Reference and Incommensurability:
What Rigid Designation Won't Get You

Abstract: *Causal theories of reference in the philosophy of language and philosophy of science have suggested that it could resolve lingering worries about incommensurability between theoretical claims in different paradigms, to borrow Kuhn's terms. If we co-refer throughout different paradigms, then the problems of incommensurability are greatly diminished, according to causal theorists. I argue that assuring ourselves of that sort of constancy of reference will require comparable sorts of cross-paradigm affinities, and thus provides us with no special relief on this problem. Suggestions on how to think about rigid designation across paradigms are included.*

Keywords: reference, rigid designation, Kuhn, Putnam, incommesnurability

1. Rigidity as a Solution to Incommensurability

For better or worse, there is an impermanence to our language and theories that presents us with a substantial set of problems regarding the univocality of theoretical expressions such as natural kind terms. Whether by the enormous "revolutions" in scientific practice that Kuhn (1996a) describes, or by the dribs and drabs of dutiful research, change is pervasive in any rigorous form of empirical inquiry and discontinuities invariably arise between what we think and mean by our words now and what our predecessors did. For instance, ancient alchemists and early modern chemists took water to be a simple substance or "principle"; we do not. Classical physicists right up to the early years of the 20th century took time and length to be absolute concepts; we do not. The most serious consequence of such incommensurability is a disconnect between speakers whose utterances ostensibly bear some sort of rational import upon one another, as when we and others speak of "water" and purport to mean the same thing. Purports may not translate into successes, of course, and part of doing philosophy is disabusing ourselves of our naiveties, but the failure to salvage some sort of commonality here would be devastating to any sense of linguistic, conceptual or theoretical continuity that we might hope to have.

Many philosophers see promise in turning our attention to reference and rigid designation, and some even take this as a solution to problems of incommensurability. In the roughest of terms, so long as the stuff we are referring to remains constant, we can say incompatible but intelligible things about it to one another. Kripke and Putnam's work suggested that the reference of proper names and natural kind terms is not determined by the other concepts or "descriptions" associated with them, but rather via some fixing of their designation by direct appeal to the objects present in the real world at their introduction. Subsequent speakers inherit this reference-fixing via anaphoric "chains of communication" with other speakers, rather than by possession of some set of reference-fixing descriptions. What we happen to associate with a proper name or kind term at the time of its introduction or subsequently would not thus be known true of the referent *a priori*, nor would it be immune from well-motivated revision. We may take Shakespeare to be the author of *Hamlet*, but subsequent investigation might show him to be a fraud. Likewise, our ancestors took water to be simple, but we have discovered its microstructure by examining samples of it. Although our theories differ greatly, our reference may remain constant throughout and thus, putatively, the discontinuity and incommensurability are eliminated.

Perhaps the strongest formulation of this view that I have read comes from Ian Hacking, who said:

> Putnam saved us from such questions [about holism and incommensurability] by inventing a referential model of meaning…Our initial guesses may be jejune or inept, and not every naming of an invisible thing or stuff pans out. But when it does, and we frame better and better ideas, then Putnam says that, although the stereotype changes, we refer to the same kind of thing or stuff all along. We and Dalton alike spoke about

> the same stuff when we spoke of (inorganic) acids. J.J. Thomson, H.A. Lorentz, Bohr and Millikan were, with their different theories and observations, speculating about the same kind of thing, the electron. (Hacking, 1982: 75)

Hacking's view on this is certainly a rather strong break from much thinking on natural kind terms. It would seem to imply a reference relation completely unmediated by any conceptual resources (he has denied that scientists need theories at all), which would be an even more radical break with past thinking than Putnam was making. Most philosophers, even those who favor causal theories of reference, would hesitate to follow him. For instance, Devitt and Sterelny (1999: Chs. 4 and 5, esp. pp. 79-81) argue that some cluster of descriptions is associated with a natural kind to form an open sentence. Devitt and Sterelney's account lets description indicate underlying causes shared by members of a sample S that satisfies that open sentence (what Kripke called the "cluster of descriptions"). Whatever *as a matter of fact* causes us to form that open sentence is the referent of our natural kind term. Stanford and Kitcher (2000) worry that this account presumes a capacity of the initial introducers of a term to pick out parts of the total cause of such sets of observable properties, which is just what those introducers cannot yet do and which causal theories were supposed to free them from need of doing. Instead, they opt for a piecemeal – but still primarily causal – account of the reference of natural kind terms that incorporates a range of samples, a range of foils, an open sentence invoking the range of observable properties associated with the kind and a condition that speakers refer to that thing which is a common constituent to the total causes of all the samples and none of the foils. By these means, they hope to avoid the "qua" problem of natural kind terms, i.e. how speakers with limited theoretical resources may fix upon an object that they cannot yet describe, and both show considerably more sophistication than the rough account offered by Hacking above.

However, one thing that does carry over from Hacking's position to the more subtle recent versions of causal theories of reference is the expectation that constancy of reference will alleviate incommensurability. Though those descriptions and other forms of conceptual mediation may change over time, we should have no worries (or few worries) that we refer to the same kinds over time. Since the same kind is causing our usage and revisions of that usage, we co-refer with past-speakers and can reject, adopt or at least consider the claims made by speakers who adopted radically different sorts of theories about those kinds. Thus, "water" came to imply very different things after Lavoisier, but both we and pre-Lavoisian speakers refer to the same item and therefore there is no contradiction or ambiguity in taking their utterances and ours to bear on one another. In this way, more recent causal theories of reference retain Kripke and Putnam's plans of fixing a referent through a baptism and letting the theoretical chips fall where they may. While confusion might abound in reading what some past speakers have said, natural kind terms should enjoy an enviable constancy of meaning and avoid the worst pitfalls of incommensurability. A brief caveat is also in order here. As Soames (2002) notes, even a direct reference theorist might acknowledge that extension (and hence, the meaning) of non-natural general terms may shift over time. For now, I will not contest this point, as my primary concern here is with natural kind terms.

I will argue that even if we can reasonably presume co-reference between speakers in purportedly incommensurable stages of theoretical development, the most serious problems linger. This is because such situations involve both semantic and epistemic issues that cannot be resolved independently. To see this more clearly, we must differentiate two dimensions of incommensurability first, recognizing the sort of problem each one presents. A brief caveat is also in order here. As Soames (2002) notes, even a direct reference theorist might acknowledge that extension (and hence, the meaning) of non-natural general terms may shift over time. For now, I will not contest this point, as my primary concern here is with natural kind terms.

2.Two Problems

Let us begin with a slightly rhetorical question: does incommensurability really matter? If the theories we endorse at present succeed in describing, predicting, and explaining the world (or however we choose to articulate our notion of success), then so what if our ancestors had different views and meanings for cognate terms? So much the worse for them and so much the better for us. People used to do all kinds of stupid things and not all of them need be reconciled with the sets of practices, concepts and terms with which we go on from here. The hint of realism here in saying that our theories "succeed" is not a necessary condition upon such an attitude, either; one might adopt a gleeful social constructivism and write off such discrepancies as having only passing historical interest to us. (Richard Rorty sometimes adopts such a tone.)

Such indifference to historical incommensurabilities of usage is not uncommon outside the scope of the natural sciences and for non-natural kind terms. For instance, take the English word "coroner" and its usage in the language. By the 12th Century, perhaps as early as the 10th, English kings appointed local officers as guardians of pleas on the crown's behalf. They were officers of the crown (hence the shared root with a word like "coronation") who kept records and performed other legal functions, including the criminal prosecution and the collection of taxes. Of particular importance in this respect were executed criminals and victims of violent crime, whose property reverted to the crown upon their deaths. Only later did they acquire more general responsibilities for recording deaths, which had traditionally been the province of religious officials, and only later than that, as Normans feared increasing violence from their Saxon and Celtic subjects, did they become responsible for officially determining the causes of all deaths. The first hint of such duties does not come until 1194, and then only in limited fashion, as they took responsibility for determining whether a death was by suicide.[1] Thus, a term in our usage bears little resemblance to its initial incarnations, but this is of little interest to anyone but medieval historians and philologists.

As a first pass at this point, let me suggest that such semantic discontinuities or divergences vary in their significance based upon the significance of the standards they play a role in making explicit. The discontinuity with 12th century English speakers is of little significance to us because, to state the obvious, we are not under the jurisdiction of 12th century English kings, laws or other practices. We have explicitly abandoned many of them as a community while adopting others, and 12th century kings have no causal powers to enforce them against our collective will, what with their being dead and all. Such practices may echo through our own in certain ways, but not so clearly as to grant such details any direct import in our judgments and actions. If incommensurability matters, it must be because the claims we make in the course of the natural sciences bear upon one another in a way that the claims in my admittedly selective example do not.

Difficulties in squaring our claims with past speakers have a semantic dimension to them and an epistemic one. We generally draw our understanding of the problem of incommensurability from Kuhn's (1996a) formulation or Feyerabend's (1962 and 1975). Both authors saw this as initially a semantic problem, and Kuhn (1990 and 1996b) continued this theme in opposition to causal theories of reference. Of particular importance in the original formulations of this problem was the absence of any sort of formal procedure for the translation of one community's set of theoretical expressions to another's. Incommensurability implied that there was no common measure of disparate sets of speakers' terms and thus no purely logical means of bringing one set of claims to bear on the other. Neither author denied that we might make *some* kind of sense of other communities of speakers, only that this process was more akin to an interpretation of the spirit of one community's terms in another's, as we find in translating literary works. (See especially Kuhn, 1990; 299-301.) To some degree, causal theories of reference implicitly reject this very assumption, since their appeal to the causal as a full-blooded partner in semantics would establish just the "common measure" needed. The epistemic dimension I describe below is thus often neglected in causal theories of reference. Also, there are those who would argue that in addition to the epistemic and semantic incommensurabilities I describe here, there are ontological incommensurabilities with which we must contend.[2] For now, I will take no stance on this matter. My only concern here is with showing the inadequacy of a particular strategy, and as we shall see, we have enough incommensurability to do that already.

What I will call the semantic problem here involves an inability to secure a sufficient commonality with other speakers on ways in which we should proceed in speaking the language. Such difficulties are often idealized or constructed by those conducting historical reviews, as with our earlier comparison with 12th century English, but acute versions of the semantic problem may also be among speakers who meet one another in the flesh. Imagine any physics department right after relativity took hold, or any geology department as plate tectonics caught on and the fights between old and new guard that would have ensued. Much of the contention surrounding this sort of semantic incommensurability arises from views of meaning that stress holism and define the meaning of a claim as its role in a network or web of theoretical claims. With different webs, a one-to-one mapping becomes impossible and speakers cannot be said to converse with one another where any discrepancies can be found. In practice, such breakdowns are probably the exception rather than the rule, since repeated interaction with other speakers tends to force revision if conversation is to be continued, thereby normalizing usage. Breakdown may still lurk in less-frequently visited parts of the web though, as well as between larger groups of speakers, e.g. local differences among members of different research programs. We may say, borrowing Wittgenstein's metaphor, that those who face the semantic problem are simply playing different games, with no larger framework available for them to appeal to in resolving such discrepancies, thus making play impossible.

The epistemic problem in cases of incommensurability involves the suspension of epistemic authority to make one's claims and employ one's methods based on the evidential support of other speakers, with a concomitant suspension of one's authority to challenge others' claims and methods. If I cannot take the terms of another's claims to be those in my own, then those claims cannot support mine and my evidential and theoretical claims pose no challenge to theirs, either. Similar problems may emerge for large parts of our methodologies, so I am using the term "epistemic" broadly here to include authority over suitable methods and models as well. Obviously, this failure will be symmetrical, since other speakers are in the same position; neither of us can support the other and neither of us can challenge the other. This matters to us for the obvious reason that discovering such incommensurability may cut us off from warrant for our claims and methods, which may undermine us in any number of ways.

Given these remarks, one might be led to conclude that the epistemic problem was a consequence of the semantic one, i.e. we have problems with justification only because our concepts and terms are shifting beneath our efforts to test and deploy them. We should resist a simplistic reading of this relation though, because when theory-building functions properly, such shifts are prompted by the failures of our theoretical terms to live up to their roles in satisfying justificatory demands, rather than some sort of prior semantic fickleness. Changes to the meaning of terms present us with the epistemic problem, but those changes in meaning reflect our ongoing evaluation of the successes of our models and theories. It will not do to "fix" either one of these, or to think of langauge and empirical inquiry operating independently of one another when incommensurability arises; these two problems go hand in hand and we should not misinterpret one as the sole cause of the other. As inquiry proceeds, the language changes, but as the language changes, it jeopardizes the support needed to conduct the inquiry. Rather than one bringing the other about, they each constitute one dimension of the difficulties that discontinuity creates for us. As we adjust the usage of our terms in accordance with ongoing empirical inquiry, the mistaken assumptions of past speakers become evident, but their usage remains ossified after their demise. They cannot make corrections along with us and the unfolding character of our theories complicates the process of interpreting what they said. But the social, discursive nature of empirical inquiry builds on stores of concepts and evidence accumulated by past speakers, which introduces a need to lean on their efforts as well. Some who recognize the epistemic problem here opt to solve it by accepting the semantic problem and historicizing virtually everything that we do. This is to recognize incommensurability and accept that the apparent fissures between communities of speakers cannot be remedied, as historicists and social constructivists would suggest. Conversely, the causal accounts of rigid designation outlined above assert that there is a continuity to the reference of our theoretical terms, but offer no means by which we would evaluate and interpret the epistemic significance of claims made by speakers whose conceptual commitments are incommensurable with our own. This might connect us with past speakers, but with the meager result that our commonality would not license us to take one another's claims as evidential, or even true or false. To say that we are talking about the same stuff is slim comfort if the things we can say about it continue to have no bearing on one another.

I contend that we should consider any attempt to address one of these problems without addressing the other an unsuitable response to the larger problem of incommensurability. My concern is that the interpretation of rigid designation I mentioned above offers a semantic solution without an epistemic one, or at least purports to do so. Even in cases where co-reference between disparate speaker communities obtains, license to infer based on others' claims may not be established, and this robs us of the epistemic purport of those claims. This makes co-reference idle unless it is accompanied by a fuller articulation of the epistemic weight of discrepancies between such speakers and an evaluation of the epistemic weight of claims by disparate communities to present ones.[3] Co-reference does nothing for the problem without being part of a larger process of establishing broader sorts of affinity with other speaker communities, and to establish this affinity, co-reference be shown, not simply presumed. Being "causally guided" by a real natural kind, if such things are possible, would not solve the problem of incommensurability.

Given the concerns I am raising, the reader may wonder whether I would endorse a bifactorial theory of meaning along the lines first proposed by Colin McGinn (1982). While I am sympathetic to such theories' efforts to balance the intentions of speakers with their environments, I have my reservations as well. A suitable account of the inferential roles of concepts and expressions deployed in the intentions of speakers will draw on both speakers' practical engagement with their environments and with other speakers. Thus genuinely narrow content strikes me as theoretical idealization at best. In this respect, we need a more effective articulation of how the cognitive aspects of meaning might be reconciled with an account of reference that emerges from the inferential role of natural kind terms and other rigid designators without resorting to an appeal to reference-fixing content, as suggested by descriptivism. (Such an account would be extensive and far outstrip our present purposes; readers may want to look at my (2006) for more on this.) We face the problem that our articulations of the inferential roles of our expressions at different historical points will not readily commensurate with one another and appeal to a causal chain linking back to the introduction of such terms will not suffice to reconcile speakers who can and do communicate with one another. In these senses, both bifactorial and more austerely direct theories of reference will leave us without a clear resolution.

These concerns are coupled together for natural kind terms – and other sorts of theoretical terms for that matter – by virtue of the role they play in the sorts of practices in which they play it. In an enterprise that purports to describe, explain and predict the natural world, as the natural sciences do, terms and concepts earn their place by facilitating those tasks, and we retain them only to the extent that we expect them to pay dividends in the process. With that purport to offer an account of the world that swings free of our attitudes about it, to whatever extent that is possible, comes a powerful epistemic burden. The license to adopt a standard of usage for a community no longer lies solely with the community, even if the realizations of such standards are always socially articulated phenomena. Purport to establish such standards for usage – to establish meanings for our words – is thus always a provisional action, since subsequent investigation may reveal our mistakes or shortcomings. We put past communities of speakers on the spot when their theories fall short, but we put ourselves in a comparable position with respect to future generations at the same time.

The contrast with the "coroner" example above should be clear. Coroners in the present day are not derelict in their duties for failing to answer to the crown or seize the property of the executed, nor were 12th century coroners unqualified for their ignorance of most of forensic pathology, simply because they are not held to the same sorts of standards by our respective sets of social practices. Semantic discontinuities are not a concern because there is no common set of epistemic and practical demands placed on the two communities that would force a crisis. At their most extreme, these are simply matters of assent. But for natural kind terms, the discontinuities do matter, for we take the standards of usage to be matters that ultimately have a status independent of our attitudes, commitments and practices, even if these are the media in which such standards are formed and articulated. The interpretation of rigid designation that I have described purports to solve all of this, though. (I should emphasize in passing that Hacking's reading probably fits Kripke's view better than Putnam's.) By appealing directly to the causes of observable properties (difficult as that may be to cash out, as Stanford and Kitcher acknowledge), their semantic contribution can remain constant throughout and we may simply make decisions about which theoretical claims are permissible by looking to the referents of such terms and basing our judgments on what we observe. It is my contention that this strategy does not stand up, however. We must either tie up the semantic with the epistemic in ways that undercut the simple picture of reference given here or else let reference float so freely of what we actually know and believe that it ceases to have any role in the language we actually speak.

I would assert that reference is too slim a semantic notion here. We may co-refer with others and still fail in various respects that are germane to the problems at hand. In part, this reflects my suspicions about the suggestion that natural kind terms are "all denotation." This approach neglects the various ways in which those terms play a role in the articulation of models and theories, without which they would be bereft of any significance and within which they carry the sorts of entanglements that the views I criticize here had hoped to avoid. Taking natural kind terms to be mere semantic pushpins neglects the varieties of implications and inferential roles they have in different models at different stages of the historical development of theories, and thus neglects the critical differences in what we actually say by our claims at various points and the epistemic work of empirical inquiry that goes along with such claims. A focus on co-reference, even where it can be reasonably asserted, presents us with only a part of the larger semantic picture and attempts to pass it off as sufficient for our needs. The role of such linguistic items in epistemically charged practices should leave us expecting more.

3. A Brief History of Water

General intuitions are all well and good here, but how might they bear on actual cases of changes in theories and concomitant changes in the meanings of natural kind terms? A number of cases have already been offered by Kuhn, particularly in response to the positivists' claims that physics had not really changed at all after Einstein, but rather shown Newton's laws to be special cases or restrictions of relativistic mechanics. (Kuhn, 1962: 101-2) His treatment of "mass" is a prime example. Consider a natural kind term like "water," a frequent example in discussions of rigid designation. Here, co-reference with our ancestors seems likely into antiquity, while its incorporation into chemical theories is a more curious and complex matter.[4]

While it is fair to say that we refer to the same stuff in the world when we use the term "water" that our ancestors (or at least the vast majority of them) did, we might reasonably ask what assures us of this when we interpret their utterances and inscriptions. We assume a broad regularity in nature, and I am certainly not suggesting that at some point mischievous forces beyond our observation filled our lakes and streams with XYZ rather than H_2O. (If that much is true, we're all in some sort of philosophical trouble.) But what assures us that they were picking out just the same bits of the world by their utterances and inscriptions? It is not enough to say "the stuff in lakes and streams," hoping that the same mode of access will be available to all parties, because it remains to be asked what relation of similarity is being evoked between such samples and those we find elsewhere. We might be picking out rocks and fish unless there is some proto-theoretical notion of a liquid involved, and that much ties the ordinary modes of usage and access to theoretical inquiry. Even Putnam (1975: 225) has suggested this much and acknowledged that theoretical mediation is needed to articulate this point. As one would suspect, very different things have been said invoking the term "water."

Before Lavoisier, Scheele and Priestly isolated oxygen and subsequently found that water could be created by burning hydrogen in oxygen, most European chemists had taken water to be a simple substance or "principle," similar to our notion of an atomic element in some ways but radically different in others. (Historically, terms that refer to water appear outside the Western tradition, but these are the historical roots of our current concept of water as H_2O and thus a more manageable point of focus for what I will address here. Further stories could be told, where needed.) That had its roots in the various strands of the tradition of alchemy, though not necessarily its most occult elements, inherited by European scientists and developed into the earliest forms of modern chemistry during the 16^{th} and 17^{th} centuries. As a hypothetical move, assuming the simplicity of water is pretty respectable. It does not decompose under conditions we could normally observe, and its abundance in most natural settings is complemented by the regularity of its observable and functional properties. One can make a variety of solutions with it, but one generally cannot take it apart, or at least could not do so over most of the course of history. Worse assumptions have been made.

The abundance of water on Earth and its centrality to so much of human praxis is probably part of the implicit basis for assuming that our ancestors could select it as a referent and that we might inherit that reference through a chain of communication. Unlike gravitational singularities and xenon hexafluoride, there's a lot of it about and we do lots of stuff with it. What animated the development of chemical theories in the 18th century and what changed so much in virtue of Lavoisier's discoveries and methods was the tension between two different sorts of models and two radically divergent conceptions of what it was to be water (or anything else for that matter). At stake during the time was whether chemistry should proceed with models of chemical phenomena incorporating only minute particles and motion, or whether a richer array of particles and forces of attraction and repulsion should be deployed. (Westfall, 1977: 65-81) The more Spartan mechanistic models had parsimony and affinity with Newtonian physics on their side, but at a cost of great shortcomings in their ostensible explanatory goals. The more lush models put forth by everyone from Paracelsus to Stahl in the 17th and 18th centuries captured a great deal more of the experimental and practical knowledge of metallurgists and apothecaries and the sense that there were distinctly chemical properties involved in the mixtures of things, but at a cost of including lots of forces that belonged essentially to the primordial substances ("principles" as it is generally translated). The broad interest in Stahl's models beginning in the early 18th century should be attributed in no small part to a growing sense among chemists that the mechanical models of the recent past were anemic explanatory and predictive tools and that a great deal of what was distinctive about chemistry as a science was lost in them for the sake of more austere models. (Gough, 1988: 22-9) At its worst, this tendency led to the most spurious excesses of alchemy, offering wordplay where explanation was wanted, though these functional notions did retain a great deal of the transformative and manipulative knowledge that first drew all those metallurgists and apothecaries to the emerging field in the first place.[5]

That functional character of the models that was waning by the late 18th century and slipped out of the models after Lavoisier emphasized that part of what made water water was its capacity to act as a solvent. Kuhn (1990) actually picks this point up and suggests that by cognates of "water," our ancestors of this time did not mean H_2O, but more something like "*liquid* water." Thus, steam and ice would not have been water at all. (More on this below.) In most models from Paracelsus to Stahl, principles were understood in two sets of terms: their nature as elements and their nature as instruments. In other words, principles were understood with a certain ontological simplicity; they were the simplest things in the world, from which aggregations and mixtures were made, but which were not themselves aggregations or mixtures, at least at the level of their smallest parts. But they also carried a measure of the teleology of Greek thought, much like Scholastic Aristotelianism and most forms of alchemy, and part of what characterized the stuff that was water was its naturally progressing towards its telos, that of making solutions of things when it could actualize that potential. (Bensaude-Vincent and Stengers, 1996: 57-61) A crucial part of the emergence of chemistry in this period was its resistance to mechanistic models (though there were those who pursued such models) and ardent insistence on the irreducibility of such instrumental features, which served as grounds for the autonomy of chemistry from physics. Such features were therefore not *awaiting* explanation from other parts of the model, as we would suggest today. We would gladly say that water acts as a solvent, but would characterize this as a consequence of other sets of properties, rather than an unexplained explainer in our models.

Thus, those models ascribed properties to water that are not simply not attributed in our models (e.g. we do not think that water has a positive valence, though other molecules do) but a property that simply does not fit our models at all. We do not have teleological properties of these sorts in our models, though we work to construct something like an image or analogue of them with the properties that we do have. In the case of water, this is fairly readily done. Being a liquid and being a solvent are captured by our models in ways that we take to be sufficiently similar to prior discourse to suggest we speak of the same things and understand those features without the teleological commitments of past usage. In part, this would require a consideration of the methods used to extract or isolate the substance in question, which would suggest to us by appeal to our own models that *aqua fortis* was nitric acid or that mephitic air was carbon dioxide. (In the case of water, it is perhaps telling that there were no such extraction methods – just get a bucket and head to a lake or stream.)

Not all chemical terms that span this history enjoy such straightforward continuity, though. "Salt" for instance (in its broader theoretical sense, not just table salt) came into parlance during the 17th and 18th centuries, or at least it is a cognate of those that did, and included in their extension many of the compounds we would call salts today. However, a key feature of this concept at the time was that salts were soluble in water. (Bensaude-Vincent and Stengers, 1996: 53) Thus, some compounds we now call salts, such as sodium chloride, would be salts in those terms, but others such as calcium carbonate and calcium phosphate would not, due to their insolubility. By the 17th and early 18th centuries, chemists had come to see the affinities between soluble salts and Boyle notably offered a three-part categorization of known salts according to the properties they exhibited as solutions (or "liquors"). (Newman and Principe, 2002: 275-81) So this is no flat-footed assumption, but it remains a case in which we have a very clear break with past usage, based on its presumption of a set of essential properties. But even here, where one part of the concept is simply replaced by its negation, the break is neither arbitrary nor complete. As the new chemistry took hold after Lavoisier and the models of these substances suggested microstructural features, affinities in those microstructural features came to have greater salience than the observable property of solubility; the new version of the concept *grew* out of the older one. In doing so, an incommensurability was created between the different stages of our theories. We cannot interpret past claims about salts by a simple recursive substitution-of-terms procedure, and grasping what was being said is not a purely logical matter, as Kuhn suggested. Though we can make sense of what is being said, doing so requires a more complex hermeneutical approach, with some provisional exceptions and inferential licenses (such as teleological properties) and a goal of understanding how the most salient implications of their claims can be approximated in our own. If we decide that we share the term "salt" with them, it is in a limited and provisional sense, with extensive commentary following the license to accept any of the claims that they make.

Having made these claims about the diachronic evaluation of co-reference among disparate linguistic communities, let me return to a claim that Kuhn (1990) made. (Much of what I say here could be said of disparate contemporaneous communities, but I will focus on the historical details for now.) In criticizing Putnam, Kuhn asserted that the reference of "water" had not been so uniform over the course of history as Putnam's example would suppose. Prior to Lavoisier, a sharp divide had been posited between the chemical and physical properties of substances, and hence it was unimaginable that the same substance should exist in both liquid and gaseous forms; hence, "water" had a reference more akin to "liquid water" as we would use the term. Some might take issue with this as a historical claim, and it would be a substantial (no pun intended) project to establish this as a thesis uniformly held across the less programmatic periods of early chemistry, when so little was held uniformly at all. But let us stipulate it for the time being to consider the ramifications it might have for rigidity and co-reference. Assuming there is such a disparity between contemporary and earlier uses of the term "water" and its cognates, we are left to explain this disparity and determine the success or failure of our efforts to co-refer. Three possibilities present themselves to us: (1) we say that they dub the same stuff with two names and wrongly believe they do not co-refer; (2) we say that they successfully dub only one thing - liquid water - and have a false belief about the existence of an extra thing ("steam" or "ice"); (3) they falsely believed in an additional thing simply called "water" (or a cognate) that was not actually water at all, and hence our words bear no semantic relation to one another, despite their etymology.

Of these three options, (3) is the least plausible. It posits the most radical sort of disparity between speakers from our perspective when we must also commit ourselves to the presence of such a substance in both their era and ours (even if they do not posit it) and sets of practices that bear substantial affinity to our own both at the pre-theoretical level and in the conduct of experimental inquiry. There are surely cases where we should not take this stance towards past speakers, e.g. the "humours" in medieval medical and psychological accounts are not proto-versions of elements in the endocrine system or neuroscience. However, such cases are the exception; while we are not committed *de jure* to a charitable reading of others' assertions, we are at least committed to it by default.

This leaves us with (1) and (2). Option (2) should only be considered in those cases in which theoretical kinds are successfully eliminated by subsequent inquiry, not simply discovered to be identical, and this will be the case only when something about the original posits of the two kinds is so disparate that discovery actually renders one of them moot and useless. Cases in which two supposed kinds or entities turn out to be one are familiar from the scientific and philosophical literature – Hesperus and Phosphorus are the same object, summer finches and winter finches turn out to be the same species – but cases in which one kind term genuinely supplants and ejects another would seem rare. We should be prepared to say something to the effect of, "Oh, we thought that there were 'P's, but it turns out that they were really 'Q's." Some might argue that "water" is just such a case, though. If there were introductions of kind terms for particular sorts of gases, including "steam," and particular sorts of liquids, including "water," and "water" and its cognates had a longer theoretical history, then we could point to the introduction of steam as a separate theoretical commitment and a superfluous addition upon subsequent inquiry. I do not believe one can make such an historical case, though. Some correlate of "steam" would seem to have as long a history in human parlance as "water" and no theory of reference, causal or otherwise, can reasonably hope to produce an actual genealogy of the anaphoric chain tracing back to the first introduction of such common terms. But more importantly, this would commit us to the implausible claim that our ancestors would have failed in baptizing something when we take it to be present and the introduction of such a kind term would invoke only minimal theoretical commitments that we can reasonably assume they could make (e.g. that it is a gas, that it exists only at certain temperatures, etc.). This would make the most reasonable case to make one that says our ancestors dubbed the same thing twice (as with the planet Venus), but were not aware of the identity.

If so, we are left with (1), which allows us to make the case that our ancestors' use of "water" generally bears sufficient affinity to ours that we may take their terms as ancestors of our own and attribute to them errant beliefs about such substances and their relation to one another. This may sound like a victory for the Kripke/Putnam/causal approach to rigid designation – there's dubbing and identities get discovered – but we should remember that these are very general conclusions about most of the people using a term within related sets of practices over comparatively short periods of time. Our determination for particular claims made by historical sources (or for disparate contemporaneous speakers) of co-reference must still be a matter of ongoing scrutiny of these sorts of affinities between speakers before they can have the epistemic benefit of licensing some further claims we might hope to make on their basis. "Causal guidance" offers us no help unless the conceptual resources that almost all theorists now concede must play a role are constant, and they clearly are not, even in short slices of our own history. Rigidity is thus a *goal* of speakers who use cognate terms, not a *given* supporting that usage, and co-reference and affinity between speakers must be earned, not presumed.

4. Conclusion: Inference and Reference

An example such as "water," in which it is most likely that we co-refer but we find ourselves unready to take the claims of such speakers as license for our own, has the pointed purpose of suggesting that sharing the reference of one's terms with another is not yet grounds for claims of epistemic weight or successful communication with another speaker, which are the issues of real concern behind incommensurability. What should strike us in considering historical examples of this sort is the difficulty of determining whether terms in those theories actually do co-refer with our own, as has been presumed. Serious consideration of the question suggests that even in cases of theories that evolve from one another, as we take the natural sciences to have done, the reference of their terms remains a matter that we determine only by the exercise of their claims and methods. If so, then the focus on reference suggested by Hacking offers us no great advantage. Reference is as much in need of determination as any other semantic feature, and even if we convince ourselves of successful co-reference, semantic and epistemic problems remain. The key point to draw here is that the status of these terms as natural kind terms implicitly commits us to incorporating them into a comprehensive explanatory account of the natural world, and as such into some sort of theory. With this comes a series of roles within such theories because theories are not simply pictures of the world, but rather engines of inference that facilitate explanations and other practical ends. Picking out the same things with our terms thus does not suffice as an account of their role and meaning, since so much more is at work here.

Thus, our thesis here is that constancy of reference is not enough to guarantee successful communication among speakers, because successful communication requires establishing semantic affinity that will permit rational accord and epistemic licenses between speakers' claims. Constancy of co-reference is not sufficient for these purposes. Two communities of speakers might share a co-referring term, yet that term might have inferential roles disparate enough in those two speaker communities that one cannot take the other at face value and assess its utterances and inscriptions as it would its own. There may also be cases in which changes in the inferential roles of natural kind terms not only create such incommensurability, but actually shift the reference of the term as well. In these cases, the constancy of reference can be only partial - *most* of the things in the extension of their term are in the extension of ours. We might still hold that at least this weaker sort of constancy of reference over time is a necessary condition for communication among speakers (or at least an adequate overlap in the purported extension of an expression with some degree of expansion or contraction). If those communities have entirely different extensions and inferential roles without any actual overlap, then any appearance that they are making commensurable claims about the world and communicating *should* go up in smoke. However, this constancy will not suffice to resolve the issues surrounding incommensuarability, as those issues are inextricably tied to the inferential roles of natural kind terms.

This move to reemphasize the inferential role of natural kind terms is not without complications when it comes to the extension of such terms. Indeed, the spirit of the argument I have tried to make here suggests that no simple solution will present itself when it comes to incommensurable inferential roles or ambiguities about the reference of natural kind terms. Such issues are invariably specific to the terms in question and any resolution of them is conditioned by the actual historical development of theories surrounding and incorporating such terms. The importance of these shifts in the inferential roles of natural kind terms should not be underestimated; they often indicate or precipitate shifts in the extension of those terms that complicate interpretation between groups of speakers who might take themselves to be speaking univocally. "Jade," which designated both jadeite and nephrite, is one actual historical example and "hepatitis," which designated both hepatitis A and hepatits B as we now recognize them, is another.[6]

It is in these cases that our contribution must be seen as corrective. To avoid the pitfalls of descriptivism, our commitment to any particular articulation of an inferential role must be provisional and revisable. We strive to capture the extensions of real kinds by inferentially articulating our understanding of them in theoretical terms and a crucial aspect of this articulation is the open, revisable character of our commitments. Disjunctive cases like jadeite/nephrite and hepatitis A/hepatitis B amount to cases in which we revise the inferential role of these expressions in keeping with our original intent to establish theoretical terms that determined natural kinds as their extensions. In the hepatitis case (and many similar ones) we effectively introduce two kind terms subsumed by the original. The original may also be abandoned, at least as a natural kind term; thus, "jade" does not figure in geology or chemistry, though "jadeite" and "nephrite" do. "Jade" refers to a set of things with diverse microstructures when it was purported, as a natural kind term, to pick out a set of things with a common microstructure. We may retain that general term for various purposes, but its status as a natural kind term comes into question. That change in status may not trouble some communities or subcultures, e.g. jewelers, but its usage detaches from the sort of empirical inquiry conducted for natural kind terms and the fixing of its refernce comes to resemble traditional descriptivism more closely, with long-established criteria of identity imposed and preserved by those communities. Once empirical inquiry demonstrates the heterogeneity of that term's extension, we face a choice in how to interpret the commitments of past communities, as I suggested in section 3. In some cases, natural kind terms will be pruned from our theories (if not our languages); in others (such as Putnam's H_2O/XYZ example), their extensions will be revised. The sense in which we are guided by a kind necessarily involves such theoretical articulation and cross-community assessments of affinity, and this includes cases in which we subdivide or amputate our conception of what we took to be natural kinds.

Does this plunge us back into a picture of radical incommensurability? Not necessarily. While something like the Aristotelian notion of force may never be reconciled with classical or contemporary physics, closer cousins may still bear interesting fruit by comparative analysis. While "mass" as it occurs in the claims and models of classical physics no longer appears in exactly the same ways in relativistic mechanics, there continues to be a concept called "mass" that relates to momentum and velocity, energy and the speed of light and a host of other similarly familiar parts of the theory. Such overlaps are not mere homophony; the explanatory roles of those concepts in those theories bear significant similarities to one another even if the complete theoretical matrices into which they are embedded cannot be transposed onto one another. With the most proximate historical antecedents of a concept, the differences are generally slight enough to make such a move rationally intelligible based on judgments of affinity, if not purely logical grounds. For larger chasms in our theory-building history, it may be necessary to adopt a more broadly hermeneutic approach to the interpretation of theoretical claims – imagining oneself as a Newtonian or a Ptolemaic astronomer, with all the concomitant commitments – in order to search for affinities with one's present theories. The same may be said of cases in which both the inferential role of a natural kind terms and its extension shift. This weaker sort of constancy, mentioned above, coupled with different inferential roles may indeed be too weak to permit communication across communities. Often, however, affinity in the explanatory roles of those terms and the degree of overlap in their respective extensions (or what they are taken to be by their respective communities) provides sufficient affinity to make communication possible and even the historical perspective for later speakers to take a corrective stance towards their ancestors. None of this is to deny the Kuhnian point that there may be cases in which the differences between communities of speakers might be so great as to prevent productive theoretical discourse. It may very well happen. My comments here are only to emphasize that we are not left with a dichotomy between theoretical terms that are purely referential and a holism about their meaning that cripples any hope of rational discourse.

For the sake of expediency, I have focused on examples in which we can plausibly presume co-reference, but one consequence that emerges from the semantic and epistemic problems I have described is that such confidence must be earned rather than presumed. A "referential model of meaning" as it was described must establish the facts of co-reference by various speakers, and this remains an acutely challenging task even when looking backwards to practices and communities that strongly resemble our own. That task is by turns theoretical, historical and hermeneutical, for the reference of our terms is as much a matter to be determined by inquiry as their implications. If so, appeal to reference offers us no quick and easy route out of incommensurability.

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NOTES

[1] One can glean much of what I am saying here from a dictionary entry, but a more thorough treatment of the subject is offered in Sachs (2002). It's also worth remembering that coroners remain political officials, not medical doctors; medical examiners perform autopsies, not coroners, despite common confusion.

[2] My thanks to an anonymous reviewer for reminding me of this point.

[3] Such incommensurabilities could be synchronic as well as diachronic during transition periods, e.g. the sorts of discrepancies between Stahlian and early post-Stahlian chemists described later in this paper.

[4] My work on this section in particular was helped by conversations with Mary Domski and Terry Winant. Any deficiencies are of course my problem, not theirs.

[5] Churchland (1981) suggests this emphasis on functional properties as the definitive feature of alchemical approaches to chemistry, though he does so in the course of deriding any model that would do so as unscientific for failing to reduce to more purely physical theories. This serves as an analogy in deriding functionalism in the philosophy of mind, a cornerstone of his early work on eliminativism.

[6] My thanks to an anonymous reviewer for suggesting the second example here.