Putnam's Theory of Natural Kinds and Their Names is Not the Same as Kripke's

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Abstract

Philosophers have been referring to the "Kripke–Putnam" theory of natural-kind terms for over 30 years. Although there is one common starting point, the two philosophers began with different motivations and presuppositions, and developed in different ways. Putnam's publications on the topic evolved over the decades, certainly clarifying and probably modifying his analysis, while Kripke published nothing after 1980. The result is two very different theories about natural kinds and their names. Both accept that the meaning of a natural-kind term is not given by a description or defining properties, but is specified by its referents. From then on, Putnam rejected even the label, causal theory of reference, preferring to say historical, or collective. He called his own approach indexical. His account of substance identity stops short a number of objections that were later raised, such as what is called the qua problem. He came to reject the thought that water is necessarily H_2O, and to denounce the idea of metaphysical necessity that goes beyond physical necessity. Essences never had a role in his analysis; there is no sense in which he was an essentialist. He thought of hidden structures as the usual determinant of natural kinds, but always insisted that what counts as a natural kind is relative to interests. "Natural kind" itself is itself an importantly theoretical concept, he argued. The paper also notes that Putnam says a great deal about what natural kinds are, while Kripke did not. Moreover, a theory about names of natural kinds is to some extent independent of a theory of natural kinds themselves, to the extent that one can accept the one and reject the other, even when both are advanced by the same philosopher.

The brilliant series of publications by Saul Kripke and Hilary Putnam, about names of natural kinds, is one of the glories of twentieth century analytic philosophy. The two men are widely assumed to have advanced, at roughly the same time, variants of the same basic idea. Hence the first commentators on their work spoke regularly of the Putnam–Kripke or the Kripke–Putnam theory. The practice continues to this day. It is seldom noticed that the theories are rather different. They had distinct motivations and different final doctrines. I shall

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not expound the semantical theory of either philosopher in detail, or defend one against the other. I wish only to distinguish the two. Putnam (1983) and (1990) has "distanced" himself from Kripke (his verb, 1990, p. 55), but I shall be urging a greater distance than Putnam himself asserted.

It may be supererogatory to cite the commentators, since names such as "Kripke–Putnam" are widely used. For purposes of recollection only, I shall note a few authors who write that way, from 1977 to 2005. Thus Mellor (1977, p. 299) roundly attacks "the natural kind essences widely advertised by Professors Kripke ([1971], [1972]) and Putnam [1975]." But Putnam did not advertise essences at all! French (1983, p. 242) states that "Natural kind terms, according to the Putnam–Kripke theory, are rigid designators, like proper names, that pick out the same thing in every possible world." Putnam had little to say about rigid designators, and not much more about picking out the same thing in every possible world. Yes, he did invoke these ideas, with explicit acknowledgement to Kripke (Putnam 1975, p. 230–2). Nevertheless, the dominant theme in his work is what names of natural kinds refer to — that is, their reference in the world ("this" world, if you think there are others). The very careful Donnellan (1983, e.g. p. 85) also referred to the "Kripke–Putnam theory," but in the light of later work by Putnam he came to see his ideas as distinct from Kripke's (Donnellan 2003, p. 73, n. 1).

Moving on to the 21st century, Stanford and Kitcher (2000, p. 99) begin their paper saying they will address the "the Kripke–Putnam account." Soames (2002, p. vii) says that "Over the years, this characterization of natural kind terms has come to be regarded by many as axiomatic of the Kripke–Putnam view." Sharrock and Read (2002) speak of "Kripke/Putnam essentialism" (p. 182; a section-title on p. 184 is "The Kripke–Putnam essentialist story"). Mumford (2005) speaks of e.g. "the Kripke–Putnam position" and "the Kripke–Putnam argument" (both on p. 428), and section 4 is titled "Kripke–Putnam essence". A couple more 21st century mentions are cited below.

A shared negative idea

The proposals of Kripke and of Putnam did indeed have things in common. Both deny that the meaning of a name for a natural kind is primarily given by some description that applies to all and only things of that kind. They also deny a variant of that, the idea that meanings are given by clusters of properties. The

critical element in the meaning is the extension of the name. It is not wholly clear what common names refer to, as Donnellan 1983 points out. But roughly, the common noun "water" refers to water, and the common noun "tiger" refers to tigers, and these references are part of the meaning. The reference is not a consequence of the meaning, as Frege on sense and reference seems to imply. The meanings of common names for natural kinds *include* their referents, which are in the world. As Wittgenstein taught long ago, meanings are not mental, "Cut the pie any way you like, 'meanings' just ain't in the *head!*" (Putnam 1975, p. 227.)

Unlike Kripke, Putnam did offer an explicit meaning of "meaning" in his classic paper, "The Meaning of 'Meaning'," (Putnam 1975, p. 215–71) — henceforth MoM. Note that the meaning of a name for a natural kind, on Putnam's account, included more than the grammar and the referent of the term. It included a stereotype, some sort of common (if possibly mistaken) knowledge associated with the name. The extension is included as part of the meaning, but "'meaning' *never* means 'extension'" (p. 224).

Putnam may have been the first to call this approach "externalist". He summarized what he, in his (1975), and Kripke (1972) were trying *not* to do:

> 1. We were attacking the idea that speakers pick out referents in the following way: each term T is 'associated' by each speaker with a property P_T (the 'intension' of T). The term applies to whatever has the property P_T.
> 2. We were giving an alternative account of how speakers *do* pick out referents if they don't associate terms with necessary and sufficient conditions (or properties P_T) as required by, say, Russell's theory. (Putnam 1978, p. 58.)

If "Kripke–Putnam" meant simply an external, referential, approach to names of natural kinds, the present paper would not be wanted. But even my meagre list of citations shows that something far more elaborate is meant by "Kripke–Putnam". The elaborations, I suggest, are all in the direction of Kripke's proposals, and have little to do with Putnam's arguments. Putnam wrote a great deal about natural kinds and their names, and his publications evolved over at least thirty years. He sometimes flirted with Kripkean ways of writing, but matters never got much beyond the occasional bow of respect.

For the record, given (2) just quoted from Putnam, Russell did not say that natural kinds are defined by necessary and sufficient conditions. He favoured

the cluster account later developed by Gasking (1960). "A natural kind is like what in topology is called a neighbourhood, but an intensional, not an extensional, neighbourhood. Cats, for example, are like a star cluster: they are not all in one intensional place, but most of them are crowded together close to an intensional centre." (Russell 1948a, p. 461; 1948b, p. 443.) Moreover he advanced a view very like that of Quine (1969): (a) "The existence of natural kinds underlies most pre-scientific generalizations [...]." (1948a, p. 335; 1948b, p. 317.) But (b): "the doctrine of natural kinds, though useful in establishing such pre-scientific inductions as 'dogs bark' and 'cats mew', is an approximate and transitional assumption on the road to more fundamental laws of a different kind." (1948a, p. 461–2; 1948b, p. 444)

Although one may think of the classic MoM as the most sustained statement of Putnam's ideas about both natural kinds and their names, subsequent work importantly clarified and sometimes modified the doctrines presented in Volume 2 of his *Philosophical Papers* (Putnam 1975). For convenience, page references to papers printed there will be given to the collection, rather than to the first appearance, which will however be indicated by a date in [square brackets]. Later work (to say the least) enriches the ideas. In this trivial respect alone Putnam and Kripke differ, for Kripke (1980) remains definitive and unaltered, adding to but not modifying Kripke (1971, 1972).

It is always to be remembered that Kripke made plain that he did not "spell out an exact theory" of names for natural kinds (1980, p. 139). And even his discussion of proper names for individuals needs, he said, "many more refinements." "In that sense it's not a theory, but is supposed to give a better picture of what is actually going on." (p. 96.) In general Kripke was far less dogmatic than many of his later adherents; he was well aware both of the nuances of common speech, and of the complexities of nature.

Kinds and names of kinds

Both writers said a lot about *names* of natural kinds. Over the years, Putnam also wrote a good deal about *natural kinds themselves*. Kripke said much less about natural kinds than Putnam, as is appropriate in work that extends a theory of proper names to common nouns that stand for natural kinds. Kripke was doing semantics and the theory of modalities, while Putnam was doing semantics and the philosophy of the natural sciences.

In what follows I say little about what natural kinds "are". My account of Putnam's ideas about natural kinds will appear in *The Library of Living Philosophers* (Hacking, forthcoming), where he will doubtless correct what I wrote. I concluded my reading by saying that his is perhaps "*the only viable account* of natural kinds" That is the opinion of someone for whom the very idea of a natural kind has become somewhat jaded (Hacking 2007a). It does not imply that Putnam's is the only viable account of *names* of natural kinds.

One significant difference between the two philosophers is that rigid designation is integral to Kripke's approach to natural-kind terms. It was at most incidental to Putnam's, and then only at one stage in his exposition. One of the sections in MoM is headed "Indexicality and rigidity" (p. 229). Putnam steadfastly insisted that names of natural kinds are indexical. Only briefly did he deploy Kripke's idea of rigidity.

This paper is about kinds and their names, so Kripke's theory of rigid designation for proper names of individuals is not under discussion. I take for granted that it is one of the three most memorable contributions to twentieth century philosophical thinking about denotation and reference — the other two of course being Russell's "On Denoting" (1905), and Strawson's "On Referring" (1950). Likewise when we turn to essence, it is the essence of natural kinds that is in question, and I shall not discuss Kripke's resurrection of the notion of individual essence. There are parallels, of course, but for example one could maintain a doctrine of essences for kinds, but reject as incoherent the idea of individual essence. Or *vice-versa*.

Although he said less about natural kinds than Putnam, Kripke nevertheless profoundly influenced analytic thinking about them. Thanks to Kripke and to some remarks by J. L. Mackie (1974), there has arisen the idea that natural kinds have essences. Indeed John Dupré (2002, p. 106) is probably representative of current opinion when he writes that, "*Traditionally* [my emphasis] natural kinds were generally assumed to satisfy all or most of the following conditions: 1. Membership of the kind was determined by possession of an essential property or properties. [. . .]" This statement is remarkable, because the entire tradition of natural kinds, from John Stuart Mill, through John Venn, C. D. Broad, and Bertrand Russell, and on to W. V. Quine, was avowedly nominalist — often to the point of a very sparse nominalism and an explicit contempt for ideas of essence or essential properties (cf. Hacking 1991).

Interest in essence marks out a central difference between Putnam and Kripke. Putnam never allowed himself much more than "so to speak" essence, and

increasingly withdrew from, e.g., the idea that water is necessarily H_2O. In the end he wholly rejected the notion of "metaphysically" necessary *a posteriori* truths about natural kinds.

Intellectual debt

Kripke acknowledged one profound debt — to Rogers Albritton, who "called the problems of necessity and *a prioricity* in natural kinds to [Kripke's] attention, by raising the question whether we could discover that lemons were not fruits" (1980, p. 23, n. 2; cf. 122, n. 62). He mentioned Putnam as one of several philosophers who "independently expressed views with points of contact with various aspects of" *Naming and Necessity* (p. 23, n. 2). The only point of contact actually examined refers back to Putnam (1962b), which urged that the statement that cats are animals is "less necessary" than the statement that bachelors are unmarried. Cats, Putnam urged, might turn out to be robots, or, Kripke later adds, demons. One learns (we might here be supposed to imagine) that cats crossing our path don't just happen to bring bad luck, they are demons who malevolently harm us when they do so. Hence the statement about cats is not analytic. But according to Kripke, if what we took to be cats turned out to be robots (or demons), this would not show that cats are robots (or demons), but that these individuals, not being animals, are not cats. Cats (whether or not there are any) are necessarily animals. Not of logical necessity, but, if we need a name for it, of metaphysical necessity.

Putnam also acknowledged Albritton, not in connection with lemons not being fruit, but with the parallel possibility that glass bottles should turn out to be organisms (Putnam 1990, p. 55). Putnam's later work was, however, not so deeply marked by this line of questioning as was Kripke's.

Many of Putnam's characteristic ideas about natural kinds and their names had emerged before he learned about Kripke's proposals — he cites his own lectures given at Harvard 1967–1968, and then in Seattle and Minnesota (Putnam 1988, p. 130, n. 17). Although his concern with analyticity is one of his starting points, he was also spurred on by philosophy of science current in the early 1960s. Against Paul Feyerabend, but also against Norman Malcolm's *Dreaming* of 1959, he urged meaning-constancy: we may learn radical new things about acids or dreams, but we are still talking about acids and dreams. The notion, that reference is what anchors meaning, provides a theory to make sense of meaning-constancy.

Kripke, in contrast, shows little concern in print with such a starting point. He was driven on by questions of modality, and, as just quoted, "necessity and *a prioricity*." Logical positivism had got analytic philosophy into a terrible mess by in effect conflating those two ideas with analyticity, and Kripke was invaluable in reminding us of the distinctions among the three notions. (For a synoptic and slightly more historical view of how and why the confusions arose, see §5, *The Terms of Art* — that is, the terms "*a priori*", "necessary", "analytic", "inconceivable", "certainty", and "apodictic" — in Hacking 2001, p. 105–19.)

The causal-historical account of reference

Putnam and Kripke were coming from different places, but Putnam did get one thing from Kripke. He graciously acknowledged it over and over again. "Kripke's work has come to me second hand; even so, I owe him a large debt for suggesting the idea of causal chains as the mechanism of reference." (Putnam 1975, p. 198; cf. pp. 176, 246.) Kripke began his quite detailed explanation of the idea, in the case of proper names, with these words:

> Someone, let's say, a baby, is born; his parents call him by a certain name. They talk about him to their friends. Other people meet him. Through various sorts of talk the name is spread from link to link as if by a chain. (Kripke 1980, p. 91.)

Somehow this got called the *causal* theory of reference, although Kripke's most common metaphor is of links in a chain of communication. "Causal chain" appears on p. 93. When we reach natural kinds, it is "causal (historical)": "[. . .] the species-name may be passed from link to link, exactly as in the case of proper names, so that many who have seen little or no gold can still use the term. The reference is determined by a causal (historical) chain" (p. 139). In the preface specially written for the book, it is "'historical chains'" — in quotation marks — and not causal ones (p. 8, n. 9). In the appendix, we read of the "historical connection of the story with a certain substance" (p. 157). Moreover "the historical acquisition picture of naming advocated here is apparently very similar to views of Keith Donellan" (p. 164). To judge by the preface and the appendices, it looks as if Kripke switched from "causal" to "historical". The latter is surely more sensible. I shall speak of the causal-historical theory, not because I think that "causal" is appropriate or needed, but because so many philosophers have since spoken of the causal theory.

The idea was that a natural kind is given a name at some point in the past, and there is a historical connection, made by successive links of usage, between the name and the kind; that leads up to our present usage. The name refers to the kind with which it was originally associated.

Putnam himself complained early about speaking of a causal theory: "Indeed what is important about Kripke's theory is not that the use of proper names is 'causal' — what is not? — but that the use of proper names is *collective*" ([1973] 1975, p. 203). At one point he (1975, p. 274) spoke of the "'historical' theory of denotation," not a causal one. Later still he insisted on the collective rather than the causal aspect, saying he would like to call his approach not a causal theory of reference, but "the 'social co-operation plus contribution of the environment theory of the *specification* of reference" (1978, p. 58). This description de-emphasizes the historical tradition that connects an old naming — a "baptism" — with present reference. Conversely it implies that this is a theory about how references are in practice specified, rather than a theory about what reference is (whatever that would be). Kripke and Putnam are widely read as holding that the reference is entirely given ostensively, by indicating samples in the neighbourhood. That has been an impossible thought after even the most casual reading of Wittgenstein. Hence a line of criticism that has been called has been called the *qua* problem (Devitt and Sterelny 1987, p. 70; cf. 1999, p. 79). Typically a batch of samples of a substance or a plant will be examples of numerous different kinds, in addition to the intended one. John Dupré (1981) stated the difficulty early on for the case of biological kinds. Stanford and Kitcher (2000) urge that realistic contexts of scientific reference-fixing demand further sophistications are needed to preserve the motivating intuitions of the theory. Agreed, but not, I think, a surprise for either Putnam or Kripke. Indeed Putnam had implicitly cut short the *qua* objection right from the beginning. This is one reason why LaPorte (2004, p. 4–7), who introduces some interesting new criticisms of Putnam, based on a closer look at Putnam's own example of jade (p. 94–100), says that the *qua* problem is not fatal to his theory or reference. Further uses of jade can be found in Hacking (2007).

His "theory can be summarized," he wrote, "as saying that words like 'water' have an unnoticed indexical component: 'water' is stuff that bears a certain similarity relation to the water *around here*." (MoM, p. 234) This emphasizes specification rather than baptism. It is true that on the very next page Putnam did invoke ancient uses of a word, taking Archimedes, the Greek name of gold, and some gold nearby (in Syracuse, presumably), for his example (p. 235).

There is the picture of some stuff being named a long time ago, and the name, by a historical tradition, continuing as the (translated) name of that very stuff. Doubtless the mention of "local gold" for Archimedes suggests the historical tradition: "gold" refers to stuff that is the same stuff as the gold around Archimedes, which I take to be the same stuff as the small amount of gold about my person right now.

What is "the same stuff"? That is the *qua* question. But of course Putnam knew his colleague Nelson Gooman's (1970) "Seven Strictures on Similarity." In the case of water, Putnam said that water is anything that is the same liquid as the water around here. That is why he calls his theory *indexical*. But what, he asks, is this relation, 'same$_L$', (viz. 'being the same liquid as')? (p. 238.) Putnam's response is typically pragmatic. He says that one thing bears the "same liquid" relation to something else if they are both liquids, and the two "agree in important physical properties".

> Importance is an interest-relative notion. Normally the 'important' properties of a liquid or solid, etc., are the ones that are *structurally* important: the ones that specify what the liquid or solid, etc., is ultimately made out of — elementary particles, or hydrogen and oxygen, or earth, air, fire, water, or whatever – and how they are arranged or combined to produce the superficial characteristics. (MoM, p. 239)

Interests! In a slightly different context — a sample consisting of a single isotope of iron, compared to some naturally occurring sample of iron with a standard mix of isotopes — he asks, are these the same substance? "Well, it may depend on our interests. (This is the sort of talk Kripke hates!)" (1990, p. 68.) Perhaps we could separate Kripke and Putnam decisively with two words: essence for Kripke, and interest for Putnam.

Isotopes have long furnished a handy example of the *qua* problem. In a parenthesis, Putnam observes that although an ordinary example of iron is a mix of isotopes, "these occur in fixed proportions—the same proportions — in all naturally occurring samples, by the way. Some philosophers who use isotopes as examples appear not to know this." (*ibid.*) As an added parenthesis, not affecting Putnam's argument, the fixed proportionality of isotopes is most characteristic, for obvious reasons, of the large central part of the periodic table, hence iron. But this feature is not universal. Helium, for example, has long been known to have 8 isotopes, two of which are stable. But the distribution of He-3 and He-4 even on earth is very variable. Rocks from the Earth's crust have helium isotope ratios

varying by as much as a factor of ten; geologists now use this fact to determine their historical origin.

Contingent interests

MoM speaks of an interest-relative notion. What is interesting to "us"? It depends who we are. Structural similarities are what are officially ("normally") deemed to be important for educated persons in a scientific culture, especially when they profess to be discussing logic or the philosophy of the sciences. But take a name from older science, "*aqua regia*", say. That corrosive stuff is a mixture of one part nitric acid and 3 to 4 parts hydrochloric acid. According to historical lore, it was first made by Geber (the Latin name for Abu Musa Jabir ibn Hayyan c.721–c.815, a great chemical pioneer who worked mostly in Kufa south of modern Baghdad). He is thought to have discovered nitric acid, hydrochloric acid, and *aqua regia*. This last is extraordinary stuff, for thanks to a sort of chain reaction, it dissolves gold, although neither of its two component acids will do so alone. This *dispositional* property was the one that was truly important to alchemists for almost a millennium. The sequence of chemical reactions that enable *aqua regia* to dissolve gold is remarkable, and depends on chemical structures, but the unique ability to dissolve gold is an emergent property, of a sort that essentialists have not much discussed. Both acids are required to dissolve gold, in a chain reaction of some complexity.

Strict essentialists would deny that *aqua regia* is a natural kind at all, because it is not a chemical compound but a mixture whose exact proportions are not critical. But in line with what Putnam himself said about interest-relativity, a mixture, of nitric acid to 3 or 4 times as much hydrochloric acid, is the same liquid as the "royal water" that fascinated Geber and later generations of alchemists — the "same" because it agreed in the important (and unique) ability to dissolve gold, and not because of its microstructure.

Keith Donnellan's (1983) was one of the wisest early discussions of these topics. One of his many astue observations was that "although one might suppose that if terms for natural kinds are to be found anywhere the language of science would be replete with them, it is not obvious that the Kripke–Putnam theory is applicable to kind terms in science. Nor is it obvious that it will apply to terms which the vernacular has borrowed from the language of science, such as 'plutonium' or 'electron'." (p. 85.) Take a borrowing in far wider common use:

"cholesterol". When health-obsessed people natter on about cholesterol, they are hardly using the word in an indexical way, the same stuff as the cholesterol around here. In fact this stuff was named in 1815 by Michel-Eugène Chevreul (1786–1889). (Yes, he lived to be 103, "the Nestor of chemistry.") The naming is actually called a "baptism" by a major encyclopedia, the French *Universalis*. The historical links through the development of new knowledge, from then to now — including a raft of Nobel prizes — can easily be traced. So a good many elements of the causal-historical approach remain — in this case.

In fact naming, the specification of reference, and the historical links in the usage of a name, are vastly more contingent and variable than most referential theorists acknowledge. Hacking (forthcoming) illustrates this for names of diseases, starting with Putnam's own favourite, multiple sclerosis. Hacking (2007b) makes related points by expanding on Joseph LaPorte's (2004, p. 94–100) discussion of Putnam's example of jade. The full history of the word 'jade' verges on the bizarre, and nicely illustrates Kripke's observation that 'real reference can shift to another real reference' (1980, p. 163, cf. pp. 93, 96–7). Not just for proper names, which is Kripke's point, but also for the names of kinds of mineral. Although Kripke had little interest in "looking up" stories of real life contingencies, he was far better at imagining possibilities than his successors. One can usually find facts that illustrate his fictions. Unlike the work of later essentialists, Kripke's book does not strike one as doctrinaire. For example, nothing in what he wrote indicates that he would deny that *aqua regia* is a natural kind, even if it is just a mixture. But if Putnam is correct, Kripke would be loath to discuss that issue in terms of the "interests" of alchemists.

Essence

Putnam seldom dabbled in essences. The closest he got may have been in a remark about gold to which I have already alluded. When Archimedes said in Greek that something was gold, "he was not just saying that it had the superficial characteristics of gold [...]; he was saying that it had the same general *hidden structure* (the same 'essence', so to speak) as any normal piece of local gold." (1975, p. 235.) It is the italicized "hidden structure" that counts. Essence is a throw-away aside, "essence" in scare-quotes, "so-to-speak-essence." Putnam never intended a resuscitated idea of essence to do any work. He did write earlier that the presence of characteristics found in a lemon "is likely to be accounted

for by some 'essential nature' which the thing shares with other members of the natural kind." ([1970] 1975, p. 140.) Once again, "essential nature" is in scare quotes, or perhaps quotation marks indicating amusement or irony. He did take the scare quotes off in the next sentence: "What the essential nature is is not a matter of language analysis but of scientific theory construction; today we would say it was chromosome structure, in the case of lemons, and being a proton-donor, in the case of acids." (p. 140–1.)

Note that "today" in this quotation, from "Is Semantics Possible?", refers to *1970*. The folk biology of the late 1960s held that species had defining molecular hidden structures, spelled out in terms of DNA. No longer. *Citrus limon*, the stout thorny tree that bears lemons, is less well determined by its chromosomes than was once thought. Hence modest essentialists, such as Wilkerson (1995), and radical essentialists, such as Ellis (2001), alike deny that species, and in particular lemons, are natural kinds at all. (This is not to be confused with Michael Ghiselin's important thesis, motivated by evolutionary theory, that species are not natural kinds, although the term "species" itself denotes a natural kind. Ghiselin (1997) is the most systematic exposition of the doctrine.) Putnam need not accept the essentialist reasoning, because he was never an essentialist in the first place. On the other hand, he had precious little to say about species beyond what he wrote from time to time about lemons: neither systematic nor molecular biology was his forte. Note that in his last major essay on natural kinds, Putnam (1990) was explicitly concerned primarily with being the same substance, not with being the same natural kind.

Hidden structure (which he later called microstructure) is at the heart of Putnam's mature understanding of natural kinds of substances. In this respect he is curiously reminiscent of Locke. An influential paper by John Mackie (1974, cf. 1976) has muddied the waters here. Mackie urged that Locke had anticipated Kripke. He drew attention to Locke's strange expressions "real essence" and "nominal essence". These were never used before Locke. They were almost never used again before Mackie revived them. Leibniz, an essentialist if anyone has ever been, thought Locke was making fun of essence in these passages, and hated him for it. Mill, an anti-essentialist if anyone has ever been, loved Locke and cheered his "immortal Third Book." (Mill [1843] 1973, VII, p. 115. [Bk. I. ch. iii.§3]). I myself read Locke as Leibniz and Mill did, and not like Mackie, but this is not the occasion to argue the point.

Locke did say that the "real internal, but generally in Substances, unknown Constitution of Things, whereon their discoverable Qualities depend, may be

called their *Essence*." (*Essay*, III.iii.15). As Uzgalis (1988) explains, he was not seriously countenancing essences. (I think he was making fun of them.) Locke, self-styled underlabourer to the Royal Society, was at one with "those, who look on all natural Things to have a real, but unknown Constitution of their insensible Parts, from which flow those sensible Qualities, which serve us to distinguish them one from another, according as we have Occasion to rank them into sorts, under common Denominations." (*Essay* III.iii.17) That is as close to a late seventeenth century explanation of Putnam's "hidden structure" as could be. That is one reason why Galperin (1995) contends that, if Locke is a precursor of any modern natural-kind theorist, it is Putnam, not Kripke, whom he anticipates.

Note that Locke wrote of the real internal (but in his day unknown) Constitution-of-Things in *Substances*. Only much later did Whewell and Mill treat kinds of substance and kinds of living thing together as *kinds*, or *real Kinds*, namely as what came to be called natural kinds. *Phosphorus* and *horse* are Mill's paradigm real Kinds, as if they had the same logic and grammar. Locke was wiser. He expected that there is an internal constitution or hidden structure for gold and, doubtless, *aqua regia*; he did not imply that for tigers or lemons. Thus he would not have been embarrassed by retraction of the 1970 folk molecular-biology of lemons and other species. Despite his regular early use of the example of lemons, Putnam was in practice a good Lockeite.

Theory of names for natural kinds independent of theory of natural kinds?

Aqua regia? What were Locke's expectations about that corrosive stuff? He did not have the concepts of compound and mixture, so he probably expected that it had an internal constitution that accounted for its powers, in much the same way that an internal constitution accounts for the powers of ordinary water. But now we know the difference. *Aqua regia* does its stuff because of a complex interaction between gold and the two component acids. The most severe post-Kripke essentialist will judge that a mere mixture such as *aqua regia* lacks an essence and so is not a natural kind. On the other hand, the causal-historical account of the specification of reference for *aqua regia* works just fine. This shows how theories about names of natural kinds, though connected with accounts of what natural kinds are, are to some extent independent.

Thus we can well imagine that around the year 776, Geber was so impressed

with this unbelievably potent stuff, that he named it royal water. The techniques were later transferred to Europe. The name, we imagine, was translated from Arabic to Latin and along with the technology it was passed on by historical links to European alchemists. In this case, the causal-historical account of the name jibes well with the folklore about *aqua regia* and its name, even for those who would hold that *aqua regia* is not a natural kind at all.

And now consider the post-Kripke insistence that biological species are not natural kinds, because they lack essences. Wilkerson and Ellis, taken as examples of essentialists, were not much interested in semantics, and so took no sustained view on the references of names of natural kinds. But one of Ellis's followers could well hold that the causal-historical account of reference applies to names for species, even though species are not natural kinds.

New species are often named by the explorers who find them and then the names are approved by supervisory committees with a set of five or six paradigm instances before them, which are later filed in cabinets in London, Paris, or Washington. "Same insect as" means having the same important properties, exactly as Putnam said. In systematic biology it is not first of all the hidden structure that matters, but the more or less surface structure, morphology, together with functions served by the various parts of the insect. Next, what counts is placing the insect within a genealogical tree. That in turn will today be investigated by hidden structure, mitochondrial DNA and the like. There are a lot of important properties, and what is important evolves over time. But the indexical account of names for biological species seems to work rather well, whether or not you think species are natural kinds.

On the score of natural kinds themselves, we should recall one of Putnam's earlier assertions from which he has never deviated:

> Even if we *could* define 'natural kind' — say, 'a natural kind is a class which is the extension of a term *P* which plays such-and-such a methodological role in some well-confirmed theory' — the definition would obviously embody a theory of the world, at least in part. [...]; what *really* distinguishes the classes we count as natural kinds is itself a matter of (high level and very abstract) scientific investigation and not just meaning analysis. (Putnam [1970] 1975, p. 141)

High level and very abstract investigation may lead one to say that although the ability of *aqua regia* to dissolve gold is the consequence of a chain of reactions starting with the hydrochloric acid but then requiring the nitric acid, even so,

aqua regia does not play a central role in any deep current theory. So, for a non-essentialist reason, it might be concluded that *aqua regia* is not a natural kind. Likewise species may fail, on Putnam's view of what is sufficiently abstract, to count as natural kinds (not lack of essence, but something related to current science). Even so, the indexical account of names may provide a good picture of the names of many species, and also of interesting substances such as *aqua regia*.

I am arguing throughout this paper for differences between Kripke and Putnam, but I do not distinguish them in respect of *aqua regia* or biological species. Essence is profoundly important to Kripke. But I find nothing in his writing to imply that his "baptismal" account of common names for kinds does not apply to *aqua regia.* I find nothing to imply that it does not apply to names of species, even if species turn out not to have essences. To repeat, the theory of common names of kinds is to some extent independent of an account of what natural kinds are. In saying this I take no view on whether a faithful student of Kripke should hold that "*aqua regia*" or "*Felix tigris*" are rigid designators.

Necessity, possible worlds, and water

There is one place that Putnam did speak of essence without scare quotes, namely in "Possibility and Necessity" (Putnam 1983, p. 46–68, on p. 58). He was explicitly examining Kripke's doctrine and distancing himself from it. By then he had come to realize — under the heading "Problems with 'essence'," that "the claim that the statement 'Water is H_2O' is true in all possible worlds is far too strong" (p. 63). The retraction became even more explicit when he published "Is Water Necessarily H_2O?" in 1990.

Yes, he did invoke possible worlds in MoM. This is a second debt to Kripke, in addition to the causal-historical account of reference. But here the relationship was borrowing, not permanent acquisition. It was indeed a very firm borrowing at first. "Our discussion leans heavily on the work of Saul Kripke, although the conclusions were obtained independently." (MoM, p. 230.) "The term 'water' is *rigid*" (p. 231). Full steam ahead:

> Once we have discovered that water (in the actual world) is H_2O, *nothing counts as a possible world in which water isn't H_2O.* In particular, if a 'logically possible' statement is one that holds in some 'logically possible world', *it isn't logically possible that water isn't H_2O.* (p. 233)

He concluded (pp. 234–5) by "heartily" endorsing Kripke's (1971, p. 157).

It happens that the passages from Kripke that Putnam cited are about proper names, not common names of natural kinds. No matter: Putnam's pages 230–5 surely justify the appellation, "Kripke–Putnam theory", don't they? Yes they would, were they not exactly the pages that Putnam has most explicitly withdrawn.

In the course of these pages Putnam urged his celebrated Twin-Earth argument. So it might seem as if no retraction can make us delete them, because Putnam is so invested in Twin Earth. But nothing in that argument relies on possible worlds. This became clearer as the argument was repeated. Thus by his (1981, p. 23), Putnam does not refer to logically possible worlds. Instead he writes of ordinary possibilities, e.g., what "one should say, if such a planet [as Twin Earth] is ever discovered." In retrospect, Putnam said that in his original work: "Far-away planets in the actual universe were playing the very same role in my own discussion that hypothetical situations ("possible worlds") were playing in Kripke's" (1990, p. 60).

Keith Donnellan has written recently that "Putnam acknowledges, correctly, a debt to Saul Kripke's views as given in Kripke (1980). I believe, however, that Putnam's views add important details and that he is incorrect is supposing that Kripke's notion of a 'rigid designation' is the locus of his debt." (Donnellan 2003, p. 73, n. 1.) I would put the matter a little more strongly. Although in the pages just cited, Putnam heartily endorsed possible worlds, rigid designation and necessity, those pages can be deleted without affecting any position to which Putnam was himself later committed. And it is only in those pages that Putnam has recourse to rigid designation. It is not so clear to me that Putnam ever thought of rigid designation as the locus of his debt to Kripke. What he owes to Kripke is the causal-historical account of the *specification* of the reference of names for natural kinds.

First retraction

How did Putnam argue that "the claim that the statement 'Water is H_2O' is true in all possible worlds is far too strong"? In (1983, p. 63) he took the possible-worlds route: "Consider a possible world in which water exists only in the form of H_6O_3 molecules." So water in that world is not strictly H_2O, although it is an oxide of hydrogen in the ratio of 1:2.

This alleged possibility does not refute what Kripke claimed. He always made

plain that he was presenting only a picture. So a fuller picture might begin by looking up *Webster's Third New International Dictionary*. That authority states that water "when pure consists of an oxide of hydrogen H_2O or $(H_2O)_x$ in the proportion of 2 atoms of hydrogen to one atom of oxygen." Shorthand chemistry may lead possible worlds theorists to affirm that the statement "Water is H_2O" is true in all possible worlds. Longhand chemistry would present the more accurate affirmation: the statement: "Water [when pure] consists of oxides of hydrogen in the ratio of 1 : 2" is true in all possible worlds. Putnam's possible world, where the only water molecules are H_6O_3, does not look like a counterexample after all.

One should insert here an observation Putnam made in an unexpected place, buried in a footnote to a discussion of Jacques Derrida and deconstruction:

> Water, for example, is not really just H_2O: real water always contains H_4O_2, H_6O_3 ... as well as D_2O, D_4O_2, D_6O_3 ... as well as superpositions (in the quantum mechanical sense) of all of the foregoing. Suppose one had a bowl full of H_4O_2; would it be a bowl of *water*? (Putnam 1992, p. 216, n. 8.)

That of course shows that water is not necessarily H_2O, if "H_2O" is taken literally. But it does not refute Kripke. It just shows that what water *is*, is a bit more complex than what children are taught in middle school. And what it *is*, on the Kripke view, is necessarily what it is. So a more physically informed version of the doctrine of *a posteriori* necessity would state that water is necessarily a mixture of (1:2) oxides of hydrogen, including oxides of different isotopes of hydrogen, including superpositions ...

Strange possibilities

The logic of counterfactual possibilities is notoriously difficult, but when we start changing physics, we hardly know what to think. It is bad enough to imagine a world in which all water is H_6O_3, but Putnam went on to consider an even more out of the way possibility. He invited us to

> consider a world in which the laws of physics are slightly different, and in which hydrogen and oxygen do not exist *separately*. What we call 'oxygen atoms' and 'hydrogen atoms' are not stable bound states, but the whole 'H_2O molecule' is. It is clear that *water* exists in that world; but it is not

> clear that 'Water is H_2O' is true in that world (because there are no such *chemical substances* as *hydrogen* and *oxygen* in *that* world). (1983, p. 63)

Leave aside the scruple that the laws of physics would have to be more than "slightly" different. What did Putnam mean by "stable bound state"? One imagines perhaps that since our familiar gases — "chemical substances" — are H_2 and O_2, these molecules are not bound in a stable way. But there would presumably be hydrogen ions in solution? No, the instability must reach deeper down into physics than that. An isotope is called stable if it does not decay, i.e. is not radioactive. No isotope of radium is stable in this sense, so no atom of radium is stable in this sense. But we have no trouble thinking of radium as a "chemical substance".

So consider some of the so-called exotic isotopes of helium, such as helium-5, with a half-life of 7.6×10^{-22} second. Now if all hydrogen had been as exotic as that, then indeed I at any rate would hesitate to call it a "chemical substance". It would not be around long enough! But then it would be no good at forming stable water molecules either. Putnam meant something else by not having a "stable bound state." Having tried out a few possibilities, we in the end just have to grant him what he imagined without understanding it.

The argument still does not appear compelling. Consider a more ordinary kind of instability, a matter of chemistry, not physics. Phosphorus, one of Mill's paradigm real Kinds (a.k.a. natural kinds), is the twelfth most common element in the earth's crust — in innumerable compounds. But it is so unstable that it does not naturally occur free anywhere on earth. No fancy instability here — it just bursts into flame, explodes, or whatever. (Not that pure H_2 is much better, as any cryogenics engineer will tell you!)

The first crude and impure candidate for free phosphorus dates from some alchemy of about 1669. Hennig Brand distilled it from months of his own urine. Between 1670 and 1900 it became possible to produce many allotropic forms of pure phosphorus, all unstable, nearly all dangerously so. Fine, phosphorus is a chemical substance ("in our world"). But if being a chemical substance means existing as a free stable atom, then phosphorous need not have been a chemical substance.

For let us imagine a world that is far easier to comprehend than Putnam's. We do not modify chemistry or physics, but human history, technology and its products. Imagine that the Black Death was far worse than it actually was. At its peak, 1665, bubonic plague kills most Europeans. This disaster is attributed

to black magic, alchemy and what ("in our world") came to be called chemistry. Such practices become taboo, on pain of death, and no one anywhere on the face of the earth, ever, advances chemistry beyond 1665. There was no free phosphorus on earth before 1665, and had chemistry and applied technologies stopped in 1665, there would never have been free phosphorous on earth.

Human knowledge would of course be different, in this imagined history, than it is in our own history. But that is not the point. The point is that there would be no free phosphorous. That is, no free phosphorus "as a chemical substance," on an Earth with that history. It would occur only in its compounded forms.

Of course, no one in that possible history would make or deny the statement, "Human bone is rich in phosphates." There would be no such sentence to utter in that world. So there would be no such statement to be true or false. But that is not the contingency that referential philosophers in the tradition of Putnam or Kripke take into account. I doubt that there would be life as we know it in Putnam's world of exotic physics and/or chemistry, and hence no statements to be true or false, but that was not his question about what is "true in *that* world". More linguistically self-conscious philosophers might prefer to say, "true *of* that world".

Leaving such scruples aside, what is true "in" my historically possible world? Since the chemical and physical laws and facts of that world are the same as ours, it would surely be "true in that historically possible world" that healthy human bones are composed of more than 50% calcium phosphates, such as tricalcium phosphate, $Ca_3(PO_4)_2$. True even though phosphorus does not exist "as a chemical substance." Analogy suggests that that it is true that water, in Putnam's more exotic world, is composed of oxides of hydrogen, such as H_2O. True even though hydrogen and oxygen do not exist "as chemical substances." To use the usual shorthand chemistry, it is after all rather "clear that 'water is H_2O' is true in *that* world".

A replacement for necessity

In the course of this discussion (1983, p. 63), Putnam agreed with Kripke, "that science does more than discover mere correlations". Many would say that is the heart of the matter, the denial of simplistic Humism. But how do we explain this more-than-correlation? At least since the work of William Kneale (1949, p. 78) on "natural necessity", analytic philosophers have tried to formulate some notion

of necessity, short of logical necessity, to explicate the difference. Putnam offered a different type of suggestion that he has never worked out, namely that "the 'essence' [his scare quotes] that physics discovers is better thought of as a sort of *paradigm* that other applications of the concept ('water', or 'temperature') must *resemble* than as a necessary and sufficient condition good in all possible worlds." (1983, p. 64.) This suggestion should be developed. We want something more than correlations, something that is not only short of logical necessity, but which is also short of necessary and sufficient conditions.

Second retraction

It is fitting that Putnam's second retraction should have been prompted by the twentieth-century incarnation of British empiricism, namely A. J. Ayer. Ayer was an avowed if subtle Humian about causation, laws of nature, and necessity. He ended his book on the philosophy of his century (Ayer 1982) with a ringing condemnation of Kripke and Putnam. Putnam replied, in his contribution to the Ayer volume in *The Library of Living Philosophers*, pre-published as Putnam (1990). Here we learn that what I have been calling Putnam's first retraction was a response to a critique of Kripke that Ayer made at a conference in the late 1970s, and to which Putnam had responded. In retrospect Putnam called this his "minimalist" interpretation of Kripke. (See Putnam 1990, p. 54, and p. 325, notes 1 and 2.)

Much of what he urged in response to Ayer is implicit in what I have reported earlier: the emphasis on microstructure and on science providing the criteria for "being the same substance as." But his second retraction is presented in more general terms than the first, as a defence of the idea of physical necessity (explicated in terms of current physics) and a rejection of any further idea of metaphysical necessity. He thinks the whole possible worlds concept collapses as soon as one starts to ask whether an allegedly possible world obeys physical laws different from those that actually prevail. "I now think that the question, 'What is the necessary and sufficient condition for being water *in all possible worlds*?' makes no sense at all. And this means that I now reject 'metaphysical necessity'." (p. 70.)

The metaphysical necessity here denied is what Kripke most often called *a posteriori* necessity, or just necessity. Hence this is an excellent point at which to leave the differences between the two philosophers. For Kripke's last words in the

last appendix to his book (1980, p. 164) appear to be diametrically opposed to Putnam's last words of 1990. Kripke ends his book by saying: "[. . .] a good deal of what contemporary philosophy regards as mere physical necessity is actually necessary *tout court*. The question of how far this can be pushed is one I leave for further work." If we suppose that the two philosophers are talking about the same concepts, then Putnam holds fast to physical necessity as a viable concept and altogether rejects metaphysical necessity. Kripke expected that physical necessity will prove to be at best a species of (metaphysical) necessity.

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Resumo

Os filósofos têm se referido à teoria de "Kripke e Putnam" sobre os termos para espécies naturais por mais de 30 anos. Embora haja um ponto de partida comum, esses dois filósofos começaram com motivações e pressuposições diferentes, e desenvolveram o tema de diferentes maneiras. As publicações de Putnam a respeito desse assunto evoluíram com o passar das décadas, certamente esclarecendo e provavelmente modificando sua análise, ao passo que Kripke não publicou nada depois de 1980. O resultado são duas teorias muito diferentes a respeito das espécies naturais e seus nomes. Ambos aceitam que o signficado

de um termo para uma espécie natural não é dado por uma descrição ou por propriedades definidoras, mas é especificado por seus referentes. A partir daí, Putnam até mesmo rejeitou o rótulo "teoria causal da referência", preferindo dizer: teoria histórica ou coletiva. Ele chamou sua própria abordagem de indexical. Sua explicação da identidade de substâncias impede várias objeções que foram levantadas mais tarde, tais como aquilo que é chamado de o problema qua. Ele passou a rejeitar a idéia de que a água é necessariamente H_2O, e a denunciar a idéia de necessidade metafísica que vai além da necessidade física. As essências nunca desempenharam papel algum em sua análise; não há nenhum sentido em que ele seja um essencialista. Ele pensou em estruturas ocultas como o determinante usual de espécies naturais, mas sempre insistiu que o que conta como uma espécie natural é relativo a interesses. O próprio conceito "espécie natural" é sem si mesmo um conceito teórico importante, afirmou ele. Este artigo também nota que Putnam diz muita coisa acerca do que são espécies naturais, ao passo que Kripke não diz. Além do mais, uma teoria sobre nomes de espécies naturais é em certa medida independente de uma teoria das prórpias espécies naturais, uma vez que se pode aceitar uma e rejeitar a outra, mesmo quando ambas são propostas pelo mesmo filósofo.

Palavras-chave
Putnam, Kripke, espécies naturais, referência, essência, necessidade.