DISCOVERING THE ESSENCES OF NATURAL KINDS

1

Following Kripke, Putnam, and others, many hold that natural kinds have essences, and that these essences may be discovered *a posteriori*. Joseph LaPorte (2004) very carefully, and in many respects convincingly, articulates an alternative view of what is occurring. Concentrating on theoretical identities, such as 'water is H_2O', LaPorte argues that there is considerable vagueness in the use of kind terms, especially vernacular kind terms. This vagueness is a matter of *open texture*. For a kind term 'K', some things will be determinately K and other things will be determinately not K. But there will be a boundary of things for which there is no fact of the matter whether they are K or not. This means that there will be no determinate fact of the matter that 'K_1=K_2' for distinct kind terms 'K_1' and 'K_2'. When a natural kind identity is established as being determinately true, that is because scientists have made a *decision* to adopt the identity as true. In so doing, it will now be determined of items that were previously in the boundary (neither K nor not K) whether they are K or not. For example, we now regard heavy water (deuterium oxide) as a subspecies of water; but scientists could have decided to exclude deuterium oxide from the extension of 'water'. So 'water is H_2O' is true in virtue of a decision. That truth, LaPorte emphasizes, is indeed a necessary truth, but it is not the discovery of some hidden essence. Rather it is an empirically motivated *stipulation*.

While I believe that LaPorte's discussion of these issues, furnishes us with many important insights, I will ague in this article for the following claims, in successive sections:

- There is rather less room for conceptual choice and stipulation than LaPorte supposes. His view is that when a stipulation is made that 'V=S' where 'V' is a vernacular term and 'S' is a more precise scientific term, there is a precisification of the vernacular term 'V'. Within the prior vagueness of 'V' there is considerable room for choice as to how the term might be precisified, which is why the truth of 'V=S' is a matter of decision. I argue that the vernacular concept is governed by rules of application that do not leave such scope for decision.

- There are many essential truths whose truth cannot be accounted for in the way that LaPorte suggests. We should not focus too much on identities, since we can discover many essentialist truths that are not identities.

- A particular set of cases that LaPorte's view does not accommodate includes identities of the form 'K_1=K_2' where 'K_1' is not a vernacular term, but is a scientific term without the high degree of vagueness attributed by LaPorte to ver-

nacular terms. 'K_1=K_2' is nonetheless not an analytic truth, because 'K_1' is introduced before later discoveries in science that allow for the articulation of the identity statement. I expand on certain examples, in particular chemical elements discovered in the nineteenth century before atomic structure was understood.

2

In this section I argue that there is less scope for decision concerning the application of our vernacular kind concepts than LaPorte suggests. (Some of the points made in this section are discussed in greater detail in my (2007).) It is important to note that LaPorte is saying more than simply that our natural kind concepts can change and that our vernacular terms can be stipulated to have new extensions in the light of developments in scientific knowledge. LaPorte's more specific claim is that such changes are in an important way consistent with the existing use of the vernacular concept. When we agree that '$V=K_2$' we are precisifiying the concept *V*. Let us call the older vernacular concept V_O and the newer precisified concept V_N. We may think of V_O as having a determinate extension (i.e. things that are clear cases of V_O), a determinate anti-extension (i.e. things that are clearly not cases of V_O), and a boundary (things that are both not clearly V_O and not clearly not V_O). The concept V_N includes the determinate extension of V_O. A precisification divides the boundary between the extension of V_O and its anti-extension. But nothing in the prior concept, V_O, determines where within the boundary a legitimate division should fall. Anywhere is permitted, and the extension of V_N may include all or none of the boundary of V_O.

For LaPorte's case to be made, it is necessary that the relevant examples satisfy three requirements:

1. V_O and V_N are both natural kind concepts.

2. Any stipulation made with regard to the extension of V_N really is a matter of precisifying V_O rather than conceptual shift. (By 'conceptual shift' I mean a conceptual change from C_O to C_N where some *x* was C_O but is not C_N, or was not C_O and is C_N. Precisification is not conceptual shift, because it involves no change to the determinate extension or anti-extension, only to the boundary.)

3. The concept V_O does not itself determine the division of the boundary region between its extension and anti-extension. (If V_O did this, then V_O and V_N would not really be distinct concepts.)

In my opinion, a counter-view to that of LaPorte can argue that each of his representative cases fails to satisfy one or more of the conditions listed.

For example, LaPorte considers the case of 'water = H_2O' (where H_2O includes all isotopic variants). He re-runs a twin-earth style thought experiment to show that scientists could, in the light of information about D_2O (e.g. that it is poisonous), reasonably have precisified the term 'water', so that its excludes D_2O, with the consequence that 'water = H_2O' is false (but 'water = P_2O' is true, where 'P' denotes protium, the isotope of hydrogen with mass number 1). I argue that scientists were not free to make such a decision without violating the concept *water* in a way that does not count as a precisification. That is the the concept $water_O$ did determine that D_2O falls within its extension. Had scientists decided to exclude D_2O from the

extension of $water_O$, then $water_N$ would not have be a precisification of $water_O$. The principle upon which I make this claim asserts that there is a division of linguistic labour among scientists, such that it is the job of a particular subset of scientists to determine the facts concerning particular sorts of natural kinds. Thus it is the job of biologists to determine the nature of and relationships between the various sorts of organism, while it is the role of chemists to determine the nature and identity of substances. Thus Linus Pauling (1970: 1) tells us:

> The different kinds of matter are called *substances*. Chemistry is the science of substances—their structure, their properties, and the reactions that change them into other substances.

In the light of this, it will be chemical facts that determine the identity of substances. The chemical facts class D_2O with other kinds of H_2O. The structure of D_2O is the same as that of the other isotopic variants of H_2O, and the reactions it engages in are the same. Its qualitative chemical properties are also the same. D_2O differs chemically from the other isotopic variants as regards certain quantitative features, such as rates of reaction. Strictly, all isotopic variants of all chemical substances differ from one another in such quantitative ways, but the difference is much more marked in the case of D_2O. As a result of this marked different in reaction rate, pure D_2O in place of water can be poisonous for many organisms. LaPorte regards this one of the reasons why scientists in his twin-earth story would be willing to exclude D_2O from the extension of $water_N$. Another reason is that D_2O can be used in the manufacture of fusion bombs in a way that the other isotopic variants cannot. Note that both these reasons come from outside chemistry. They are reasons, therefore, that are not pertinent to the science whose job is is to investigate the nature of and to classify water. Given the principle enunciated above, I regard $water_O$ as including D_2O within its extension and so see no distinction between $water_O$ and $water_N$.

I am not suggesting that LaPorte's twin-earth story is implausible or that his hypothetical scientists would be unreasonable. Undoubtedly our concepts do indeed change, which is to say that a term may first express one concept than another, distinct but usually related concept. Rather, I am saying that their decision would be a decision not to precisify the concept *water* but to shift it, so as to exclude, on sensible pragmatic grounds, a subset of its earlier extension. Thus the actual history of the concept *water* violates requirement 3., in the sense that the concept $water_O$ already determined whether the apparent boundary region between determinate water and determinate non-water should be drawn. The concept $water_O$ just is the concept $water_N$. On the other hand, the hypothetical change envisaged by LaPorte violates requirement 2, since his story involves a conceptual shift.

The pragmatic concerns that might stimulate a conceptual shift are likely to be present in the use of vernacular terms for kinds, since those kinds play a significant part in our social, economic, and cultural lives. I regard LaPorte's very informative discussion of the history of the term 'jade' in this light. LaPorte points out (in contrast to almost all other philosophical discussions), that Chinese jade workers and experts were fully aware that a new jade-like substance, jadeite, that was being imported into China, was different from their traditional jade, nephrite. Nonetheless, they took a decision to regard the new material as an instance of 'jade', along with their traditional nephritic jade. LaPorte take this to show that we cannot be confident of Putnam's judgment that faced with XYZ and the facts of its composition, we would deny it is is water. Oscar and his friends might very well decide that it is reasonable to call XYZ 'water', in order to indicate to other Earthlings that it is safe to

use just like Earth water, just as they might, in LaPorte's story decide to exclude D_2O because of its toxicity. But it does not follow from the facts that such decisions might reasonably be made that such decisions are precisifications as opposed to conceptual shifts. And even if the decisions are precisifications, it needs to be argued that the terms in question are indeed natural kind terms. So in the case of jade, we might agree with LaPorte that 'jade' (or, rather, the Chinese term 'yü') had an open texture such that while nephrite was determinately jade, jadeite was initially in the boundary region, and furthermore that Chinese jade workers then decided to precisify the term by including jadeite. But why should we regard 'jade' as a natural kind term? The fact that the extension of a term 'T' is the extension of a natural kind term does not make T a natural kind. The extension of 'humodo' includes all humans and dodos, and so has a current extension that is a natural kind, but 'humodo' is not a natural kind term.[1] So even if the (determinate) extension of $jade_O$ was precisely nephrite, we cannot conclude from that fact that 'jade' was then a natural kind term. In my view, if the term failed determinately to exclude a substance of very different composition, then that shows it did not name a natural substance, and so requirement 1 fails. Equally, if 'jade' did name a natural substance, then the decision by Chinese jade workers and connoisseurs to admit jadeite as a kind of jade amounts to a conceptual shift, from a natural kind term that excluded jadeite to a term that is not a natural kind term and which includes jadeite.

LaPorte (2004: 97) anticipates such a response, remarking 'but the claim that that is the right moral to draw seems ill-supported and motivated only by a desire to save a theory.' However, my motivation is that alluded to above, that chemists regard it as a necessary condition of being the same substance that two samples share the same or very similar composition and engage in the same reactions. Clearly $jade_N$ is not the concept of a natural kind of substance, since it violates this condition. If $jade_O$ had the open texture that LaPorte ascribes to it, then it would equally have violated this condition, since this condition determinately excludes samples of jadeite from being the same substance as samples of (traditional) jade, whereas the open texture view leaves that indeterminate. So either $jade_O$ was not a natural kind concept (violating requirement 1) or it was a natural kind concept, but the change to $jade_N$ is not a precisification (violating requirement 2). Either way the change from $jade_O$ to $jade_N$ is not a precisification of a natural kind concept.[2] If *jade* never was a natural kind concept that would not be surprising if the relevant experts to whom we defer in the division of linguistic labour in deciding what determines the extension of a concept, are in this case not chemists but are jade workers and connoisseurs, whose position is rather closer to those who have to decide whether sparkling wine made outside of the champagne region of France may be called 'champagne' or cheese manufactured other than in Somerset is correctly designated 'cheddar'.

Our discussion suggests that our concepts might fail to be natural kind concepts, because certain practical concerns make some other kind of concept more useful. Nonetheless, when classifying natural objects, classifying them by their natural

[1] It might be objected that the extension of 'humodo' includes not only current humans and dodos but also past and future ones, and so does not have a natural kind extension. We can change the example the to 'humalien' whose extension includes humans and bug-eyed aliens from Mars (presumably non-existent at all times).

[2] The concept $jade_N$ clearly violates requirement 1. But that is not relevant here, since LaPorte's point about jade is to illustrate the fact that natural kind terms can have open texture and so natural kind concepts do no determine the response that Putnam claims they do. It is consistent with that view that some decisions about how to precisify an open textured natural kind concept should lead to a non-natural kind concept.

kinds is one compelling way to go. Often practical interests and the desire to classify things by their natural kinds will coincide. We want to classify ores by their natural kinds, because the same natural kind of ore will produce the same metals and other minerals. We classify the metals by their natural kinds, because the instances of the same natural kind of metal will possess the same properties, the properties that make the metals useful for a distinct purpose. But, as in the case of jade, there may be interests that pull away from the natural kind concept. Another kind of practical concern is that of maintaining as much of an existing pattern of usage as possible in the light of discovery. Because of their superficial similarity and ignorance of their deeper differences, items of kind X and kind Y might be both be regarded of kind K. When the deeper differences are discovered, there will be a tension between, on the one hand, maintaining 'K' as a natural kind term, and, on the other hand, the inconvenience of suddenly denying that Ys are Ks. As in LaPorte's discussion, a choice will have to be made, but this does not mean that the older concept K_O does not determine an extension or that the choice is between different precisifications of K_O. We can choose, if we wish, to make a conceptual shift. LaPorte discussed a number of interesting biological cases. Some zoologists think that guinea pigs latest common ancestral population shared with all other creatures commonly classed as rodents is very early, and that this ancestral population is also an ancestral population of many non-rodent mammals, such as horses. We could go in three directions: (a) deny that guinea pigs are rodents; (b) accept guinea pigs as rodents and regard the kind of rodents as including guinea pigs and all the other standard rodents, *plus* the intermediate kind (horses etc.); *rodent* would still be a natural kind concept; (c) accept guinea pigs as rodents and regard the kind of rodents as including guinea pigs and all the other standard rodents, but *exclude* horses and so forth; 'rodent' would be a polyphyletic classification—covering two kinds but not the kinds in-between, and as such $rodent_N$ would not be a natural kind concept. While (a) is the choice of some zoologists, LaPorte points out that (b) and (c) have precedents. When it was discovered that birds are descended from dinosaurs, it became widely accepted that birds are living dinosaurs. On the other hand, that fact also means that if *reptile* were to be monophyletic (covering all of just one clade), then birds would have to be classed as reptiles. A consequence of this has been a move to exclude the term 'reptile' from taxonomy, because it would be paraphyletic (covering a clade minus one sub-clade, the birds).

The fact that different choices might be made may suggest (as LaPorte holds) that there is open texture that permits these different precisifications of a natural kind term such as 'rodent'. While I should repeat that a choice does not imply a precisification (it could be a shift), there is, however, a pattern to the decisions made that suggests that our past uses of the relevant kind terms does determine an extension. The idea is this. We use the relevant terms with the intention that they name a natural kind. Which kind, K, is that? The answer is given by the principle (TAX): (i) K must be a clade (i.e. 'K' should be monophyletic—which is to say, it really does pick out a natural kind); (ii) a clear majority of subtaxa regarded as paradigmatic of K should be included in the extension of*K.* (iii) a clear majority of subtaxa regarded as typical foils for K (i.e. paradigmatic non-Ks) should be excluded from the taxon. If it is impossible to meet these requirements, then 'K' does not pick out a natural kind, in which case it may well be natural to continue to use it in the vernacular, as a polyphyletic or paraphyletic non-natural kind term. While there may well be residual vagueness, it does determine answers to 'are guinea pigs rodents?' (no: because rodents form a clade by excluding guinea pigs but not mice, rats, gerbils and so on,

and without including horses and primates, and 'are birds dinosaurs?' (yes: because dinosaurs taken to include birds form a clade; birds are not important, paradigmatic foils for dinosaurs, since the paradigmatic foils for dinosaurs are not modern creatures but are mesozoic animals such as the triassic crocodilia and the thecodonts. Lastly, if we ask 'are birds reptiles?' the answer 'yes' would include as reptiles an important group of paradigmatic foils for reptiles, the birds. On the other hand, the answer 'no' makes it impossible to regard reptiles as forming a clade. 'Reptile' must therefore be excluded from scientific taxonomy, and not be regarded as a natural kind classification. But it may be retained in the vernacular as a paraphyletic, non-natural classification.

In this discussion I have not disproved LaPorte's claim that natural kind terms have an open texture that allows for precisification. Rather, I have contrasted his view with another which also fits the data in a non-arbitrary and, I hope, non-question-begging way, and which regards natural kind terms as having much more determinate extensions.

3

In this brief section I argue that LaPorte's arguments about identities, and the claim their truth is stipulated, leaves many essentialist claims untouched. I shall not press this point at length because LaPorte accepts it. Nonetheless, it is worth reiterating because it illustrates two distinct forms of argument for essentialism. One draws its force from Kripke's discussion of names and rigidity. If 'K_1=K_2' expresses a true proposition, and if 'K_1' and 'K_2' are rigid designators, then it is necessarily the case that K_1=K_2 (the rigid designators might be rigidified definite descriptions). Furthermore, depending on the content of the term 'K_2' 'K_1=K_2' *might* reveal the essence of 'K_1', as in 'water = dthat(the compound H_2O)' (although there is of course no guarantee—some further argument is required in any particular case to show that the necessity thus produced is an essentialist necessity). However, according to LaPorte, it might not be determinate which natural kind 'water' refers to, and so there is room for different precisifications, one of which makes 'water = H_2O' true, other of which do not. If we choose to precisify 'water' in that way, then 'water = H_2O' comes out as necessarily true, for the reasons given. But this necessity is the result of stipulation.

On the other hand, we may establish essentialist claims via a different route, such as Putnam's twin-earth thought experiments, and the intuitions to which Kripke appeals when discussing, for example, the essentiality of origin. Such arguments do not appeal to the necessity of identity or to the properties of rigid designators. Rather, they appeal to intuition, for example, the intuition that if something is XYZ rather than H_2O, then it would not be water. Such argument do not of themselves establish identities. For the intuition referred to gives us: necessarily, if S is water, then S is composed of H_2O. But it does not yield: necessarily, if S is composed of H_2O, then S is water. That would require an additional argument, and is harder to establish. (Indeed Putnam seems to imply that this is false, because he, perhaps carelessly, implies that water is a liquid, so solid or gaseous H_2O is not water.) As a result, such claims are less sensitive to the open texture that LaPorte holds to exist, even if he is correct. The open texture of 'water' may leave it open whether it designates the kind H_2O or the kind P_2O, which is itself a subkind of H_2O. But either way it will be (necessarily) true that if S is water, then S is H_2O.

Another illustration of the fact that open texture does not exclude all essentialist claims concerns the classification of animals that *clearly* fall within a clade. A newly discovered species might fall determinately within a clade, even if the boundaries of that clade are vague. So LaPorte points out that the genealogy of the giant panda puts it somewhere between the brown bear (a bear) and a racoon (a non-bear). He says that it is a matter of decision whether the bear clade should include pandas or not. Let us allow that LaPorte is right—and if one believes in any open texture at all, this would seem to be a plausible case. But that fact does not mean that it is similarly a matter of decision, undetermined by the concept *bear*, whether to classify the rare Tibetan blue bear, first considered by European naturalists in 1854. For this new discovery is a subspecies of the brown bear. So *every* precisification of 'bear' includes the Tibetan blue bear within its extension. So the Tibetan blue bear is essentially a bear, and that fact is a matter of discovery (since its ancestry is a matter of discovery).

4

Finally I will argue that LaPorte ignores a range of important cases where essentialist claims, including identities, can be established without facing the problems that he identifies. LaPorte concentrates largely on claims involving vernacular kind terms. These help LaPorte make his case, since one might suppose that open texture is going to be more likely and broader for terms whose use is established without the benefit of scientific theory. Furthermore, the fact that their use is more susceptible to influence by extra-scientific concerns makes their extensions appear subject to choice and decision.

It might appear that the traditional essentialist, for who essences and identities are discovered not stipulated, faces a dilemma. Sentences (such as identities) employing vernacular terms can be treated using LaPorte's analysis. On the other hand, sentences employing technical, scientific terms, look as of they too may be regarded as stipulations, even as analytic. Take 'mendelevium = the element with atomic number 101'. The team at Berkeley which in 1955 synthesized mendelevium by bombarding Einsteinium with alpha particles knew precisely that they were synthesizing the element with atomic number 101, and on confirming that they had done so, proposed the name mendelevium for the new element. In this case it looks very much as if 'mendelevium = the element with atomic number 101' is analytic, certainly at least that this is a stipulation.

The response to this worry is to show that this is a false dilemma. It is false because many natural kind terms are scientific, not vernacular terms, yet they were introduced in advance of the relevant discoveries that would permit making a stipulative or analytic definition of the form analogous to 'mendelevium = the element with atomic number 101'. The key point in what follows is that a certain amount of scientific knowledge is required to introduce a new natural kind term, knowing that it is indeed a kind term and knowing that it names a different kind from other kind terms similarly introduced. However, this amount of knowledge can be *less* than the amount of knowledge required to know the essences of those kinds. Hence when essence stating identities do become known, they must be known *a posteriori*.

We can articulate this point with respect to the chemical elements as follows: it was possible to know that newly introduced names for element (e.g. 'actinium', circa 1899–1904) did indeed name a distinct element. But it was not possible until later

in the 20th century to know that 'actinium = the element with atomic number 89'. Hence the latter proposition cannot be a stipulation or analytic.

Nine elements were known in ancient times. Arsenic, antimony, and zinc were identified as distinct substances in the medieval and renaissance periods. Phosphorus is the first element to be isolated by what we might think of as modern, chemical means, by Hennig Brand in 1669. The rise of modern chemistry in the eighteenth century saw the identification of several new elements: bismuth, platinum, nickel, and cobalt were identified between 1732 and 1753. The chemical revolution centered on the investigation of gases, and the discovery of hydrogen, oxygen, nitrogen, and chlorine between 1766 and 1774.

The important theoretical development was the introduction, primarily by Lavoisier in his *Traité Elémentaire de Chimie*, of the idea of a chemical element. Although the idea of an element goes back to Plato, Empedocles, and Aristotle, Lavoisier (1789: xvii) gave an account of chemical element as that which cannot be decomposed further by chemical analysis: "we associate with the name of elements, or of the principles of substances, the idea of the furthest stage to which analysis can reach." On this basis Lavoiser listed thirty-three elements rather than the traditional four of five (earth, air, fire, water, quintessence). While fire remains in Lavoisier's list as caloric, Lavoiser lists several different kinds of elemental 'airs'. Water, on the other hand, is a compound, since it decomposes in to oxygen and hydrogen, as Lavoisier showed by passing steam over heater iron. The chemical revolution thus instituted a new paradigm of normal chemical science—isolating new chemical elements.

The next theoretical advance of significance was Mendeleev's development of the periodic table. As is well known, Mendeleev noted a correlation between atomic weights and certain chemical properties which repeated in a periodic fashion. The ordering of the elements by atomic weight thus gave rise to a new chemical property, the position of an element in the periodic table, its atomic number. This number was not simply the ordinal number of the element in the list of known elements ordered by empirically measured atomic weight, since Mendeleev used the periodic property to identify gaps in the table too be filled by hitherto undiscovered elements and in some case reversed the ordering of atomic weights. It thus may appear that Mendeleev's work identifies atomic number as the crucial identifying property of elements. On the other hand, it seems clear that Mendeleev did regard atomic *weight* as fundamental, for tint he case of the reversed ordering (where the element with lower atomic number has a higher atomic weight), Mendeleev thought that the atomic weight would need to be corrected. The order *correct* atomic weights would be the order of atomic number.

So although Mendeleev introduced the notion of atomic number, this did not have a role distinct from that of atomic weight, which remained the fundamental notion. This was overturned in 1914 by Henry Moseley, who showed that there is a relationship between atomic number and X-ray frequency. The square root of the latter is proportional to nuclear charge in the Rutherford–Bohr model of the atom. Hence Moseley was able to show that atomic number is exactly equal to nuclear charge. On this basis Moseley could demonstrate that nickel's atomic number is one greater than cobalt's without having to maintain that the measured atomic weights (cobalt's atomic weight being greater than nickel's) needed correcting.

When we consider the sentence 'actinium = the element with atomic number 89', it is only once the notion of atomic number is associated with atomic charge, rather than position in a table ordered by atomic weight and periodicity of properties, that we can regard the sentence articulating the essence of actinium. For it is Moseley's

notion of atomic number that is the explanatory one, which tells us the nature of the element.

The period between Lavoisier and Mendeleev was a period when scientists were able to isolate, identify and distinguish new elements as such, but were not in position to know the truth of sentences of the form 'X = the element with atomic number N'. During that period, thirty-nine elements were discovered, in addition to the ten elements isolated during the eighteenth century before 1789.[3] To these we should add the twenty-two elements discovered after Mendeleev and before Moseley's work, since although during that period sentences of the form 'X = the element with atomic number N', we known, they did not express essential truths. Either way, we have a large number of elements discovered, which were given scientific names. That is to say, there is no question but that the names were used to name those natural kinds. Furthermore, they name exactly the same natural kinds as we refer to using the same names. There has been no conceptual change as regards these names of elements, not even precisificiation.

5

Whether any concepts have open texture is itself a debated question. However, one the assumption that there are such concepts, LaPorte makes a strong case for vernacular natural kind terms having open texture. That, he argues, leads to the truth of essence stating identities being a matter of decision and stipulation rather than discovery. In the paper I have argued for three points:

(i) There is an alternative account of the data about what we do or would say in the light of new information. LaPorte argues that the data shows that there is room for choice, which is explained by the open texture of the concepts. I respond that it is difficult to distinguish the precisification of an open textured concept from a conceptual shift (involving a re-assignment of extension). The data might equally be interpreted as the conceptual shifting of concept with little or no open texture, where the shift may be motivated by extra-scientific concerns. What seem to be instances of choices being made in varying directions can be explained by an account, (TAX), of the relationship between out concepts and paradigm instances and foils.

(ii) Even if we accept the case for open texture, that leaves many propositions concerning essential properties to be discovered *a posteriori*. Vagueness between red and orange leaves it determinate nonetheless that a ripe tomato is determinately red. Likewise while it may be indeterminate whether $K_1 = K_2$, it may be perfectly determinate that the kind K_3 is a subkind of K_1. And so it may be an essential property of K_3 that K_3s are also K_1s. Thus the concept *water* may have open texture so that it is not determinate whether D_2O is water. But that is consistent with its being determinate that all water is D_2O. It may have been indeterminate whether the giant panda is a bear, but fully determinate that the Tibetan blue bear is a bear.

(iii) The claims about open texture are most plausible concerning vernacular terms that appear to be natural kind terms. But important identity statements

[3] The numbers are to be regarded as in exact, since the discovery of elements in the eighteenth and, in some cases, in the early nineteenth centuries is a vague matter.

> in science are not just those that conjoin a vernacular kind term with a scientific one. They include those joining one scientific term with another, as in 'actinium = the element with atomic number 89'. Such statements are necessary truths (indeed, essentialist truths), but are discovered *a posteriori*. That is possible because is it possible to have knowledge of the determinate identity and difference of kinds without knowledge of the truth of such essentialist facts. In the case of the chemical elements, the techniques of laboratory analysis were sufficient for chemists to isolate, describe, and identify new elements, and so name them, before any knowledge of atomic number was introduced.

Thus I conclude, LaPorte's persuasive and informative arguments notwithstanding, that there remains considerable scope for essentialists to maintain that many essentialist truths, both those asserting essentially necessary conditions and those asserting identities, are not stipulated but are discovered *a posteriori*.

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