A POSTERIORI KNOWLEDGE OF NATURAL KIND ESSENCES: A DEFENCE

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Abstract

I defend this claim that some natural essences can be known (only) *a posteriori* against two philosophers who accept essentialism but who hold that essences are known *a priori*: Joseph LaPorte, who argues from the use of kind terms in science, and E. J. Lowe, who argues from general metaphysical and epistemological principles.

1 Introduction

Saul Kripke, Hilary Putnam, and others have argued for a pair of related claims (i) that we can have knowledge of the essences of natural kinds, and furthermore (ii) that this knowledge is often *a posteriori*. Although such an opinion has had widespread acceptance, there have nonetheless been numerous philosophers who have rejected these arguments. Typically their target has been Kripke–Putnam essentialism, (i), or the externalist, referential semantics that is often taken to be related to essentialism. Thus essentialism is often opposed by those who wish to defend Kuhnian claims concerning incommensurability. But more recently there have been objections to the second of the pair of claims by philosophers who accept the truth of (i). Such philosophers are essentialists, but regard all knowledge of essences as being *a priori*. In this paper I shall focus on two sets of objections to the claim that our knowledge of essences is often *a posteriori*, the first from Joseph LaPorte and the second from E. J. Lowe.

2 LaPorte: kind essences are stipulated

According to LaPorte (2004), theory change is frequently accompanied by conceptual change. Our earlier scientific concepts, natural kind concepts in particular, are vague and the relevant scientific discoveries reveal the possibility of alternative precisifications of the concept. How to modify the concept is a matter of choice. And thus, although there are Kripke-style necessities pertaining to natural kinds, the choice of a precisification of a kind concept in effect amounts to a stipulation that fixes the relevant necessities as true. LaPorte's argument can be encapsulated in the following claims:

> (a) our natural kind concepts are vague;
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> (b) theory change and other kinds of discovery precipitate precisifications of those concepts;
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> (c) that precisification is a matter of choice and not scientifically determined;
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> (d) the facts encapsulating the essences of those kinds are the outcomes of the process of precisification in (c).

Claims (c) and (d) together yield the result that essences are stipulated but not discovered. LaPorte does acknowledge that Kripke-style discoveries of essences could occur, but denies that they in fact do.

I shall argue principally that (d) is false, but I shall also cast some doubt on (c). As a consequence I do not believe that LaPorte's arguments motivate any very significant departure from Kripke at all, although those arguments are nonetheless important in that they require at least a more nuanced view of the Kripkean paradigm.

3 Essences of chemical kinds

LaPorte gives a range of interesting and informative examples. Most of these are in biology but some come from chemistry. I shall start with the latter. LaPorte starts with the example of jade and argues, contrary to the tradition following Putnam, that jade presents an example very similar to his Twin Earth thought experiment, but with a different outcome. For many centuries the jade that the Chinese worked with and called 'yü' was just nephrite. Only towards the end of the eighteenth century was jadeite from Burma introduced into China. Chinese jade experts were aware that it was a different material, but nonetheless decided also to regard it as 'yü'. LaPorte (2004: 94–100) argues that the terms 'jade' and 'yü' have a certain kind of vagueness, such that it was not determinate whether the new jade, jadeite, was within the extension of those terms. A decision was made to include the new material, thus introducing a *change* to the concepts 'jade' and 'yü', rather than an application of previous, unchanging concepts. Furthermore, that decision went in a *different* direction to the direction that is alleged in the Twin Earth story. There we are supposed to reject XYZ as water despite superficial similarity. With jade the chemically different but superficially similar jadeite was included, not rejected. If LaPorte's understanding of this case is right, then the extension of 'yü' is not fully determinate, but is vague. Given that for centuries the Chinese only ever knew nephrite, one might have expected, from Putnam's Twin Earth story, that they would reject jadeite, on the grounds that is is different kind of substance. But, they eventually *decided* against that.

LaPorte (2004: 101–2) then contrasts the cases of topaz and ruby. Topaz was initially identified by its yellow colour, but the discovery that some blue minerals have the same chemical composition led to their inclusion under the extension of 'topaz'. Ruby is also identified by its colour, this time red, but in this case the existence of blue minerals with the same underlying structure were *not* included within the term's extension.[1] The contrasting cases seems to suggest that it is a matter of choice whether we take the extension of a term to include newly discovered items that share some of the features of established sample but not others. It is thus a matter of choice that topaz is the mineral with formula $Al_2SiO_4(F,OH)_2$—presumably the discovery of blue minerals with the same formula could, as in the case of ruby, have led to a restriction of 'topaz' just to the yellow variety of the mineral—and consequently the essence of topaz is a matter of stipulation.

The story here, I think, may be a little more complicated. For in the case of ruby there was already a term available for the mineral species of which ruby is a variety, viz. corundum, and furthermore, there was already an ancient name in use for blue corundum, viz. 'sapphire'. So it is not clear how much choice played a part in the case of ruby. In which case it would not offer a fair contrast to the case of topaz, which in turn may then look less like a case of choice too.

However, the real problem with the cases of jade, topaz, and ruby is that we are dealing here with gemstones and so there are interests competing here with the scientific ones. It would not be a surprise to find that when there is tension between the two that the former predominate. It is not that the scientific concept is forgotten completely but rather that some compromise is sought. And so one could take the view that jade really was just nephrite but that the makers of jade artifacts decided that it was in their interests to allow jadeite also to count as jade. As LaPorte makes clear, not just any old material that might appear to be jade can count as jade.[2] There are other cases where this clearly happened. The calendar is an obvious case. The tropical (i.e. solar) year is 365.2423 days long. But clearly it is impractical to have a year starting six hours into the day. So the calendar year has 365 days except in leap years. The non-scientific calendar year differs from the scientific tropical year.

[1] The colour of a gemstone is typically an effect of impurities such as vanadium or chromium.

[2] Likewise it is worth noting that the chemical composition of ruby is at least part of its essence. Two of the stones in the Crown Jewels were once called rubies but are not regarded as rubies because of their distinct chemical composition; they are instead *spinels*.

Nonetheless the former concept is guided by the latter, in so far as the Julian and then Gregorian reforms were introduced with the aim of ensuring that the mean calendar year approximates closely to the mean tropical year (they now differ by one day in 35 centuries).

I believe that a similar diagnosis may be made of LaPorte's other principal chemical case, that of water. LaPorte rejects the Kripke–Putnam view that we discover that the essence of water is H_2O, because "*The majority of what we prescientifically called "water" has more than one microstructural feature that we could have concluded distinguishes the true from the spurious samples believed to be water*" (LaPorte 2004: 103. Original italics). LaPorte is referring to the fact that included among the samples typically called 'water' are molecules of deuterium oxide (D_2O), the oxide of the isotope of hydrogen containing a neutron as well as one proton. The doubling of the mass of the hydrogen atom means that the bonds involving deuterium are stronger than those involving protium, the much more common hydrogen isotope with no neutron. This leads to very slightly different chemical behaviours, principally slower reaction rates for reactions involving deuterium as opposed to protium. But in biological systems these small differences are amplified, such that plants will not grow in pure D_2O and animals will eventually sicken and die if the normal water in their diet is replaced by D_2O. LaPorte constructs a Twin-Earth-style story in which the travellers to Deuterium Earth find that the substance called 'water' there is in *some* superficial respects like water but in others certainly is not: it freezes at 3.82°C and boils at 101.4°C; it has a density 10% greater than that of water; it kills fish that attempt to live in it. The visitors to Deuterium Earth decide that this stuff is not water. This story has the same structure and so probative force as Putnam's Twin Earth story. LaPorte's conclusion is not, however, that D_2O does not count as water, but rather that there is vagueness in the term 'water' that may be precisified in more than one way. LaPorte tells his story in such a way that two communities—the visitors to Deuterium Earth and the Earth scientists back home (who, in the meantime, have discovered the existence of small amounts of D_2O in what they continue to call ‘water’)—can make different but both seemingly reasonable decisions about whether D_2O counts as water, and be in possession of exactly the same set of scientific facts.

It is unclear to me, however, that the decisions are equally reasonable then viewed from the point of view of chemistry. Arguably the visitors to Deuterium Earth are unduly influenced by their discovery that D_2O is toxic to animals and may be used in a design of bomb. But neither of these facts is of especial significance in chemistry. Furthermore, both these facts have analogues elsewhere. If all the potassium and carbon in our diet were replaced by the naturally occurring radioactive isotopes of those elements, then the resulting beta emissions would cause radiation sickness. Similarly, U-235 can be used in a fission weapon but U-238 cannot. But we are not inclined to regard it is plausible that we might have reserved the names ‘carbon’, ‘potassium’, and ‘uranium’ for certain isotopes alone. I should add that there are good reasons for chemists not wanting to have their principal division into kinds occurring at the more fine-grained level of isotopes. First, there would be enormous multiplication of chemical substances, almost all of which would be utterly redundant, with one or two esoteric exceptions. For example, instead of the one simplest organic molecule, methane, we would have forty-five naturally occurring different molecules involving the three naturally occurring isotopes of hydrogen and three naturally occurring isotopes of carbon. If we throw in the non-naturally occurring isotopes also, the number mounts to 225 different molecules. Secondly, many of these isotopically differentiated compounds cannot exist in a pure state. Consider the oxides of water: we have already discussed heavy water D_2O as well as light water P_2O. But we must add semi-heavy water DPO (not to mention the various variations due to the different isotopes of oxygen). But one cannot have a stable test tube full of liquid semi-heavy water, since liquid water exists in a partially ionized state allowing for the transfer of hydrogen nuclei between molecules, and a test tube of semi-heavy water would rapidly become a mixture of D_2O, P_2O, and DPO. So although there are three isotopically different molecules here, there are not three corresponding distinct substances. Chemistry, as the science of substances, naturally distinguishes in this case one substance composed of molecules, which, while all of one species, can be subdivided into 102 isotopically distinct varieties.[3] I suggest therefore that insofar as the travellers had reasons for not wanting to classify D_2O as water, they were motivated by concerns from beyond chemistry, and that their decision is not simply an alternative to the decision that Earth scientists in fact took.

In brief, then, may argument is: (i) water is a substance; (ii) chemistry is the science of substances; (iii) the balance of *chemical* reasons favour taking water to include D_2O, even if there are non-chemical reasons for excluding D_2O; therefore (iv) water includes D_2O. I have emphasized the reasons for accepting (iii). Premise (ii) is the definition of chemistry given by Linus Pauling (1947), "Chemistry is the science of substances—their structure, their properties, and the reactions that change them into other substances." Premise (i) might perhaps be doubted. Perhaps the response of the visitors to Deuterium Earth suggests that water is *not* a substance in the chemical sense. It is perhaps possible to construct a case that there is a vernacular use of the term 'water' according to which that term does not denote a substance or any sort of natural kind, noting that tea is not called 'water' whereas sea water is (despite the fact that both have similar proportions of H_2O).[4] But if so we won't find an apparently Earth-like world where that term is used to refer to all and only P_2O. The debate between LaPorte and me proceeds on the assumption that 'water' refers to some substance of other; LaPorte thinks it is a matter of choice which substance the term refers to, I don't (likewise the difference between the Earth scientists and the visitors to Deuterium Earth in LaPorte's story is a difference about which substance they take 'water' to refer to). Might 'water' be used in such way that it is intended that it refers to that infimal substance kind that is most similar to actual samples of water, across the board (including biological and military behaviour)?

[3] Hydrogen has three isotopes, oxygen has seventeen.

[4] Cf. Malt (1994) and Chomsky (1995). But see also Abbott (1997) for a response defending the claim that 'water' refers to the natural kind that is H_2O.

That would seem to pick out P_2O rather than H_2O. But strictly it would pick out one of seventeen kinds of P_2O that differ in their isotope of oxygen. The problem is this. We need an account of the semantics of 'water' that makes is a term that refers to a substance, albeit subject to a certain degree of open texture that allows it to be indeterminate which substance it refers to. But it is unclear that there is a way of making P_2O one of the potential referents, and that is because 'P_2O' aims to specify a substance partly in terms of an isotope (P) and partly in terms of an element (O) that is undifferentiated with respect to isotope. I am not sure that there is any way of doing that. To put the worry another way, I don't think that there really is a *substance* that is P_2O—P_2O is just a mixture of seventeen of the hundred-plus isotopic variants of H_2O; *a fortiori*, there is no substance P_2O that is a potential referent for 'water' even if the latter is regarded as having indeterminate reference.s

As regards LaPorte's chemical cases I have been arguing that while there may have been conceptual change or reasonable opportunity for such change, the motivations for such change have come from outside the relevant science, often from concerns of practical significance. It should not be surprising that there may be conceptual change when there are classificatory interests conflicting with the scientific ones. In which case one might ask whether the terms in question, in the scientific context, really do have the vagueness and open texture attributed to them—or whether this is just an artifact of external, practical concerns.

It might be argued in defence of LaPorte, or of a position similar to his, that my point might be correct, but the the phenomenon in question is ubiquitous, that the open texture often arises precisely because of a tension between scientific and other concerns, and that the latter is not the exception but the rule. One might go further to suggest that there really is not clear distinction between practical and scientific concerns.

Some terms for natural kinds spell out the essence of the kind to which they refer, or part of it, e.g. 'H_2O' and '*Homo sapiens*'; other terms, such as 'water' and 'man' do not. Let us call the latter 'essence-free' terms. Potential conceptual change as a result of an interaction between scientific and practical motivations for classificatory practices would be ubiquitous if:

> (a) most essence-free terms are employed in classificatory practices of practical significance; and
> (b) there is a potential tension between the scientific and the practical classificatory practices.

First a few remarks about (b). Talking of 'scientific' and 'practical' gives a sense of a false dichotomy. Of course, science is often practical and the practical is often scientific. Nonetheless, the tension I am referring to should be clear: it refers to cases where we might have some interest in classifying items in a way that differs from the manner of classifying them were we led *solely by the science of the kinds and entities in question.* Our practical interests may well be scientifically informed, but by a science other than the one appropriate to those kinds and entities. Biology determines the classifications (there may be more than one) appropriate to organisms; botany classifies plants and their parts; and chemistry classifies substances. Culinary interests may classify rhubarb as a fruit but exclude tomatoes, and it may perhaps be that such classifications could be informed by some culinary science, perhaps on the basis of fructose content or a certain kind of taste when cooked. But that classification, however scientifically informed, would not be the classification scientifically appropriate to entities in question, parts of plants, which must be a botanical classification.

This is what seems to be happening in LaPorte's D_2O case. The decision to identify 'water' with 'P_2O' thereby excluding D_2O from the extension of 'water' was scientifically informed. It was informed by toxicology and physiology as well as fusion physics. But those sciences are not the sciences appropriate to the classification of substances *qua* substances. If you want to know what a substance *is* rather than what it will do to your body or whether you can make a fusion bomb from it, you turn to a chemist. And it seems clear to me that chemists, left to themselves as it were, would do as they did do and regard D_2O as a subspecies of the water-kind. Let us imagine that chemists were fully aware of the different reaction rates for D_2O and P_2O, but it was also the case that the physiologies of most creatures were sufficiently robust that D_2O was not toxic for any mammal and for most other animals, excepting a few insignificant beetles. Would this state of affairs have led to the exclusion of D_2O from the extension of 'water'? I think not, and what this shows is that in LaPorte's thought experiment the limitation of water to P_2O, although not chemically disreputable, is not one that is driven by chemistry alone.

These remarks are intended to illustrate the way in which, for want of a better word, 'practical' interests may be in tension with scientific ones, which is not to say that the practical motivations are scientifically uninformed, nor that their outcomes are scientifically unrespectable. Even so, there seems to be a distinction between the classificatory interests of the science in question and other kinds of classificatory interest. The more significant question is whether (a) is true, since if (a) is false, then (b) is irrelevant. There are reasons for thinking that it might be quite common to find our essence-free terms employed in practical classificatory activities, since the items that we have bothered to classify in the vernacular are most likely to be those that have some practical interest to us. Most large mammals have common names, but few beetle species do.

On the other hand there are many cases where science has done the classificatory work irrespective of practical concerns but without knowledge of the essences of the kinds in question. The identification of the various chemical elements is a prime example of this. Nine elements were known to the ancients with four more being isolated or described in the medieval and renaissance periods. The seventeenth century saw the first discovery of an element, phosphorus, by chemical means. The chemical revolution of the eighteenth century brought a flood of discoveries of new elements that continued until the beginning of the twentieth century. Seventy newly discovered elements were identified and named between Georg Brandt's discovery of cobalt in 1735 and Georges Urbain's identification of lutetium in 1907. However, the modern concept of atomic number as nuclear charge came about only circa 1913, thanks to Henry Moseley, confirmed by Ernest Rutherford's discovery of the proton a few years later. However, one might instead reasonably argue that a satisfactory concept of atomic number was available after 1869, as the position in Mendeleev's periodic table. Even if we take the earlier date, forty-nine elements were discovered by chemists before it was possible to know their essences. Thus we have a large number of cases to which LaPorte's analysis does not apply. The examples from chemistry may be multiplied when we turn to inorganic and especially organic chemistry. Many compounds had been identified as such before Friedrich Kekulé and Archibald Couper introduced the notion of chemical structure in 1858, and since the means for creating and isolating new compounds did not necessarily imply a determinate chemical formula let alone structure, it remained possible for chemical compounds to be identified and named without knowing their essences. Similar remarks may be made about biological kinds. During the eighteenth and nineteenth centuries naturalists studied and identified myriad new kinds. If, as LaPorte argues, these kinds have historical essences, then those essences could not have been known or stipulated at the time of their discovery. This is a point to which I shall return.

I shall now sum up my discussion of LaPorte on chemical kinds. LaPorte's real and imaginary cases seem to suggest that new discoveries present us with opportunities to choose where the boundary of a natural kind term's extension lies. Such cases, urges LaPorte, exemplify vagueness or open texture which permit precisification as science progresses. I have suggested that these cases are misleading. Certain other influences over our classificatory scheme, such as concerns of a practical nature, are brought to bear from outside the science responsible for classifying and identifying the nature of the kind in question. If so, one cannot conclude that the choice and consequential conceptual change are to be explained on the basis of vagueness in the original concept. After all we can engage in conceptual change for all sorts of reason, the precisification of vague concepts being only one. If I am right, then it has not been shown that it is not perfectly determinate whether some newly discovered item falls under the original concept. This response, however, might be thought to be rather weaker if it turned out that every essence-free kind concept is subject to these kinds of potentially competing classificatory pressures. Then LaPorte's examples could hardly be dismissed as misleading. Even so, I think we could distinguish the competing interests, and can ask a counterfactual question, how would the scientists have ordered their classifications in the absence of such pressures. In the case of water and H_2O I think that the answer is clear. Putting that thought on one side, it is in any case true that many kind terms are free from such influences. One reason is that essence-free names for kinds can be introduced in a science in advance of knowledge of essences. This last point enables a general case of counterexample to LaPorte's more significant claim that essence-encapsulating facts are those that are the outcomes of precisifications of kind concepts. This is shown by the example of the elements discovered by scientists in the nineteenth century. Subsequent developments in atomic theory did nothing to change the relevant concepts. The concepts of iridium, sodium, silicon, argon, and so forth underwent no precisification with the development first of the periodic table and then the discovery of the proton. But those discoveries did furnish essences for those kinds.

4 Essences of biological kinds

LaPorte does however present cases where the appearance of conceptual choice cannot be explained by interests external to the science in question. LaPorte's discussion of *biological* kinds turns on quite different issues.

LaPorte first raises the case where scientists seem to have corrected earlier speakers' classifications. This looks to be fertile ground for the discovery of essences. For example, people have long taken the set of rodents to include not only mice and rats but also guinea pigs. But investigation shows that guinea pigs are not at all closely related to mice and rats—any clade that includes mice, rats, and guinea pigs should also include horses and many other mammals.[5] So it looks as if we have discovered that guinea pigs are not rodents. Yet, says LaPorte, this is not a discovery but a stipulation or choice. For the scientists had three options as regards the rodent grouping:

> (a) reduce the class so as to exclude guinea pigs;
> (b) expand the class so as to include the other mammals sharing the same clades as both guinea pigs and the other traditional rodents;
> (c) dispose of 'rodent' as a cladistically respectable term so as to retain its existing extension (both guinea pigs and other traditional rodents but not horses, primates etc.).

According to LaPorte, the fact that we ended up with (a) represents a choice on the part of the scientists. Expanding the grouping, (b), even dramatically, is not unprecedented—it is now common to hear that the dinosaurs did not go extinct but live on in the form of birds. As for (c), 'reptile', is a term that has been abandoned in cladistics but retained in the vernacular.

The relevant work on guinea pigs was carried out in the 1990s. Interestingly, the debates took place under the headline 'Is the guinea pig a rodent?',[6] and there seems an underlying presumption, on both sides of the still-continuing scientific debate, that *if* the science showed that guinea pigs have an ancestry that branches from the other mammals before primates and other traditional rodents, *then* guinea pigs would not be rodents. So there seems to be near universal agreement that (a) *would* be right. Nonetheless, there is some remaining tendency to say things such as 'rodents are not monophyletic' which would indicate a usage conforming to (c).

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[5] A *clade* is a class of organisms that includes an ancestral organism plus all (and only) its descendants. As John Dupré has pointed out, the cladistics-based essentialism under discussion will not work for micro-organisms since they experience a considerable degree of lateral gene transfer. Arguably this undermines cladistics for high organisms too, because they are dependent on the micro-organisms that make up the bulk of their body mass and which are also congenitally inherited.

[6] Cf. Graur et al. (1991); Li et al. (1992); Cao et al. (1994); D'Erchia et al. (1996); Sullivan and Swofford (1997).

But this may be just a hangover from previous habits—it was clear that the order *Rodentia* was held to be monophyletic. No hint of (b) as a possibility was given.

Here is a proposal as regards the use of names for higher taxa, in the face of apparent changes to extension:

(TAX)

> (i) the taxon should be a clade;
> (ii) a clear majority of subtaxa regarded as paradigmatic of the taxon should be included in the taxon;
> (iii) a clear majority of subtaxa regarded as typical foils for the taxon should be excluded from the taxon.[7]

If it is not possible to meet these requirements, the name is held not to name a taxon.

The idea behind (TAX) is that while we classify things under conditions of imperfect knowledge, we aim to do so in such a way that out taxonomic groupings are natural—in this case, so that they are clades. But because of our partial ignorance, we may in fact fail to achieve this. In the light of further information we are willing to correct our previous classifications. But how should we do so? We do so in such a way that, if possible, our new classification is natural (a clade) and that as many of our previous classifications as possible are maintained as correct. But not all previous classifications are equal—some are especially paradigmatic, and some function as typical foils. These prior classifications are the ones we are most interested in preserving. Another way of looking at (TAX) is that it says that a classificatory term designates that clade which best satisfies our existing classifications, at least the privileged ones. But if there is no reasonably nearby best satisfier, then the term is does not designate a clade at all, but is not a non-natural kind term.

[7] For the classic paradigm and foil account of kind extension see Quine (1969). Unlike Quine I am not suggesting that our paradigms and foils are definitive of the kind. Rather they represent our best guess at a kind in the absence of full scientific information.

(TAX) would explain the data that LaPorte presents.[8] As regards rodents, if Dan Graur and his colleagues are right, then (TAX) would tell us to exclude guinea pigs, since that keeps rodents as a clade, excludes only one family of putative typical rodents (the guinea pigs—an even these are not especially typical). The idea that we could extend the taxon to include pretty well all mammals would fail thanks to (iii).

Turning to dinosaurs, the inclusion of modern birds does not extend the apparent extension of 'dinosaur' to include enough foils to contravene (iii). Indeed I do not think that modern birds are a foil at all for the dinosaurs, since the typical dinosaurs are mesozoic animals and so the principal foils are mesozoic as well—that is, the triassic crocodilia and the thecodonts.

The case of reptiles is different, since their typical representatives are living rather than dead creatures. For reptiles the obvious foils are living mammals and living birds. But for reptiles to be monophyletic, they should include the birds also. Doing so would contravene (iii). On the other hand, excluding birds would require also excluding lizards, snakes, turtles, and tortoises, contravening (ii). So 'reptile' looks like a good candidate for exclusion from scientific taxonomy.

I would not wish to suggest that (TAX) should be part of anyone's best theory of taxonomy. Rather, I am suggesting that it is a reasonable hypothesis that is not excluded by LaPorte's data, nor by any argument he gives. Consequently, we cannot conclude with any degree of confidence that the outcomes in these cases are to be construed as choices that could have gone another way, as opposed to requirements of the way taxonomy works.

LaPorte continues his case against the discovery of essences with a discussion of the species problem and similar problems for higher taxa. As regards the species problem, LaPorte point out, first, that the different species concepts define quite different sets of species, and, secondly, even where the different species concepts agree, the difference between the concepts means that antecedently to the choice of a specific concept, we cannot be in a position to have knowledge of essences. I shall

[8] But it does, of course, leave a fair degree of vagueness nonetheless.

not dwell on these points, which though well-taken, do not seem to me to be especially problematic. One can be ignorant of what exactly constitutes essence while nonetheless knowing certain essential facts, and one can know such facts about a particular species so long as it is a species on any plausible species concept. The most obvious way to generate such cases is to take negations of species identity statements: the Ceylon Spiny Mouse (*Mus fernandoni*) is not the African Pygmy Mouse (*Mus minutoides*).

The problem of higher taxa arises because the boundaries of a taxon may not be clear. LaPorte gives the example of the giant panda: is it or is it not a bear? The latest common ancestor of the paradigm bears is just a little later than the latest common ancestor of the paradigm bears plus the panda. Both groups form clades. Which is the bear clade? According to LaPorte there was no fact of the matter at the time the panda was first encountered by Europeans. This argument shows at most that the agreement that a certain boundary species falls within the clade constitutes a concept-changing stipulation. It does not show that every assignment of a species to a clade is stiplulative. Even if we were to concede to LaPorte's argument, it remains the case that many other statements concerning clade membership are unaffected by this vagueness and can only be understood as discoveries rather than stipulations.

It is important to recollect that LaPorte is not attacking biological essentialism. On the contrary he defends essentialism, maintaining that biological kinds have historical essences of the kind Kripke (1980: 110–13) and McGinn (1976) ascribe to individuals. Thus tigers, members of the species *Panthera tigris*, necessarily are descended from the ancestor populations they are in fact descended from, and so are necessarily members of the cladistically defined broader kinds Mammalia and Chordata (LaPorte 2004: 33, 60–1).[9] LaPorte's dispute is just with the claim that such

[9] It is commonly held that Kripke thinks that the essence of a biological kind is, like that of a chemical kind, a matter of the internal structure of its members. While he does give this impression, it is also true he does not explicitly say that biological kinds have microstructural essences. He does say that we

essences are discovered. Thus he writes, "But though necessarily true, 'Mammalia = the clade that stems from the ancestral group *G*' and 'Aves = the clade descended from the ancestral group *A*' do not seem to me to have been discovered to be true. . . . these terms have undergone meaning refinement to *make* them refer to the relevant clades."

LaPorte does acknowledge that historical relationships are discovered, for example, "For any three organisms, *x*, *y*, and *z*, scientists can discover whether *x* and *y* belong to a species or clade that excludes *z*" (LaPorte 2004: 66). But if historical facts such as these can be discovered, and the essences of biological taxa are historical, then it follows that facts about essence can be discovered. It was discovered that modern birds are descended from therapod dinosaurs of the Cretaceous Period. And that fact is part of the essence of birds. This discovery of an essence is immune from LaPorte's discussion of the changing (apparent) extension of vernacular names, of the species problem, and other problems of vague boundaries for taxa. Similarly, while the panda may have been a boundary case, not all newly discovered bears are: given the paradigm bears, the rare Tibetan blue bear, first classified in 1854, has to be classified as a bear if the paradigm bears form a clade, however narrow or broad or vague the boundaries of that clade are.

LaPorte holds that 'the clade that stems from the ancestral group *G*' is a rigid designator. I think he is right. If so, then so are 'the smallest clade to which both species *A* and species *B* belong' and 'the clade descended from the common ancestor of organisms *X* and *Y*'. In that case we can have informative necessary identities that are revealed empirically. A statement of clade identity (CI) of the following form

> (CI) the smallest clade to which both species *A* and species *B* belong =
> the smallest clade to which both species *C* and species *D* belong

could determine kind membership—or more precisely kind *non*-membership by investigating internal structure, e.g. that a tiger-like reptile is not a tiger, and likewise for a cat-like automaton (Kripke 1980: 120–2). But that does not imply that true tigers and true cats have microstructural essences.

will be a necessary truth or a necessary falsehood, but one which will typically be known only as a result of a scientific investigation, and likewise for other groups or organisms that might substitute for A, B, C, and D. For example 'the smallest clade to which both gerbils and Delany's mouse belong = the smallest clade to which both spiny mice and Malagasy rats belong' is a necessary truth the can only be established using techniques involving nuclear and mitochondrial DNA.

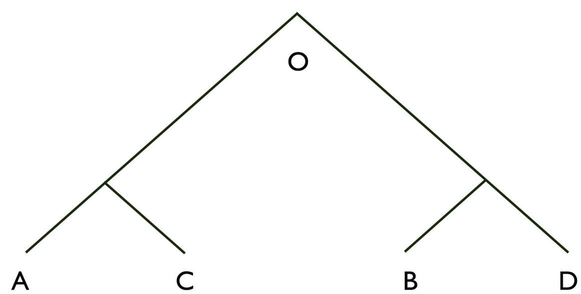

Figure 1: According to this cladogram, it is true that the smallest clade to which both species *A* and species *B* belong = the smallest clade to which both species *C* and species *D* belong.

In conclusion, while there may well be cases of vague taxonomic concepts in biology that get precisified as a result of new discoveries. The inclusion of a species within a clade (such as giant panda within the bear clade) might therefore be a case of an essence that it stipulated. But such cases far from exhaust the range of essential facts concerning biological classification. Several other kinds of case were identified: the inclusion of a newly discovered species within a clade, when that species falls between existing paradigm cases; non-identity claims between species; true claims of the form 'the smallest clade to which both species *A* and species *B* belong = the smallest clade to which both species *C* and species *D* belong'.

5 Lowe: essence precedes existence

LaPorte's resistance to *a posteriori* essences is founded on claims concerning actual taxonomic practice in science, whereas E. J. Lowe (2008) bases his argument on the metaphysical principle that essence precedes existence. This principle has an ontological aspect and an epistemological aspect. It is the latter that concerns us. Lowe says that in general we can know the essence of something antecedently to knowing whether is exists. One might take this claim as saying that for everything it is possible to know its essence without knowing whether it exists. That might seem a little strong, in that one might doubt whether it is true of every thing that we can know its essence at all. But the qualifier 'in general' may be supposed to exclude such extreme cases, restricting our attention to those things whose essences we can know; for them, we can know their essence without knowing of their existence. In my view we come to know the essence of water by investigating samples of water. But water could be like synthetic compounds who natures were known before chemists attempted to synthesize them, or like synthetic transuranic elements whose essences were known before any samples were created. However, as Lowe himself points out, our knowledge of the essences of those elements is founded on the knowledge of the existence of other things, protons, neutrons, and so forth. But if that is the case it looks as if it would not be possible to know of the essences of *all* kinds without knowing of the existence of their instances.[10] Conceivably, one might come to know of the essence of protons thanks to knowing of the existence of some other subatomic entity (quarks, perhaps). But it looks as if this process will have to stop with some kind whose essence we do not know before we know of its existence.[11]

[10]This is not to say that there must be some basic kind such that its existence is known before its essence. There may be kinds K_1 and K_2 such that one needs to know of the existence of one or other of them before knowing of the essence of the other; but it need not be that one is more basic, in this respect, than the other.

[11]Perhaps we could know of the essence of kinds simply by knowing the laws of nature which might tell us how certain properties are appropriately co-instantiated in kinds. This is a contentious but not implausible proposal. But it is not one that Lowe himself would endorse for two reasons. First, it would

Lowe in fact seems to have something stronger in mind than the interpretation given above. For he defends the principle by saying that "Otherwise ... we could never find out *that* something exists. For how could we find out *that* something, *X*, exists before knowing *what X* is—before knowing, that is, *what it is* whose existence we have supposedly discovered?"[12] This defence of the principle suggests that knowledge of essence is *necessary* for knowledge of existence to be possible, that is one *must* know something's essence before we know its existence. Lowe does concede that it is conceivable that God's essence does not precedes his existence; but Lowe's choice of such an esoteric case reinforces the interpretation that he thinks that for all ordinary things, such as natural kinds, knowledge of their essence is not preceded by knowledge of their existence.

If Lowe is right, then we cannot come to know the essence of a natural kind by *a posteriori* investigation of its instances. For that would be a case of knowing the existence of the kind before knowing its essence. Accordingly, Lowe resists the Kripkean arguments for *a posteriori* knowledge of essences. In particular, therefore, since water was known before it was known that samples of water are constituted by H_2O, it cannot be that being constituted by H_2O is any part of the essence of water.

Lowe agrees with Nathan Salmon (1982) that substantive knowledge of essences cannot be gained from semantic, conceptual, and empirical knowledge alone, that the semantic arguments presented for modally significant arguments, if they are valid, must contain modally charged premises or rely on unstated modal assumptions. Thus Lowe maintains that if the form of the assertion 'water is H_2O' is understood as an analogue to 'Hesperus is Phosophorus' (i.e. as two rigid designators flanking the identity sign), then the former is a triviality (like the latter). I agree with Lowe and Salmon that the Kripke–Putnam arguments do deliver their conclusions

amount to a reductive account of kinds, whereas Lowe takes natural kinds to be a fundamental ontological category in his four category ontology. Secondly, Lowe regards laws themselves as employing kinds.

[12] Lowe (2008: 40). This principle is reminiscent of the Socratic contention that in order to have knowledge of something one must be able to define it.

by appeal to modal intuitions. That appeal is sometimes tacit, but not always. Many of Kripke's arguments make no use of semantic machinery at all and are clearly to be understood as appeals to metaphysical intuition. Kripke's arguments concerning the essentiality of origin exemplify this, as do his arguments concerning tigers, gold, and other kinds. Consequently I think it is misleading to think of the arguments for the necessity of theoretical identities as proceeding along the lines of arguments for the necessity of 'Samuel Clemens is Mark Twain'. What those arguments really establish is that the relevant alleged essence is (necessarily) necessary and sufficient for instantiation of the kind. Furthermore the two components, necessary conditions and sufficient conditions, can be decoupled, so that one may regard the arguments as establishing partial essences, that is (necessarily) necessary conditions for kind membership, even if they do not establish (necessarily) sufficient conditions. To illustrate the above, one can recast Putnam's Twin Earth thought experiment as asking whether samples constituted by XYZ are samples of water, and delivering by appeal to intuition (or what Lowe calls "insight into nature or essence") the answer *no*. But that answer shows only that being constituted by H_2O is a necessary condition of being water, not that it is a sufficient condition. If I am right that the arguments for *a posteriori* essences (e.g. water's essence being, in part, a matter of being constituted by H_2O) are either overtly dependent on intuition or may be reconfigured so that they are, then Lowe's (and Salmon's) complaints about the inadequacy of semantic arguments are immaterial.

Let us return then to Lowe's key contention, that unless we suppose that there are essences and that we know about them, we cannot "talk or think comprehendingly about things at all". Lowe (2008: 35–6) gives an example, "How, for instance, can I talk or think comprehendingly about *Tom*, a particular cat, if I simply don't know what cats are and which cat, in particular, Tom is?" Lowe concedes that I don't need to know everything about cats to think about Tom, and in a footnote suggests that possibly I need not know more about cats than that they are animals or living organisms. This is an important concession. For now Lowe's condition for comprehending thought about something places only a very weak requirement on how much knowledge of essence is required. As a result, Lowe's claim leaves open the possibility that while some knowledge of the essence of X is required to think comprehendingly about Xs, there may be many other essential facts about Xs, knowledge of which is not required for comprehending thought about Xs. If only partial knowledge of the essence of Xs is thereby required to be *a priori*, then Lowe's claim does not rule out the contention that some facts (perhaps many) concerning essence are knowable only *a posteriori*.

Lowe's argument would only rule out this space for *a posteriori* essentialism if what I called 'partial essence', that component of essence required to be known *a priori*, is in fact *all* there is to essence. But that seems implausible as well as something Lowe would not himself accept. For, as he says, essence concerns the nature of something, what it is to be that thing or to be of that kind. So different kinds must have different essences. But as we saw, Lowe conceded that in order to think about cats or some particular cat, all I need know is that cats are animals. Yet that essential fact about cats does not differentiate cats from pigeons. Of course, there are debates about whether species and other biological kinds have essences at all. But the point is a general one. If Lowe's requirement for comprehending thought is correct, it might be, in order to think comprehendingly of some copper object, such as the dome of St Paul's Cathedral, MN, that I know that copper is a metal (or perhaps only that is is a material substance). But that is not enough to distinguish copper from any other metal. So nothing in Lowe's argument, even if we grant it, demands that further facts about copper's essence may not be known *a posteriori*.

The forgoing response to Lowe's 'comprehending thought' argument makes it look thoroughly implausible that there are no aspects of kind essences that are known only *a posteriori*. A possible response for Lowe would be to deny that we genuinely know that something exists at all before discovering its essence. For example, we only really know that what we have been calling 'water' is a substance when we discover that it is H_2O. That discovery of essence tells us that 'water' refers to a kind rather than to a mixture or hybrid of kinds, like 'jade'. In that sense our knowledge of the existence of the kind or substance water is preceded by our (*a posteriori*) knowledge of its essence. Such a view would be not so very distant from that articlared by LaPorte and discussed at length above; LaPorte would add that the process of scientific discovery reveals that there are several possible candidate essences, determining different kinds, each consistent with priori usage and intention, and so there is a choice to be made about which of the kinds the kind term refers to (or indeed whether it refers to a kind at all). This decision is made in the light of knowledge of candidate essences, and so knowledge of actual essence, the one chosen, is *a priori*.

The problem with this argument is that it assumes that in order to know that something is a genuine and unique kind or substance one must know its essence. But as the discussion of particulars shows, we can know that something (e.g. Tom) is a genuine particular without knowing much about its essence at all. The same is true for kinds. Kind essences include facts that differentiate kinds from one another. But many kinds are such that we cannot have this knowledge *a priori*. On the other hand, there are features that *do* allow us to distinguish kinds, but these facts may very well not be essential. So we can distinguish and identify genuine kinds and substances without knowledge of the relevant essences.

Above, in section 3, I mentioned the many transition elements discovered during the nineteenth century. At that time facts about atomic structure were unknown. Chemists were nonetheless able to distinguish the different elements, using properties such as relative atomic mass and physical characteristics such as melting point. But those properties cannot be regarded as constituting essences. For example, the relative atomic mass of an element cannot be essential to it, since it is a mean of the atomic masses of the isotopes of the element, weighted by their relative abundance on Earth. But those relative abundances can be highly contingent. Melting point fares little better: it too is, strictly speaking, dependent on isotope abundance; more pertinently, different allotropes of an element will have different melting points. So one can have good empirical grounds, as nineteenth century chemists did, for thinking that one has discovered two distinct elements, X and Y, but those grounds supply only a non-essential basis for distinguishing X and Y. Hence, on the assumption that kinds essences do distinguish kinds, it follows that there are facts about the essences of X and Y that remain to be discovered by *a posteriori* means. If there are kind essences, it is the case that these essences, including essences that distinguish between kinds, are often discovered *a posteriori* and, in particular, after knowledge that the kind exists and is a kind.

6 Is there derived *a posteriori* knowledge of essences?

Given that Lowe's requirement for *a priori* knowledge is relatively thin and general, leaving considerable space for potentially *a posteriori* knowledge of essences, one might think that Lowe should concede that there is *a posteriori* knowledge of kind essences, but claim that this is derived from non-modal empirical knowledge plus *a priori* knowledge of essences. Thus Lowe can still claim that knowledge of essences is fundamentally *a priori*.

Certainly, the idea of *a posteriori* knowledge of truths that are necessary should be familiar by now. Consider the proposition $p \vee q$ where p is some abstruse mathematical claim ('Liouville's theorem is true') and q is some obvious empirical claim ('grass is green'). For many people $p \vee q$ is known empirically even though it is necessary. Now consider the following argument:

> (a) If K is a chemical kind, then all samples of K share the same chemically relevant microstructure.
>
> (b) The chemically relevant microstructure of **s**, a sample of the chemical substance thallium, is that it is composed of atoms with atomic number 81.
>
> *therefore*

(c) All samples of thallium are composed of atoms with atomic number 81.

This argument has a necessary and arguably *a priori* first premise and an *a posteriori* second premise. The conclusion is necessary and *a posteriori*, as one can see when explicit mention is made of essences in the following argument:

(a′) If K is a chemical kind, then it is part of the essence of K that all samples of K share the same chemically relevant microstructure.

(b) The chemically relevant microstructure of **s**, a sample of the chemical substance thallium, is that it is composed of atoms with atomic number 81.

therefore

(c′) It is part of the essence of thallium that all samples of thallium are composed of atoms with atomic number 81.

This suggests that *a posteriori* knowledge of essential truths can be factorized into *a priori* knowledge of an essentially true premise plus *a posteriori* knowledge of a modally innocent premise.[13] Nonetheless, Lowe (2007) wants to resist even this. Lowe's argument is that we do not have a worked-out logic of essence that permits us to see whether the inference from (a′) and (b) to (c′) is valid. The fact that orthodox modal logic tells us that the argument is valid is no help for two reasons. First, as Fine (1994) reminds us, what is entailed by an essential truth many not be an essential truth although it will be a necessary truth—a worked-out logic of essence would be restricted in comparison to modal logic, and so the validity of the corresponding modal inference does not imply the validity of the argument framed in terms of essence. Secondly, Lowe does not believe that we should be so quick to endorse orthodox modal logic in any case, since the orthodoxy is informed by a possible-worlds

[13] I say 'modally innocent' rather than contingent, since some elements of (b) may not be contingent: it may be necessary that thallium is a chemical kind and that **s** is a sample of thallium. But these necessary truths play no part in explaining the necessity of (c).

model of modality, which is antithetical to the view that "represents necessary truths as being grounded in truths about essence".

I find neither of these reasons for resisting the inference compelling. It is certainly true that we should not endorse an inference concerning essences just because the corresponding modal inference is valid. Although necessarily everything is such that Fermat's last theorem is true, the truth of Fermat's last theorem is not part of the essence of every object. That much is clear because essential truths concern the *nature* or *identity* of things, and the truth of Fermat's last theorem does not concern the nature or identity of most things. But when we turn to (c), there is no reason to suppose that it does not express an essential truth, for it does concern the nature and identity of thallium. While we would expect a worked-out logic of essence to reject inferences with conclusions such as 'thallium is essentially such that Fermat's last theorem is true' we would not expect that logic to exclude inferences with the conclusion 'thallium is essentially such that its samples are composed of atoms with atomic number 81'.

Nor should we be afraid of endorsing orthodox modal logic even if modality is grounded in facts about essence, not about possible worlds. Modal logic was well-advanced before the possible-worlds model was introduced, and Kripke himself is cautious about thinking of modality of terms of possible worlds. And one kind of thing, A, can be grounded in another kind of thing, B, yet we may have a better knowledge of A than of B. The truth of Maxwell's equations are grounded in deeper facts concerning the structure of space and time and concerning the source of electromagnetism, matters about which he was largely either ignorant or had false beliefs. But that ignorance was no obstacle to his knowing the truth of his equations. Modal logic concerns the formalization of facts about the way things can be and must be, and we can know a reasonable amount about that without knowing what grounds such facts. On the assumption that thallium is a natural kind, the argument from (a′) and (b) to (c′) can be recast in terms of necessity thus:

(a″) $\Box\forall x\forall z[\mathrm{K}x\wedge\mathbf{m}(x)=z \rightarrow \Box\forall y[\mathrm{K}y \rightarrow \mathbf{m}(y)=z]]$;

(b′) $\mathrm{K}\mathbf{s}\wedge\mathbf{m}(\mathbf{s})=\mathbf{a}$;

therefore

(c″) $\Box\forall y[\mathrm{K}y \rightarrow \mathbf{m}(y)=\mathbf{a}]$,

> where $\mathrm{K}x \equiv x$ is a sample of thallium, $\mathbf{m}(x)$ is a function from x to x's chemically relevant microstructure, and **a** is the microstructure that is a matter of being composed of atoms with atomic number 81.

But this argument does not rest upon principles of modal logic that are validated only by appeal to possible worlds semantics or which may be invalidated by some satisfactory future logic of essence.[14]

7 Conclusion

LaPorte (2004: 110–11) accepts that, "The possibility of a posteriori discoveries of essences seems *coherent*, then, even if natural language does not cooperate with the Kripke–Putnam picture. The orthodoxy that 'Water = H_2O' was discovered by empirical investigation seems wrong. But the limited claim that there *could be* statements about kinds' essences that are discovered to be true by empirical investigation seems right." In my view LaPorte has underestimated the extent of essentialism's scope. The reference to natural language suggests that he has in mind 'Water = H_2O' as a paradigm, where a vernacular kind is identified with a scientific kind, where the latter articulates the essence of the former. Even if this may be the impression that Kripke gives, essentialist truths go well beyond propositions of that form. Essentialist claims need not involve vernacular terms ('water') or scientific terms that are precisifications of such terms ('mammalia') at all. It is possible to introduce a kind term with a high degree of confidence that it does indeed name some kind but in

[14] The arguments of this and the preceding paragraph are articulated in more detail in Bird 2008.

ignorance of what the underlying kind essence is. The elements discovered in the late eighteenth and early nineteenth centuries provide an example of this. Chemists were confident that they had identified new elements, but only in 1869 could they have begun to understand the essences of those elements, and that understanding could not have been completed until circa 1913–18.

Much of LaPorte's argument rests on the vagueness of kind terms. But the possibility that when we move from vague concept to a more precise replacement, we stipulate rather than discover what belongs to the kind, can only ever affect the status of boundary cases. But not all cases are boundary cases. Some kinds fall clearly within higher taxa and some clearly outside. Those cases, such as the fact that the Tibetan blue bear is a bear but the koala is not, are genuine discoveries of essential facts. Cladistics in fact permits the removal of much vagueness: 'bear' might be vague, but 'the smallest clade to which brown bears and pandas belong' is much less so. There may be some residual vagueness still, but that won't affect identities of the form of (CI) given above.

Lowe's argument for *a priori* essences comes from a different source—a requirement for comprehending thought. My conclusion is similar, that the argument establishes at most that *some* essential truths must be knowable *a priori*. That argument fails to show that no essential truth is knowable *a posteriori*, since it gives no reason to exclude additional essential truths being known as a result of empirical enquiry. Lowe resists the thought that *a priori* knowledge of essence can be supplemented by empirical knowledge to yield new *a posteriori* knowledge of essence, but the general reasons he gives, while applicable in some cases of putative *a posteriori* knowledge of essences, are not obviously applicable to all cases. Again the example of the chemical elements in the nineteenth century is useful, since the example shows that we can have identifying knowledge of a kind without complete or even identifying knowledge of its essence. In such cases further knowledge of essence, knowledge of atomic number, is clearly *a posteriori*.

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