that it is up to science to determine what the relevant underlying features are.

In conclusion, the Kripke/Putnam account of natural kind terms, which has dominated philosophy of language since the 1970's, is untenable. Although Kripke and Putnam were quite right to criticize traditional, definitionalist accounts of natural kind terms, and although they were right to stress the close interaction between semantics, metaphysics and philosophy of science, the semantic paradigm that emerged, captured in the KP-thesis, cannot be

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<sup>35</sup> Some semantic theories combine the KP-thesis with descriptivism: see (Chalmers [1996]) and (Glüer and Pagin [2012]). Such theories partly evade the threat of failed extension determination. For instance, according to two-dimensionalism, the primary intension of the term is determined by the associated descriptions, rather than by underlying, essential features. However, since the secondary intension is supposed to track such features, the threat remains in full force that it will fail. In effect, then, the secondary intension will drop out; the remaining semantics will simply be a version of descriptivism.

sustained. Time is ripe to cut the cord with the legacy from Kripke and Putnam, and start afresh.

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