**Title**

**“**Thomas Kuhn and the Causal Theory of Reference”

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**Abstract**

It is typically held that Thomas Kuhn was committed to a descriptivist view of the meaning of theoretical terms, and that his most infamous thesis – incommensurability – was a consequence of this. The causal theory of reference supposedly rules out incommensurability by allowing the extension of a term, rather than merely the intension, to (at least partly) constitute the meaning of the term, thereby ensuring that part of the ‘meaning’ remains constant across theory changes. It is therefore surprising to find Kuhn endorsing aspects of the causal theory in several later essays while still maintaining the possibility of incommensurability. This paper will investigate how Kuhn understood both the causal theory and incommensurability, such that his endorsement of both was not the bald-faced contradiction it would be according to the standard reading. In fact, many of the affinities of Kuhn’s view with the causal theory are part of what make incommensurability possible, or so I will argue. More generally, I will suggest that Kuhn should be thought of as rejecting the very idea that the meaning of scientific terms is some aggregate of extension, and intension or sense.

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**Introduction**

It is typically held that Thomas Kuhn was committed to a broadly descriptivist view of meaning, and that his most infamous thesis – incommensurability – was a consequence of this.[[1]](#footnote-1) On such a reading, the meaning of a theoretical term is wholly constituted by the definition used, and its connections to other theoretical terms. If the definition changes while the same string of symbols remains in use, the earlier and later term in no way talk about the same thing, making the theories in which they figure are incommensurable. However, causal or externalist theories of reference, supposedly rule out incommensurability by allowing the extension of a term, rather than merely the intension to (at least partly) constitute the meaning of the term, ensuring that a part of ‘meaning’ remains constant across theory changes. Some readers of Kuhn, such as Howard Sankey, have noted a development of Kuhn’s incommensurability thesis as presented in *Structure* into what has been termed ‘taxonomic incommensurability’. While noting that it avoids some of the more unpalatable consequences imputed to Kuhn’s view in *Structure*, Sankey and others still take ‘taxonomic incommensurability’ to presuppose descriptivism, and to be completely neutralized by the causal theory.

Given all this, it is surprising to find Kuhn endorsing aspects and limited applications of the causal theory, while still maintaining the possibility of incommensurability. This paper will investigate how Kuhn understood both such that this was not the bald-faced contradiction it would be according to the standard reading. In fact, I will argue, many of the affinities of Kuhn’s view with the causal theory are part of what make incommensurability possible. More generally, I will suggest that Kuhn should be thought of as rejecting the very idea that the meaning of scientific terms is some aggregate of extension, and intention or sense. In sorting out these issues I also hope to show that Kuhn’s later taxonomic development of incommensurability was not the ‘watering-down’ of his earlier ideas that many have taken it to be. Incommensurability as it appears in the later Kuhn is a development of what was always a ‘local’ rather than ‘a global thesis. Moreover, it is a development that does not abandon practical considerations in *Structure* for a merely semantic point.

I will also examine two other essays that sympathetically discuss Kuhn on these issues, one by Wes Sharrock and Rupert Read, the other by Jouni-Matti Kuukkanen. While these two papers get much of Kuhn’s view correct, I believe both have related shortcomings. Namely, both present Kuhn as rejecting a causal theory of meaning for theoretical terms in favour of a Fregean descriptivist account. Such a reading fails to do justice to several places where Kuhn disclaims just such an account and fails to see the residual affinities within Kuhns view to the causal theory. Ultimately, I will argue that Kuhn should not be read as a partisan of ither the descriptive or causal theory, nor as offering a hybrid account – rather both those accounts should be seen as abstractions from the meaning a scientific term possesses in a thriving paradigm as it is extended from its paradigmatic applications in certain problem solutions to the solutions of new puzzles and problems. This means that understanding the vocabularies employed in past scientific paradigms is neither a matter of grasping the senses underlying these terms nor identifying their referents, but a matter recognizing the problems for the solution of which the taxonomic structure of the respective lexicon was developed and the various contextual factors that shaped these lexicons.[[2]](#footnote-2) Incommensurability in the later Kuhn is thus still very much concerned with scientific practice, and is not merely a semantic thesis. Appreciating this point also helps to see why incommensurability is a distinct phenomenon from that of the creation of new lexicons via speciation of scientific disciplines.

1. **Incommensurability and the Causal Theory**

First, I will quickly recap the argument for how the causal theory is usually supposed to inoculate against incommensurability. This argument shows up in the years following the publication of *Structure* and the appearance of causal theories of reference. However, it continued to be extremely prominent over the next several decades. A key feature of this argument is that it presupposes a ‘global’ understanding of incommensurability, something Kuhn himself explicitly disavowed in later writings. In subsection 1.2. I will explain how Kuhn himself described incommensurability in these later writings, and why he saw it as a ‘local’, rather than a ‘global’ thesis. Some authors have argued that even Kuhn’s later version still falls prey to objections from a causal theory of reference, so I will examine one such author’s argument for this in 1.2.

* 1. **The Original Reading of Kuhn on Incommensurability**

Because, the argument goes, Kuhn takes the meanings of scientific terms to be determined by the contexts of the theories in which they figure, he is some sort of holistic descriptivist for whom the meaning of any one term will be a whole bunch of descriptions to which the original term is related in a given theory. So, if the same word shows up in two different theories it will be embedded in different wholes and connected to a network of different descriptions, hence it will have two different meanings. Because, for Kuhn there is no theory neutral observation language, there are also, according to the standard reading of Kuhn, no common observations against which the theories may be compared or measured. That is to say, they are incommensurable. John Michael and Miles MacLeod nicely summarize and endorse just such a reading of Kuhn on incommensurability as follows: “Descriptive theories … are central to the incommensurability claims of Feyerabend (1962) and Kuhn (1970). Indeed, Kuhn and Feyerabend assume a strong and, above all, holistic reading of descriptive theories since they maintain that the meaning of a term is dependent on the entire theoretical structure in which it occurs. As a result, theory change leads to meaning change and, thus, also to the failure of the terms of the old theory to refer” (Miles and MacLeod 2013, 214-215).

As I will detail below, there are many places where Kuhn disclaims just such a reading of himself or says things that simply don’t fit with it. Nevertheless, it is something like the dominant reading of Kuhn on incommensurability. It was, I think, certainly something like what Arthur I Miller had in mind when he wrote that “Clearly the descriptive theory of meaning is the proper vehicle for incommensurable paradigm shifts” (Miller 1991, 101).

The causal theory of reference though, promises to identify a component of meaning that can be held fixed apart from changes in belief or the descriptions used in defining a term. According to Kripke and Putnam, an initial definition can serve to ostensively define a kind while still being false. That initial description can later be revised and replaced with the essence of the kind referred to all along. The initial description was just a way of ‘so-calling’ the stuff we wanted to begin referring to, so that we could study it and discover what it essentially was. Here, is one summary from Michael Levin, of how this kind of approach is supposed to avoid the possibility of incommensurability: “Not only can I succeed in referring to an object by a description the object does not satisfy, but you can use my description to the same object-even while believing the attribution is incorrect. This suggests the introduction of the ‘so-calling’ operator. … The significance of the so-calling operator for the analysis of scientific change is this. I have just shown how two speakers can talk about the same thing even if they share no sub-theoretical observation vocabulary” (Levin 1979, 420).

Levin in his description, has foremost in his mind Donnellan’s work on the causal theory, but Miller states the same moral with respect to Putnam’s work specifically: “Putnam renders natural-kind terms trans-theoretical by lifting them out of theories. Fixing the reference of a term by an introducing act enables the term to be used in different theories because as more characteristics or stereotypes of a natural kind accrue each component of meaning becomes enlarged, while reference remains fixed” (Miller 1991, 102). In short, what causal theories of reference give us is a way of individuating kinds and entities independently of theories and beliefs about them. Scientific theories can be badly wrong about something, and yet still be speaking about the things they are badly wrong about. We have thereby gained a theory neutral Archimedean point by which to evaluate the transition from any one scientific theory to another, contra Kuhn’s claims.

On this reading of Kuhn, it should then come as a great surprise to read him declaring that “I take this analysis of reference to be a great advance, and I also share the intuition of its authors that a similar analysis should apply to the naming of natural kinds” (Kuhn 2000b, 198-199). Or to find Kuhn saying that he [saw] “in the causal theory of reference a significant technique for tracing the continuities between successive theories and, simultaneously, for revealing the differences between them” (204). Kuhn did not give up on incommensurability as a possibility and historical actuality, so how could that be possible given his praise for the causal theory? Turning to Kuhn’s later development and clarifications of incommensurability will help to indicate how this is possible. In doing so, I will also have to disagree with some existing readings in the secondary literature regarding Kuhn’s later writings on incommensurability and their relation both to Kuhn’s views in *Structure* and to the causal theory of reference.

* 1. **Kuhn’s Later development of Incommensurability**

In the decades following the publication of *Structure,* Kuhn did a great deal to clarify his position and, I believe, showed the original reception to have rested on misreadings. For Kuhn, incommensurability was not a global property of theories or languages that barred all potential communication or comparison. Instead, it was a local phenomenon, affecting particular kinds of terms: “Insofar as incommensurability was a claim about language, a claim about meaning change, its local form is my original version. … The terms that preserve their meanings across a theory change provide a sufficient basis for the discussion of differences and for comparisons relevant to theory choice. They even provide … a basis from which the meanings of the incommensurable terms can be explored” (Kuhn 2000a, 36).

This situation of local incommensurability was also one that could be overcome to achieve communication and comparison, albeit not by what many philosophers had called ‘translation’. On such an understanding of ‘translation’, “the language into which the translation is cast existed before the translation was begun. The fact of translation has not, that is, changed the meanings of words or phrases. … The translation consists exclusively of words and phrases that replace (not necessarily one-for-one) words and phrases in the original” (Kuhn 2000a, 38). Incommensurability is the impossibility of this sort of translation. But there is another sort of activity, integral to the actual practice of translation, which Kuhn calls ‘interpretation’:” [I]f the interpreter succeeds, what he or she has in the first instance done is learn a new language, ... or perhaps an earlier version of the interpreter's own language. … Acquiring a new language is not the same as translating from it into one's own” (38-39).

Although interpretation will not produce a logically coherent language in which the theories may be chosen between via a neutral decision procedure, there is in no in principle bar to practitioners of one theory coming to understand the other, or to any subsequent comparison. Moreover, incommensurability as a phenomenon that interests Kuhn is restricted to particular kinds of untranslatable terms: “kind terms … terms that refer to the sorts of objects, materials, situations or properties which could occur in the natural or social world” (Kuhn 2022, 175). Incommensurability is supposed to apply especially to natural kind terms – one of the paradigm instances of the causal theory of reference.

Even if one comes to understand two incommensurable theories, one will not be able to bring them to bear simultaneously. That is because one set of kinds that will embody one set of generalizations and expectations, while a different set will embody different generalizations and expectations. With kind terms

the expectations they embody are therefore projectible. Some of those expectations are different. … If a referent lay in the overlap region … it would be subject to two incompatible natural laws. … Kind-terms supply the categories prerequisite to description of and generalization about the world. If two communities differ in their conceptual vocabularies, their members will describe the world differently and make different generalizations about it. Sometimes such differences can be resolved by importing the concepts of one into the conceptual vocabulary of the other. But if the terms are to be imported are kind-terms that overlap kind-terms already in place, no importation is possible. (Kuhn 1993, 319)

This does not rule out any possibility of comparison whatsoever, but it does rule out a choice between the theories based solely upon a comparison of both with one and the same observational evidence. **“**If one cannot state two competing beliefs … in the same language, then one cannot compare them directly with observational evidence. That should not suggest that there are no good reasons why, over time, only one of them survives. Nor should it suggest that those reasons do not rest most fundamentally on observation. But it should suggest that the standard conception of a *choice* [Kuhn’s italics] between the two on the basis of observational evidence cannot be quite right. Comparison requires simultaneous access to the things being compared, and that is here barred by the no overlap principle” (Kuhn 2022*,* 259).

In a number of works, Howard Sankey has also discussed these developments in Kuhn’s conception of incommensurability.[[3]](#footnote-3) Here is Sankey’s summary of Kuhn’s later position, which Sankey terms “taxonomic incommensurability”: “[T]hree general points emerge as basic to Kuhn’s position. First, direct comparison of theories requires their formulation in a common language. … Second no such common language is available. … Third, exact translation between the languages of theories. … Thus in clarifying incommensurability, the issue of translation failure between theories becomes the dominant theme” (1997a, 27).

In a later paper, Sankey also explains translation failure between incommensurable paradigms as arising from incompatible kind sets and the no-overlap principle. Sankey writes,

Change of taxonomy may involve the redistribution of members among previously existing categories and modification of classificatory criteria, as well as the introduction of entirely new categories … Such taxonomic change has an impact at the semantic level … taxonomic change may induce change in the meaning of the preserved terms, which may include variation of reference. Where new categories are proposed, new terms may be introduced that differ semantically from previously employed terms. … A term cannot be translated from one lexicon into another if its extension includes items belonging to distinct kinds within the rival taxonomy, since that would violate the no-overlap principle. (2018, 78)

There are two related elements of Sankey’s reading of Kuhn’s “taxonomic” development of incommensurability with which I will take issue in this paper. First, Sankey thinks that although the later taxonomic version of incommensurability is more “plausible” than Kuhn’s original version, it still falls prey to objections based upon a causal theory of reference. In the same 2018 paper, entitled “The Demise of the Incommensurability Thesis”, Sankey writes, “to the extent that reference may be determined independently of description, as it is according to the causal theory, it need not vary with change in the descriptions given by theories in the entities of their domain …change of descriptive content of theories need not be accompanied by wholesale discontinuity of reference” (73). Kuhn, on Sankey’s reading, presupposed some form of descriptive theory of reference both early and late in his career. But all Kuhn has shown, Sankey believes, is that terms in incommensurable paradigms may differ in meaning or sense, while still overlapping in reference. Comparison of the sort Kuhn thought impossible, is therefore perfectly possible according to Sankey.

From the supposed fact that the terms of incommensurable theories differ in meaning, it does not follow that the content of such theories is unable to be compared. If the theories are applied to the same domain of entities, then, to the extent that the terms refer to anything, the terms will either co-refer or else overlap with respect to extension. Because of co-reference and extensional overlap, the two theories are able to enter into logical relations with respect to content. More specifically, claims made by one theory may agree or disagree with claims made by the other theory. As a result, the theories may be compared with respect to what they say about the world. (2018, 85)

It is as equally surprising on Sankey’s account as it is on those in the previous section to read Kuhn’s positive comments on the causal theory of reference. Why, if Kuhn both early and late presupposed a descriptivist theory would he have taken the causal theory to “be a great advance, and … share[d] the intuition of its authors that a similar analysis should apply to the naming of natural kinds”? Why would he have agreed with Richard Boyd in seeing in that theory “a significant technique for tracing the continuities between successive theories and, simultaneously, for revealing the differences between them”?

The second point of disagreement I have with Sankey, is that I believe he exaggerates the differences between the early and later Kuhn. That is, Sankey effectively agrees with the criticisms that dominated the initial reception of *Structure.* Although Kuhn said of incommensurability, “its local form is my original version”, Sankey contends that “the thesis of local incommensurability was neither developed in detail nor clearly evident in Kuhn’s original discussion” (34). Consequently, “while [the local version] may very well have been what he originally intended, it is not what he originally conveyed” (note 19, 40). Noting Kuhn’s claimed original intentions, but not finding any evidence of the view, Sankey then simply imputes the view to *Structure* that many of Kuhn’s original critics did – namely that of “‘world change’ and wholesale change of reference” (32). I believe that this too is a misreading. Where Sankey sees a gross disconnect between Kuhn’s early and later conceptions of incommensurability, I instead find a continued refinement of what was always a local thesis.

The grounds for my disagreement with Sankey on these two points are interrelated. Firstly, Kuhn’s own examples in *Structure* simply make no sense on a global understanding of incommensurability. Secondly, there are parts of *Structure* where Kuhn rejects key aspects of a descriptivist theory of meaning for reasons that are echoed in his later sympathies toward the causal theory. My aim is not to suggest that Kuhn sought to provide a theory of meaning in *Structure,* rather Kuhn sought to describe scientific practice and thereby provide constraints upon possible theories of meaning.[[4]](#footnote-4) My claim is not that Kuhn tacitly subscribed to the causal theory, as a correct theory of meaning either of all terms or just scientific terms. Rather, my point is that the workings of language detailed by Kuhn tell against a descriptivist theory, and thereby reveal sympathies to aspects of the causal theory. I believe that even in *Structure* the constraints provided by Kuhn’s descriptions of scientific practice are such as to show a descriptivist theory to be a fundamentally wrongheaded approach. If this is right, asking whether Kuhn was a descriptivist or a causal theorist is the wrong question to ask. Rather, Kuhn’s primary aim was a description of scientific practice. Descriptivism and the causal theory should then be seen as objects of comparisons for Kuhn’s descriptions of the uses of terms by practicing scientists. What I will argue is that there are far more similarities between Kuhn’s descriptions of scientific practice and the causal theory than the secondary literature has recognized. Moreover, there is also a long-running anti-descriptivist thread in Kuhn’s writings that I hope to bring out.

In the next section I examine Kuhn’s discussion of the causal theory in several of his papers, to show that Kuhn’s relationship to that view is a good deal more nuanced than his original critics and subsequent interpreters, like Sankey, have noticed.

1. **Kuhn on the causal theory of reference**

Let us now look at Kuhn’s descriptions of scientific practice and how he saw these as bearing on the causal theory of meaning. Looking at these**,** one finds Kuhn saying many favorable things about it, while doubting its application in full generality. Although, “many of the advances achieved with its aid are likely to prove permanent” (Kuhn 1990, 309) Kuhn thinks its extension from proper names to kind terms will not always prove successful. The theory, Kuhn thinks, fares quite well with ‘gold’:

Excluding proper names, I doubt that there is any set of terms for which causal theory works precisely; but it comes very close to doing so for terms like 'gold'. … Terms that behave like 'gold' ordinarily refer to naturally occurring, widely distributed, functionally significant, and easily recognized substances. Such terms occur in the languages of most or all cultures, retain their original use over time, and refer throughout to the same sorts of samples. There is little problem about translating them, for they occupy closely equivalent positions in all lexicons. 'Gold' is among the closest approximations we have to an item in a neutral, mind-independent observation vocabulary. (Kuhn 1990, 302)

Gold is a special case, Kuhn thinks, because although we refined our standards for detecting gold, we never did anything that amounted to a ‘redubbing’, because gold remained widely available for ostension from people without special equipment, spread across various cultures. That is, prior to the discovery of its chemical composition, it had already come to approximate “an item in a neutral, mind-independent observation vocabulary”.

Part of the real value Kuhn saw in the causal theory was in preserving reference to individuals across major changes in the kind terms into which they were divided. As for instance in the shift from the Ancient Greek concept of a planet to a Copernican one: [the] causal theory provided … a wonderfully persuasive way to trace the individual bodies we call Earth, Moon, Mercury, Venus, Sun, Saturn, and Jupiter through the conceptual upheaval known as the Copernican revolution, but it could not do the same for the kind *planet* [Kuhn’s italics]. What it is to be a planet is simply not the same before and after Copernicus” (Kuhn 2022, note 1, 194).

Here, it is important that the individual bodies were observable or isolable independently of both the earlier and later theoretical framework. That is not to say that these bodies are observable or isolable completely independently of *any* [Kuhn’s italics] theories or concepts or beliefs about how the world is. Gold is a kind term that fits the causal theory reasonably well because, prior to the discovery of its chemical composition, it had already come to approximate “an item in a neutral, mind-independent observation vocabulary”.

It may be tempting at this point to think that Kuhn was only sympathetic to a causal theory as applied to proper names for individuals, in which case these remarks of Kuhn’s would have little import for understanding Kuhn’s views of kind terms in the natural sciences. But that would be an overly hasty, all-or-nothing response. It would be hasty because it would ignore Kuhn’s saying that a “similar analysis” to the causal theory should apply to scientific terms, as well as failing to acknowledge Kuhn’s discussions of the causal theory as applied to terms in the sciences. It would be too ‘all-or-nothing’ a response in that it would assume that if the causal theory was not a perfect fit for the semantics of scientific terms, nothing could be learned by comparison from it, and that the correct account would be exhausted by a descriptivist alternative. I will argue below that the kind terms that where the causal theory fits less well, it does so not because a descriptivist theory fits better, but because of a phenomenon Kuhn calls ‘re-dubbing’. This phenomenon concerns the both the difficulty of initially establishing referential ties and the potential instability of referential ties once they are established. It is to this phenomenon that I now turn.

In general, kind terms, unlike proper names or a term “in a neutral, mind-independent observation vocabulary”, require multiple acts of ostension and this brings with it a risk of redubbing: “In the case of proper names, a single act of ostension suffices to fix the reference. … But, if I were to exhibit to you the deflected needle of galvanometer, telling you that the cause of the deflection was called ‘electric charge’, you would need more than good memory to apply the term correctly in a thunderstorm or to the cause of the heating of electric blanket. Where natural kind terms are at issue, a number of acts of ostension are required” (Kuhn 2000b, 200).

Moreover, these multiple ostensions do not take place one at a time, but will in general involve (implicitly or explicitly) a network of other theoretical terms: “For terms like ‘electric charge’, the role of multiple ostensions is difficult to make out, for laws and theories also enter into establishment of reference. … Galvanometer needles may be deflected by gravity or by a bar magnet as well as by electric charge. In all these areas, establishing the referent of a natural kind term requires exposure not only to varied members but also to membership of others” (Kuhn 2000b, 200). ‘Dubbing’ for Kuhn, is always a locally holistic act that involves the simultaneous establishment of either contrast classes, or of other related kind terms. What matters in establishing reference the references of a locally holistic network of terms is not having grasped the correct, or mature, theory underlying those terms but “exposure”.

As Kuhn brings out with his discussion of the example of ‘water’ this redubbing affects not just the theoretical terms, but the terms for the supposedly ‘superficial’ properties too. This brings Kuhn to suggest a very different response to the ‘twin Earth’ scenario than Putnam did. In Putnam’s telling, Twin Earth is a distant planet in the actual world that happens to be a duplicate of Earth, but for the fact that its lakes, rivers, and faucets contain not H2O but some other compound with a very, long formula abbreviated to XYZ. The inhabitants, of Twin Earth, happen to also apply a word that’s spelt and sounds identical to the English word ‘water’ on Earth. If we were to discover such a planet, or when we consider its possibility, we should, according to Putnam, say that the Twin Earth word ‘water’ does not have the same meaning as our word ‘water’. Because our word means ‘H2O’, and theirs ‘XYZ’. Kuhn suggests a very different response: “Within the lexicon of modern chemistry, a world containing both our Earth and Putnam's Twin Earth is lexically possible, but the composite statement that describes it is necessarily false. Only with a differently structured lexicon, one shaped to describe a very different sort of world, could one, without contradiction, describe the behavior of 'XYZ' at all, and in that lexicon 'H20' might no longer refer to what we call 'water'” (Kuhn 1990, 310).

On Kuhn’s view, the dubbing and redubbing of the kind ‘water’ did not have the consequence of fixing the term ‘water’ to the property of being ‘H2O’ no matter how other things are with the world and its supposedly superficial properties. It all had the effect, rather, of building up a network of connections between chemical theory and an observational vocabulary featuring pre-theoretic terms and other, earlier scientific terms. H2O is not anything like a proper name for Kuhn but encodes a great deal of connection to observational and experimental results. To suspend this rich a network of connections between the molecular level and the observable world is to undermine the principles by which we identify and track what we call ‘hydrogen’ and ‘oxygen’. Kuhn is not denying that ostensive definition plays no role in science, but he is claiming that the theoretical terms of a given science cannot be introduced one-by-one, or once and for all, in that way. For that reason, unlike with proper names, we do not get criteria free identification when considering counterfactual possibilities.[[5]](#footnote-5)

There are other important differences, Kuhn thinks, between ‘water’ and ‘gold’. Opinions as to what counted as samples of gold, did not change significantly, according to Kuhn, with the discovery of its chemical structure.

‘H2O' picks out samples not only of water but also of ice and steam. H2O can exist in all three states of aggregation-solid, liquid, and gaseous-and it is therefore not the same as water, at least not as picked out by the term 'water' in 1750 … Water, in particular, was an elementary body of which liquidity was an essential property. For some chemists the term 'water' referred to the generic liquid … before. Not until the 1780s, in an episode long known as the "Chemical Revolution," was the taxonomy of chemistry transformed so that a chemical species might exist in all three states of aggregation. Thereafter, the distinction between solids, liquids, and gases became physical, not chemical. The discovery that liquid water was a compound of two gaseous substances, hydrogen and oxygen, was an integral part of that larger transformation and could not have been made without it. (Kuhn 1990, 311-312)

Differences concerning samples of gold after the discovery of its chemical composition, in Kuhn’s telling, concerned mostly impurities. What was discovered was one property that we could call upon to explain the superficial properties of gold we had previously used to identify it, and also to explain those properties. “What remains special about 'gold' is simply that, unlike 'water', only one of the underlying properties recognized by modern science-having atomic number 79-need be called upon to pick out members of the sample to which the term has continued through history to refer. 'Gold'” (312).

With ‘water’ however a revolutionary change occurred, and it came to inhabit a different structured lexicon than previously, rather than just an enriched one. ‘Water’ thus underwent a ‘redubbing’, whereas ‘gold’ did not. Kuhn then, contrary to the usual story is not denying outright that the causal theory or an externalist semantics is a completely false picture of scientific terms. Instead, he expects scientific terms to vary more or less from proper names along two dimensions: the other terms connected to it, and the length of time over which the meaning established by a dubbing remains stable. Contrary then to the authors cited in section one, it is not sufficient to rule out incommensurability simply by bringing in the causal theory. Rather, for each potential case of incommensurability Kuhn cites, it would have to also be shown that a redubbing did not occur in the shift from earlier to later paradigm. In Kuhn’s view, a change from one structured lexicon to another will be evidence that a redubbing has occurred.

In the next section I note how some other authors have interpreted Kuhn’s discussion of the causal theory of reference. The conclusion others have drawn is that Kuhn was ultimately criticizing the causal theory so as to defend some form of descriptivism. I hope to show why that is not the right conclusion to draw.

1. **Kuhn’s Meaning**
   1. **Other Commentators**

That Kuhn provided thoughtful responses to the causal theory of meaning and its supposedly fatal consequences for his view has not gone unnoticed by others. Rupert Read and Wes Sharrock’s article “Thomas Kuhn’s Misunderstood Relation to Kripke-Putnam Essentialism”, argues that Kuhn “decisively undermines” the causal theory (Read and Sharrock 2002, 151). The result of Kuhn’s “devastating attack” is, according to Read and Sharrock, “to defend and re-specify the notion of incommensurability against the idea that it is reference, not meaning/use, that is overwhelmingly important”. Although there is much I agree with in their exposition, I think this conclusion has two major shortcomings. One, it seems incompatible with Kuhn’s statement that, even given his criticisms, he still saw “in the causal theory of reference a significant technique for tracing the continuities between successive theories and, simultaneously, for revealing the differences between them” (Kuhn 2000b, 204). Even if Kuhn felt that the causal theory did not perfectly fit actual kind terms, he did after all state that he thought “a similar analysis should apply”. Two, it also suggests that Kuhn is accepting something like the Fregean framework of a distinction between sense (or “meaning/use”), on the one hand, and reference, on the other, and arguing that the former is “overwhelmingly important”, in contrast to the latter. But that is to miss, I believe, a large part of what is novel and exciting in Kuhn’s view.

Another, more recent treatment of Kuhn on these issues comes from Jouni-Matti Kuukkanen in “Kuhn on Essentialism and the Causal Theory of Reference”. Kuukkanen does an especially thorough job of bringing out how Kuhn’s views on these issues connect with a naturalistic strain in his thought very much informed by developmental psychology. But Kuukkanen perpetrates the same misreading as Sharrock and Read. Kuukkanen begins by assimilating Kuhn’s view (as well as Wittgenstein’s) to Eleanor Rosch’s prototype theory of concepts: “Rosch explicitly called into question the classic account of concept definition by a set of necessary and sufficient conditions and proposed Wittgenstein’s theory of concepts as an alternative to it. She reasons that, because all members of a category just do not share any set of exactly the same features, the features determining membership are better described as a large set over which individual instances overlap but do not share completely, that is, as a family resemblance concept. That is also Kuhn’s view” (Kuukkanen 2010, 550).

Although Rosch’s work was targeted at the classical theory of concepts that required a concept to be a list of necessary and sufficient criteria, other weaker forms of descriptivism have since emerged in the works of philosophers such as Frank Jackson and David Lewis.[[6]](#footnote-6) Kuukkanen therefore places, Kuhn in that revised-descriptivism movement:

Because Kuhn’s theory specifies not only what properties a kind can have but also what it cannot have, it is a rather peculiar sort of descriptivism. … [T]he set of properties that characterize a stereotypical member of a kind is not a definition of the kind. This means that a description does not have to be fully satisfied for a term to refer. The function of a description is to determine to what category/kind a particular individual belongs. … The essential claim is that reference is determined by a certain mechanism, which is the possession (and exclusion) of associated properties up to a satisfactory degree. (2010, 560)

A common device for descriptivist or hybrid causal descriptivist accounts has been that of a ‘Ramsey sentence’ – existentially quantified sentences that describe the functional roles of a number of inter-defined theoretical terms such as ‘proton’, ‘electron’ and ‘neutron’[[7]](#footnote-7). A Ramsey sentence thus encodes a description of a functional role, which might be realized by different entities in different possible worlds, or even within the actual world. A Ramsey sentence of ‘water’ is thus supposed to provide a ‘common concept’ between Earth and Twin Earth speakers, while the causal theory would account for the differing referents of the term on the two planets. Ramsey sentences can be used to formalize precisely the “peculiar sort of descriptivism” that Kuukkanen reads into Kuhn by stating what properties must be satisfied wo what degree for a given term to refer. Kuhn rejects Ramsey sentences as anything more than aids that might be used in translation.[[8]](#footnote-8) The problem, Kuhn thinks, is that Ramsey sentences by themselves fail to establish how they are to be used to identify their referents: “having hit upon the referent of a Ramsey-defined term at one point in a text would be no help in finding that referent in its next occurrence” (Kuhn 2000a, 46). More generally, what Ramsey sentences miss for Kuhn is that their semantics are inherited from the problems to which they are applied. “[T]he laws of a scientific theory, unlike the axioms of a mathematical system, are only law sketches in that their symbolic formalizations depend upon the problem to which they are applied. … Ramsey sentences are simply not a rich enough source of clues to block a multitude of trivial interpretations. To permit reasonable interpretation of a text studded with Ramsey definitions, readers would first have to collect a variety of different ranges of application. And, having done so they would still have to … invent and test hypotheses about the sense of the terms introduced by Ramsey definitions” (47).

Kuhn’s use of the phrase ‘sense’ is a little unhappy here, as it does suggest a Fregean view according to which there is some part of the semantics of a term separable from reference, adequate grasp of which enables one to pick out the referents. But I think more carefully attending to Kuhn’s complaint shows that he is rejecting that direction of explanation. A stretch of scientific text is meaningful first and foremost within a range of application where it serves to solve a particular problem. It might be that the terms employed in solving that problem will count as ‘referring’ by the lights of a subsequent theory, but they may also fail to do so. Similarly, one might abbreviate the working knowledge of a scientist in something like a Ramsey definition or sentence, or some other descriptivist gloss on what a Fregean sense is. Taken in isolation from the environment in which a scientist used that term and apart from the largely tacit know-how with which they deployed it, the descriptivist gloss will fall well short of capturing anything like the ‘meaning’ that phrase had in use.[[9]](#footnote-9) In this sense, I believe, the ‘meaning’ of a scientific term as Kuhn discusses it, is not well thought of as anything like an aggregate of sense and reference. Rather the latter two notions are abstractions from a scenario in which a given term figures in a theory that solves some problem in a given domain of application.

The differences of this picture of Kuhn’s from more standard views, is I think in evidence in remarks the Kuhn made across his career concerning ‘meaning’ and concept acquisition more broadly. I turn to these in the next section to explore how we are to understand the meaning of ‘meaning’ in Kuhn’s work.

* 1. **Kuhn’s ‘Meaning’**

In this section I want to explore how Kuhn thought about ‘meaning’ very broadly and to develop the suggestion at the end of the last section that as Kuhn uses the term, ‘meaning’ is not an aggregate of sense and reference. I begin with a passage from Kuhn’s unfinished manuscript *The Plurality of Worlds* before showing that central ideas of that passage were already to be found in *The Structure of Scientific Revolutions*. Recall that Kuukkanen placed Kuhn in the revised descriptivism movement largely on the basis of an alleged affinity between Kuhn (and Wittgenstein) with Eleanor Rosch’s prototype theory of concepts. In this late passage however,

Kuhn makes it quite clear that he saw his view very to be different from Rosch’s prototype theory:

What [one] must extract from a learning process is not a particular list of features, but rather some measure of similarity and difference that produces clusters of the member of each kind and puts empty space between them. In the process, he necessarily also acquires a similar measure of the arrangement of the various kinds. … It is that arrangement that [we] *speak* of as “structure,” and it is structure alone that must be shared by individuals who cluster the same individuals into the same kinds. That clustering is prerequisite to their ability to communicate unproblematically about the clustered creatures, and it is that communication which testifies to their sharing the relevant concepts and the same meanings. Ironically prototype theory, the theory of meaning and concept formation most like my own, misses the need for this bit of local, non-Quinean holism. Like those who believe in characteristic features, prototype theorists conceive classification and meaning as attaching to nature one at a time. The distance they are concerned with is that separating an individual from a prototypical example of the kind in which it should be placed. No role is played by the distance to neighboring kinds. (Kuhn 2022, 241-242)

That is, what Kuhn thought was at issue in incommensurability wasn’t anything like sense or a description of features however watered down, but the actual structuring and grouping of things that might result from the guidance of various such senses or descriptions. Meaning is not to be identified with some content, description or abstract entity grasped prior to (and causing) an act of classification, but the resulting classification or categorization of things. Meaning for Kuhn is not to be identified with something logically or causally prior to use but is found in use. I take this feature of Kuhn’s view to shed light on a remark of Kuhn’s that “‘meaning’ is not the rubric under which incommensurability is best discussed” (Kuhn 2000a, 36). ‘Meaning’ is not the best rubric for discussing incommensurability if one understands meaning as a description or sense apprehended prior to an act of classification. In his unfinished book, *The Plurality Worlds* however, Kuhn referred to himself as providing “a long-sought theory of the meaning of kind terms” capable of explicating “experiences of incommensurability” (Kuhn 2022, 214). ‘Meaning’ is not the best rubric for discussing incommensurability if one understands meaning as a description or sense apprehended prior to an act of classification. For incommensurability concerns the actual resulting classification or carving up of things. If, on the other hand, we should individuate meanings and concepts according to the acts of communication and clustering that they facilitate, then meaning is precisely what incommensurability concerns. When I use the term ‘meaning’ in connection with Kuhn’s view from now on, I will intend his ‘rehabilitated conception’. I now want to turn to that theory of meaning Kuhn was developing, while showing that aspects of it were strongly anticipated by *The Structure of Scientific Revolutions*.

On the descriptivist reading offered by Kuukkanen, Kuhn was supposed to be objecting mainly to the idea of an underlying list of necessary and sufficient conditions, and instead suggesting that our concepts are clusters of descriptions, which may be neither sufficient nor necessary.[[10]](#footnote-10) Kuhn made clear that he was opposed any such view of conceptualization as necessarily, or at bottom, proceeding from a list of features. Instead, Kuhn suggests that using a term according to a set of criteria (even one weaker than a jointly necessary and sufficient set) is logically and psychologically derivative on an ability to use terms on the basis of a non-inferential capacity for perceiving likeness or analogy. Kuhn starts by considering by our ordinary capacity for facial recognition. That case “suggests with special clarity that neither identification of a particular individual nor of its kind requires knowledge of some special constellation of qualities, properties, or features shared by all presentations of that object or by all members of that kind. One need only know features that can differentiate the substance or kind in question from others with which it might, *in the world as it is* [Kuhn’s italics], be confounded” (Kuhn 2022, 210). Although one might go off ‘some features’ in differentiating something from same range of alternatives in a particular case, one may not yet know them in the sense of being able to bring them to explicit self-consciousness. A list of differentiae is not merely a partially complete list of criteria: “Additional differentiae … make successful identification more likely, but they do not guarantee it; nor is their absence incompatible with it. There is nothing quite like having gotten the list of required features right” (210).

Talk of a list of features can be heard as suggesting some form of descriptivism, where the lists of features are the descriptions that determine reference and guide use. However, I believe that Kuhn distances himself from such an understanding. ‘List of features’ only suggests descriptivism if one takes Kuhn to be answering a certain question – namely ‘in virtue of a prior grasp of what does competent use of a concept consist’. Instead, Kuhn suggests that what we have here is a form of ability that undergoes continuous refinement, whose acquisition is not a punctate episode, and which is without a final terminus. It is, as Kuhn puts it: “a continuing process requiring a space of differentiae that must … be steadily enriched as the learner encounters more and more objects that must be told apart” (Kuhn 2022, 226). Inferential processes are in turn built upon capacities like recognition by differentiae or the perception of similarity, but the latter remain “basic when inferential processes are developed” as they supply “the domain of recognizable objects and situations that inferential identification processes require” (213). In turn, the kinds to which incommensurability pertains are initially identified “in an appropriate field of differentiae where members of the same kind clustered at a distance from the clusters formed by members of other kinds”(220). This aspect of Kuhn’s view also sheds light on why introducing the term ‘electrical charge’ requires multiple acts of ostension, and, moreover, why this is not simply a point about the teaching of the term. ‘Electrical charge’, and kind terms more generally, require multiple acts of ostension because the space of differentiae, or contrast classes, are a part of their semantics for Kuhn: and not mere pedagogical aids for the teaching of the term.

Long before developing this account, Kuhn in *Structure* had already identified aspects of identification by differentiae in both the education of scientists and the history of science itself. In the former case, the scientist-in-training learns by analogically extending paradigmatic problem solutions to new puzzles: “the science student, confronted with a problem, seeks to see it as like one or more of the exemplary problems he has encountered before. Where rules exist to guide him, he, of course, deploys them. But his basic criterion of similarity is a perception of similarity that is both logically and psychologically prior to any of the numerous criteria by which that same identification of similarity might have been made. After the similarity has been seen, one may ask for criteria, and it is then often worth doing so. But one need not” (1977, 308).

In the latter case, a normal science develops by extending new, exemplary problem solutions to new contexts by a process Kuhn likened to analogy or perceiving similarities – a process Kuhn called paradigm articulation. Rules in the sense of symbolic generalizations describing the new use only come after this process, and are not present to guide it, even tacitly. “[A] single normal-scientific tradition … [may not] satisfy some explicit or even some fully discoverable set of rules and assumptions that gives the tradition its character. ... Instead, they may relate by resemblance and by modeling to one or another part of the scientific corpus which the community in question already recognizes as among its established achievements. … [T]he coherence displayed by the research tradition in which they participate may not imply even the existence of an underlying body of rules and assumptions that additional historical or philosophical investigation might uncover” (1962/2012, 45). Rules may be stipulated or laid down when a previously flourishing research tradition enters a period of crisis: “Normal science can proceed without rules only so long as the relevant scientific community accepts without question the particular problem-solutions already achieved. Rules should therefore become important and the characteristic unconcern about them should vanish whenever paradigms or models are felt to be insecure” (47-48).

In addition to this evidence that Kuhn was not a descriptivist when he wrote *Structure*, consideration of Kuhn’s examples of incommensurability therein also suggest he was not committed to a global version of incommensurability. Consider for instance, Kuhn’s discussion of Newton’s laws in comparison to Einstein’s:

Can Newtonian dynamics really be *derived* [Kuhn’s italics] from relativistic mechanics? Imagine a set of statements E1, E2, …, En which together embody the laws of relativity theory. These statements contain variables and parameters representing spatial position, time, rest mass, etc. From them, together with the apparatus of logic and mathematics, is deducible a whole set of further statements including some that can be checked by observation. … Apparently Newtonian dynamics has been derived from Einsteinian, subject to a few limiting conditions.

Yet the derivation is spurious, at least to this point. Though the Nis are a special case of the laws of relativistic mechanics, they are not Newton's Laws. Or at least they are not unless those laws are reinterpreted in a way that would have been impossible until after Einstein's work. The variables and Parameters that in the Einsteinian Eis represented spatial position, time, mass, etc., still occur in the Ni’s; and they there still represent Einsteinian space, time, and mass. But the physical referents of these Einsteinian concepts are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian is convertible with energy. only at low relative velocities may the two be measured in the same way, and even then, they must not be conceived to be the same). Unless we change the definitions of the variables in the Nis, the statements we have derived are not Newtonian. If we do change them, we cannot properly be said to have derived Newton's Laws, at least not in any sense of "derive" now generally recognized. … [O]ur argument has, of course, explained why Newton's Laws ever seemed to work. In doing so it has justified, say, an automobile driver in acting as though he lived in a Newtonian universe. An argument of the same type is used to justify teaching earth-centered astronomy to surveyors. (1962/2012, 101-102)

Kuhn’s point here is that although the word ‘mass’ occurs in both Newtonian and Einsteinian mechanics, within each of those theories it a member of an interdefined set of terms – one set in Newtonian theory, another set in Einsteinian theory. Within each of those families the words mass, mass, energy, space, time occur, are importantly interrelated. But in the two theories, the exact relationships between those terms are crucially different. Within each of those families the relationships between those terms are different. In particular, in Einstein’s the terms are all far more intimately related That prevents treating ‘mass’ it occurs in Newtonian theory as instantiating the same concept as ‘mass’ does in the Newtonian context. Here Kuhn identifies what is not shared in contrast to what *is shared –* shared in the sense of being intersubstitutable one-by-one. If he were truly committed to the global thesis, Kuhn would have to deny that *any* concepts *were* shared in that way between practitioners of the two paradigms. Although the Newtonian and Einsteinian would disagree over what definition should be attached to a variable – *what* physical quantity it should represent – they agree on what a variable is in discussion of the two theories. ‘. The numerals must therefore be shared by the members of the two paradigms, even if they physical quantities they attach them to are not. Although the two theories offer very different predictions about what the perihelion of Mercury is – they agree on what a ‘perihelion’ is and what Mercury is. Similarly, non-mathematical terms like ‘Earth’, ‘surveyors’, ‘automobiles’ are also shared, and shared so as to allow one-by-one inter-substitution.

Turning to another example from *Structure,* Kuhn describes Lavoisier’s key observations as filling in pre-existing doubts Lavoisier harbored concerning phlogiston-based chemistry – doubts that can be coherently articulated only given a local understanding of incommensurability. Kuhn writes: “Long before he played any part in the discovery of a new gas, Lavoisier was convinced both that something was wrong with phlogiston theory and that burning bodies absorbed some part of the atmosphere. … What the work on oxygen did was to give much additional form and structure to Lavoisier’s earlier sense that something was amiss. It told him a thing he was already prepared to discover – the nature of the substance that combustion removes from the atmosphere” (56).

It is striking that in Kuhn’s description, Lavoisier’s observations furnished him with the answer to a question he had formulated *prior* to the chemical revolution. If incommensurability were a global phenomenon, even Lavoisier himself would not have retained any concepts over the course of the chemical revolution. Instead, Lavoisier would have simply dumped all of his concepts for a completely different set of concepts, in the process swapping one set of questions for another. Although, to be sure, Lavoisier did give up a number of puzzles from the phlogiston paradigm, this quote of Kuhn’s describes a continuity in Lavoisier’s thoughts and concerns that is simply inexplicable on a global understanding of incommensurability. For we have here a common question, requiring some common concepts – it is simply that the eventual answer to that question required novel concepts incommensurable with those of the phlogiston paradigm.

As with our example above, this involved a set of terms whose meanings did not change, as well as concepts that had to be discarded as they could not fir into the new paradigm, and new concepts that could not have been integrated into the previous paradigm. After Lavoisier’s revolution, concepts like ‘oxygen’ are introduced which could have no place in the earlier paradigm. Likewise earlier terms, like ‘phlogiston’ and ‘dephlogisticated’, which could have no place in the new paradigm, are eliminated. Other concepts are retained across the transition though – ‘burning bodies’, ‘atmosphere’, ‘absorbed’, ‘gas’, ‘air’, ‘bottled’, and so on. One finds the same patterns in the previous example. Concepts which could have no place in Newtonian physics were introduced in Einsteinian physics – such as ‘light cone’, ‘space-time continuum metric’ or those involved in the Principle of Equivalence – while concepts from Newtonian physics that could have no place in the new paradigm were dropped – such as ‘luminiferous aether’.

I think that establishes that Kuhn saw his relation to the descriptivism very differently from

the vast majority of commentators have portrayed it. Both Read and Sharrock, as well as Kuukkanen were rare exceptions in identifying Kuhn’s criticisms of the causal theory, but they were wrong in construing those criticisms as arguments for the importance to Kuhn of Fregean sense or descriptivism. Sometimes ‘Fregeanism’ and descriptivism are simply thought of as views that hold there to be more to meaning than extension. Kuhn is certainly not contending that all the semantic or cognitive meaning of scientific terms is their reference or extension. But he is denying that the meaning of scientific terms should be thought of as an aggregate of extension, and intension or sense or a description of any sort. Moreover, shifting the emphasis away from senses as the home of incommensurability towards the sorting and categorizing of actual things, helps to bring out some residual affinities of Kuhn’s view to the causal theory, and why he still saw in it a “significant technique” for comparing successive theories.

Meaning, as the locus of incommensurability, is a plainly anti-individualist kind of meaning, in Tyler Burge’s sense – it depends not only on an individual but on their relations to others and on the things in their environment. That is because it depends upon potentially successful communication and the potential sorting of actual things in the world. Even if Kuhn was doubtful of applying the causal theory to all scientific kind terms, he did describe the learning of scientific kind terms in a manner that recalls both the social and indexical aspects or externalist or anti-individualist theories of meaning: “How do individuals learn to use such terms? Through simultaneous interaction with someone who already knows how to use them and also with the world to which they apply. The learning process is thus a transmission process, from one generation to the next” (Kuhn 2022, 265).

While Kuhn was critical of Putnam’s original Twin Earth thought experiment, he Putnam’s thesis of ‘the linguistic division of labour’: “Laypeople can, of course, say that water is H2O without controlling the fuller lexicon or the theory that it supports. But their ability to communicate by doing so depends upon the presence of experts in their society” (Kuhn 1990, note 26, 318).

That “learning” a term involves interaction with the “world” meanwhile reflects the emphasis externalists like Putnam have placed on the indexical quality of kind terms – their meaning is determined by how the world to which they apply is. As Kuhn sees it ‘meaning’ is not something that can simply be factorized into independently intelligible notions of sense and of reference, where the former determines the latter. And nether, should meaning, for Kuhn, be identified with one or the other. Kuhn has been widely recognized is as not wanting to identify meaning with referents the attached to our words by factors beyond our practices. As Kuhn himself put it, he was urging “the rehabilitation of a conception of meaning that includes more than extension” (2022, 174). I hope by this point to have provided evidence that that rehabilitation also required something quite different for Kuhn than just the traditional notion of *intension*, or sense.

Meanwhile, the continuity of the later Kuhn’s anti-descriptivist conception of meaning with the remarks from *Structure* provides evidence that this was not a complete overhaul of Kuhn’s earlier views, but a refinement and extension of ideas present in his earlier work. I noted earlier that Sankey accepts that Kuhn’s later view of incommensurability was a local one – he sees no continuity here. Sankey takes Kuhn to have propounded a global version of incommensurability in *Structure*, and only later shifted to a local version, all the while presupposing a descriptivist theory of meaning. I take the passages cited from the “Priority of Paradigms” chapter, and their anticipation of Kuhn’s later works, to pose problems for this reading, and to speak in favour of a reading of Kuhn that sees the development of the incommensurability thesis as a refinement of what was always a local claim. Moreover, it is equally inaccurate to say of Kuhn, as Sankey does, that “Analysis of the reasoning employed by Kuhn … reveals that [he] assume[s] that reference is determined by description” (Sankey 2009, 197). On the contrary, I take the analysis I’ve provided to show that reference is not determined by description for Kuhn. Rather, reference is determined by using a vocabulary to solve problems and then extending that vocabulary by non-rule governed processes such as paradigm-articulation or by means that are initially non-inferential, such as recognition by differentiae or analogy. Here the use of the terms in question serves to determine both their sense and their reference, rather than either of the latter determining the former.

To summarize Kuhn’s anti-descriptivism and his nuanced relation to causal theories, it will help to look at Kuhn’s response to Philip Kitcher concerning the reference of phlogiston. Kitcher had argued on the basis of a version of the causal theory of reference that tokens of the phrase ‘dephlogisticated air’ as spoken by Joseph Priestley should be taken as referring to ‘oxygen’. Kuhn agrees with Kitcher that something like the causal theory “can be used to identify referents of the terms and expressions of eighteenth-century chemistry, at least to the extent that those terms refer” (Kuhn 2000a, 40). Against Kitcher, though, Kuhn maintains that “reference determination” is not by itself “interpretation” but is “merely a prerequisite to it” (54). Kuhn here is not just registering the uncontroversial point that co-reference is not sufficient for two concepts being identical though. Although, there are occasions where ‘phlogiston’ referred to hydrogen and occasions where it failed to refer, phlogiston is still incommensurable with modern chemistry, but not because there is no strict synonym for it in the latter theory. Plenty of terms are not synonymous with any combination of terms in modern chemistry, but that does not make them incommensurable with the theory. Having performed reference determination with respect to phlogiston theory, and even having gone on to succeed in interpretation, one will have learned the meaning of the term ‘phlogiston’, but it still remains incommensurable with the modern theory. It remains incommensurable because it simply cannot be put into the lexicon of modern chemistry – it is drawn from a different structured lexicon and would require incorporating other terms that would cut across the hierarchy of terms in modern chemistry. Phlogiston chemistry “differed from its twentieth century successor not simply in what it had to say about individual substances and processes, but in the way it structured and parceled out a large part of the chemical world” (44). The way phlogiston theory “structured and parceled out … the chemical world” is incompatible with modern chemistry and so adding terms from phlogiston theory would alter “what it is to be an element and a good deal else besides”, it would “change rather than add to what was there before” (54). This change would not produce, according to Kuhn, a structured lexicon that could serve the purposes of puzzle solving constitutive of normal science. Incommensurability registers this phenomenon, which is something over and above the mere failure of synonymy. The term ‘water’ is also featured in phlogiston chemistry, and since it was retained across the change in chemical theories, we have here a redubbing. While many of the same items continued to be counted as water, ‘water’ came to be associated with a different structured lexicon and acquired new contrast classes. Even then if some tokens of a term refer to the same property across a revolution, we have a redubbing of the term type and an instance of incommensurability.

* 1. **Kuhn’s Rigid Designation**

There remains one more element of Kuhn’s view, clearly to related causal theories of reference, which I want to discuss – his use of the phrase ‘rigid designation’. In this final section I will argue that Kuhn’s use of it, differs importantly from Kripke’s. The main importance of registering this difference is that correctly understanding Kuhn’s use of this phrase is a way of capturing what happens in episodes of incommensurability. Correctly understanding this will also help to reiterate why the meanings of two incommensurable structured lexicons in use are not aggregates of sense and reference, as discussed earlier in 4.2. While doing this I also hope to fend off a worry that Kuhn takes past scientific theories to have referred to abstract properties like ‘being phlogiston’ or ‘having Newtonian mass’ that happened not to be instantiated in the actual world.

In Kripke’s parlance, a term is rigid if refers to the same thing in all possible worlds. Although Kuhn was sceptical of the potential for establishing the rigid designations of natural kind terms individually, based on single baptisms, he allowed that such terms might designate rigidly over more restricted time spans, under certain circumstances: “Only for the periods between those acts, I shall argue, does dubbing result in rigid designation” (Kuhn 1990, 298).[[11]](#footnote-11)

The two key questions about this are, ‘what do such terms rigidly designate?’ and ‘in what sense do they do so?’. Taking the first question, Kuukkanen brings out an immediate, seeming problem for Kuhn: “it is difficult to square the idea that general terms designate their extensions with rigid designation. It is not clear how we could preserve the set of individuals across all possible worlds, even when the taxonomic system remains invariant” (Kuukkanen 2010, 556).

That is, if natural kind terms are to be rigid, they cannot simply designate their actual extensions – if the world had slightly less water in it, the term ‘water’ would designate something different and so would fail to be rigid. A common resort for causal theorists wishing to preserve rigid designation is therefore to claim that natural kind terms designate abstract properties or universals like ‘being H2O’, that enjoy some sort of independent existence from their instances.[[12]](#footnote-12) There has been a tradition of interpreters beyond Kuukkanen who have interpreted Kuhn as some kind of ontological nominalist. Ian Hacking presented one such reading, but Kuhn himself was wary of identifying with this reading. As Kuhn put it: “[Hacking’s] nominalist version of my position – there are real individuals out there, and we divide them into kinds at will – does not quite face my problems. The reasons are numerous, and I mention only one here: how can the referents of terms like ‘force’ and ‘wave front’ … be construed as individuals?” (1993, 316).

Even if Kuhn is wary of taking the nominalist route that a kind term simply designates its actual extension, I agree that positing abstract properties or universals doesn’t fit Kuhn very well.

For help, let’s look at what Kuhn says about kind terms. According to Kuhn, “*kind terms* [Kuhn’s italics] … name the kinds of things, situations, and properties which occur in the world as we know it” (2022, 168). Furthermore, kind terms “are projectible: to know any kind term at all is to know some generalizations satisfied by its referents and to be equipped to look for others” (Kuhn 1993, 317). Although kind terms are primarily identified and tracked by the recognition of differentiae, potentially without recourse to symbolic generalizations, this process gives rise to expectations that take the form of generalizations and laws. These generalizations may be only “normic generalizations”, which “admit exceptions” like ““Liquids expand when heated sometimes fails, e.g., for water between 0 and 4 degrees centigrade” (317). “Nomic generalizations”, however, are “exceptionless generalizations”, or “usually laws of nature: Boyles law for gases or Kepler’s laws for planetary motions are examples” (317).

What Kuhn calls incommensurability affects both kinds obeying normic generalizations, and those obeying the nomic ones. This, for Kuhn, gives kinds some sort of independence from their actual instances or our beliefs about them. Most fundamental to a structured lexicon are not the generalizations it generates, but the structure it provides, and that can be abstracted from the extensions of a set of terms in the actual world. When Kuhn talks in his unfinished manuscript of unchanging kinds, I therefore take him to have in mind such a structure, rather than, say, a ‘universal’. “People and other objects change over time. Kinds of materials do not. People have believed that some everyday materials evolved from others. … But that does not affect the constancy through time of the *kinds* of material. … That constancy does not, furthermore, imply a corresponding constancy in the means by which identification was achieved … though tests for and corresponding beliefs about gold may change with time, the kind named *gold* cannot itself change any more than the kind of figure named *triangle*” (Kuhn 2022, 254; Kuhn’s italics). This is not because our term gold happily glommed onto *the universal* ‘gold’ but is a reflection of the fact that our word gold came to have a place in a particular structured lexicon: “The place of gold within the kind set for metals must remain the same. Whatever set of differentiae are used in distinguishing metals, they must at all times preserve the empty space between them” (254).

Beliefs about alchemical transformations of metal did not, Kuhn thinks, run afoul of the structure of the kind set containing gold. There were simply false ideas about the possibilities of transforming members of one kind set into others. This passage clearly states some commitment to a notion of kind more robust than just the extensions of objects to which we happen to apply a kind term at a given time – what matters is not the particular objects but the structure into which they’re sorted: “It is that arrangement that [we] *speak* of as “structure,” and it is structure alone that must be shared by individuals who cluster the same individuals into the same kinds” (Kuhn 2022, 241). These structures, moreover, are not identical with senses or definitions. In the actual world, more than one set of definitions could give rise to the same structure. In other circumstances, a set of definitions could produce a structure quite different to that in the actual world. As discussed in the previous section, simply classifying by differentiae, rather than by inference from definitions, could equally well produce such a structure.

I turn now to the second question for Kuhn’s use of ‘rigid designation’ – ‘how do they designate rigidly’? More precisely, in what range of ‘possible worlds’ do kind terms designate for Kuhn? Is it all metaphysically possible worlds? It would seem clearly not. For Kripke, it was a metaphysical possibility that H2O could lack all its typical observable properties and some other chemical substance XYZ could possess them instead. As Kuhn points out, that is not a nomological possibility by the lights of our current theories, and beyond that a question like ‘Is water still H2O’ “[does] not individually have ‘yes’ or ‘no’ answers” (Kuhn 1990, 306). So, for Kuhn to understand the meaning of a kind term we shouldn’t merely look at what it refers to or to textbook or speakers’ definitions of it. As Kuhn discussed in his reply to Kitcher, some tokens of a kind term might refer to instances of a later subsequent kind term, other tokens might have failed to refer at all. Meanwhile, if we generate Ramsey sentences from various given definitions, we will at best get ‘generalization sketches’ for how to apply the term. Instead, we should seek to identify the structured lexicon in which it figured and understand what problems that lexicon solved and how it did so.

The rigidity of theoretical terms for Kuhn is not their picking out individuals in all possible worlds, as proper names do for Kripke. Neither is it in kind terms picking out properties or universals one-by-one independently of their connections to one another and any observational vocabulary. What is rigid for Kuhn is a whole structured lexicon built up through repeated ostensions to actual samples of stuff. And “Only while that system endures do the names of the kinds it categorizes designate rigidly” (Kuhn 1990, 314-315).

That Kuhn believes something like dubbing takes place, rather than laying down of fully formed senses or descriptions relates back to Kuhn’s insistence that kind terms are primarily “identified by differentiae”. And it is in part only by understanding that conception in relation to Kuhn’s talk of rigid designation that one can properly understand how Kuhn’s “long-sought theory of the meaning of kind terms … brings with it a way of explicating the experiences of incommensurability” (Kuhn 2022, 214). Any world or scenario in which one kind term designates is a world in which the whole system designates, and its generalizations apply. This is then a different use of ‘rigid designation’ from Kripke. In Kripke, a term was a rigid designator if it picked out the same individual or property in all possible worlds. For Kuhn, on the other hand, a system of theoretical terms designates rigidly if either all of its terms apply, or none of them do. I say, ‘apply’ rather than ‘refer’ because it is important not just that the terms happen to refer while the generalizations in which they figure are all completely false or fail to facilitate the puzzle solving characteristic of normal science.[[13]](#footnote-13) Kuhn’s sense of rigid designation then, is ultimately a way of capturing incommensurability!

1. **Incommensurability in action**

In this final section, I begin by looking at two of Kuhn’s examples of incommensurability, fleshed out in the terminology developed over the last several sections. I then turn to discussing how practical concerns and other contextual factors shape structured lexicons, and how these in turn frame possibilities of action. This in turn allows me to explore connections between the incommensurability thesis and issues in the practice of science concerning the speciation of scientific disciplines. Some recent authors have argued that for Kuhn topic of taxonomic incommensurability should just be identified with the development of different lexicons that comes with the speciation and fragmentation of scientific disciplines over time. While these topics are importantly related, I argue here that they should not simply be identified. I believe that appreciating this point also helps to see why Kuhn’s development of incommensurability in his later work was not the ‘watering-down’ of his earlier ideas that many have taken it to be.

Firstly, here is how I propose to articulate some of Kuhn’s famous examples of commensurable and incommensurable terms. The ancient Greek term *planētēs* is incommensurable with the post-Copernican concept of a planet because both are parts of incompatible structured lexicons. Pre-Copernican Revolution, the term *planētēs* was a rigid designator – in considering possible scenarios it was inextricable from its associated generalizations and contrast kinds. We can now entertain a sense that is co-extensional with the older term *planētēs*, and which is therefore a good translation. But we cannot consistently use that term and the system in which it is embedded, given our present state of knowledge. Gold is meanwhile a kind term where the causal theory is approximately correct. Even though believers in alchemy held elaborate theories about which substances could be transformed into gold in which circumstances, because of continuity concerning what objects were counted as gold and which were sorted into contrast classes, Kuhn is here prepared to grant continuity in reference across theory changes. Of course, other terms of alchemical theories still prove incommensurable with respect to modern chemistry. That a term like ‘gold’ and its contrast classes may not be incommensurable between theories, while other terms are incommensurable, is part of what I take the ‘local’ aspect of Kuhn’s ‘local holism’ to come to. ‘Water’ for instance would still seem to remain incommensurable on Kuhn’s view between modern chemistry and the chemistry theory of Priestley. The reason being, unlike gold, that a redubbing occurred that moved ‘water’ from its place in one structured lexicon to another. Where once ‘water’ had been an element with contrast classes including air and phlogiston, it came to be defined as a compound with new contrast classes and an extension including phenomena and entities that had not previously been counted as ‘water’.

Looking at these examples, it is possible to also detail how some contextual factors affect the structured lexicons formed in the pursuit of solving problems. Taking first the example of ancient Greek astronomy, a factor that undoubtedly shaped that structured lexicon was the limitation to the naked eye. Given the importance of tracing the position of visible bodies in the night sky for the purposes of keeping a calendar and the restriction at the time to phenomena that were immediately visible to the naked eye, it should not be surprising that a structured lexicon formed that grouped according to such features as were then observable and were relevant to such endeavors as tracking correlations between changes of seasons and the motions of heavenly bodies. Across the astronomical revolution from Aristotle to Copernicus, the re-tooled concepts of heavenly bodies (the Copernican criteria for individuating stars, satellites, and planets) no longer rested on properties immediately visible to the naked eye and or relevant to answers to questions concerning agriculture and navigation. What is functionally significant for Aristotle’s individuation of the heavenly bodies did not survive the transition to Copernican astronomy, turning to gold, its being “functionally significant” as Kuhn says, can be seen to play an important role in establishing a structured lexicon that largely weathered the transition to modern chemistry. The importance of gold to commerce along with its malleability and low melting point, required distinguishing it carefully from visually similar metals such as Pyrite. These interests lead to the establishment of contrast classes which were preserved across the chemical revolution – even while other networks of terms underwent revolution. Seeing that gold provides a case of a kind term that closely approximates the causal theory thus requires attending not only or primarily to historical definitions, nor only or primarily to the referent of gold by our current lights but attending to the practical aims that shaped the structured lexicons in which gold figured.[[14]](#footnote-14)

Some commentators have felt that Kuhn’s later “taxonomic” version of incommensurability, may have turned incommensurability into a merely verbal or semantic issue, at the expense of exploring more practical and methodological incompatibilities between different scientific disciplines. Lydia Patton, in an article that shares much of my reading of *Structure*, writes, “Kuhn’s statements about incommensurability came to be seen – even by Kuhn himself – as broad claims about lexical or taxonomic “speciation” between theories … and as limitations on the ability of scientists even to express their results using rival conceptual frameworks. Such developments are a shame, in my view. Kuhn’s original work did not restrict “paradigm” to “theoretical framework,” nor did he restrict the perspective of scientific practice to the content of propositions with a truth value” (Patton 2018, 124).

I agree with Patton that in Kuhn’s original work “paradigm” should not be equated with something like “theoretical framework”, but I disagree with any reading of the later Kuhn’s taxonomic version of incommensurability that takes it to force a restriction of the locus of incommensurability to anything like a theoretical framework. Kuhn, in clarifying the uses of the term ‘paradigm’ in *Structure,* came to identify the “more fundamental sense” of ‘paradigm’ as that of an exemplar or “concrete problem solution” to be imitated (Kuhn 1977, 298). Kuhn contrasted that sense of paradigm with another found in *Structure*, where ‘paradigm’ had been used to refer to a “disciplinary matrix” of which paradigms in the sense of exemplars are one element, along with “symbolic generalizations”, “models”, professional standards – “all the shared commitments of a scientific group” (297-298, 294). What I’ve been trying to show throughout much of this essay is that a structured lexicon should not be identified with any particular set of “symbolic generalizations”, either explicit or implicit, as a descriptivist reading would have it. What is primary in both the initial creation and subsequent extension of a structured lexicon is not the guidance of “rules”, “criteria”, or “symbolic generalizations”. Just as Patton sees the ‘paradigm’ in *Structure* should not be equated with ‘scientific theory’, neither should ‘structured lexicon’ and ‘theory’ be identified in Kuhn’s later work. Many specific theories might share a taxonomy. A structured lexicon, like a paradigm in structure, is a model of the concepts that should be applied to phenomena. Like a paradigm in *Structure*, a structured lexicon is therefore broader than a theory. Also, like a paradigm, a structured lexicon also restricts the possible space of theories. A structured lexicon then should not be identified with a theory or a “theoretical framework” in the sense of a set of schematic generalizations, and the study of structured lexicons should not primarily be through the lens of “semantic theorists like Quine, Davidson, and Sneed”” – as Patton claims Kuhn himself saw it – but through attending to particular applications to problems and puzzles – among which is the “context of pedagogy” that Patton (rightly) emphasizes.

What is true is that Kuhn was interested in exploring how practical considerations shape different structured lexicons. It is also true that one can explore how practical considerations shape the structured lexicons in the cases of incommensurability. However, it is crucial to separate two related, but distinct issues that have often been run together. The topic of incommensurability has been confused by conflating it with the issue of the speciation of scientific disciplines. Though there are connections between the two topics, running them together has obscured Kuhn’s incommensurability thesis in his later writings. Talk of different structured lexicons indeed comes up in both topics, but it is wrong to conclude that wherever there is a different structured lexicon, there is incommensurability. Sorting out this conflation will help to reinforce some points from earlier in this essay, while also helping to demonstrate why Kuhn’s taxonomic development of incommensurability was not merely a shallow point about semantics.

An important development in Kuhn’s thought is a transition from a punctuated equilibrium view of the evolution of scientific theories that emphasizes sudden revolutions, to a more gradualist picture. The “revolutionary” characteristic comes to be seen more as the impression of the historian looking back, rather than the scientist moving through time: “Might not the holistic language changes that the historian experiences as revolutionary have taken place by a process of gradual linguistic drift?” (2000a, 57). Incommensurability occurs then not all at once, but over time – both as effect and cause of the increasing specialization of scientific subfields: “Over time a diagram of the evolution of scientific fields, specialties and subspecialties comes to look strikingly like a layman’s diagram for a biological evolutionary tree. Each of these fields has a distinct lexicon, though the differences are local, occurring only here and there. There is no lingua franca capable of expressing, in its entirety, the content of all of them or even of any pair” (2000c, 99).

While important to recognize how scientific specialization may create completely disjoint structured lexicons, it is also important to keep this phenomenon separate from that of incommensurability. Although two disjoint scientific disciplines will most likely lack a common lingua franca, that does not make them incommensurable. Since incommensurability is local, it depends upon a background of commensuration that requires at least some overlap in domains of application. It is therefore worth distinguishing the two phenomena of incommensurability and speciation, rather than collapsing them.

In another paper, Kuukkanen, has done an excellent job in detailing Kuhn’s shift towards a more gradualist picture of the evolution of science. Kuukkanen, however, takes incommensurability to characterize the relationship between all different structured lexicons across the sciences.[[15]](#footnote-15) For Kuukkanen, the speciation of scientific disciplines always creates incommensurability: “Incommensurability plays then a central role in Kuhn’s theory of scientific speciation, because speciation may lead to taxonomic breaks between disciplines, the relationship of which is characterized by the concept of incommensurability” (2012, 139).

Although any two distinct scientific fields will have a distinct lexicon that will not admit translation into a common “lingua franca”, I do not think that most such cases should be labelled incommensurability. Two scientific disciplines may be so disparate in their domains of application that the question of translation is simply inapplicable. In cases of incommensurability, even when interpretation has occurred, one cannot import a term belonging to one structured lexicon into another. A scientist may even become bilingual and be able to switch between different paradigms, but they will still be unable to combine the two structured lexicons into one lexicon capable of sustaining a normal science. Simply because two sciences employ different structured lexicons, that does not mean that attempting to apply both would violate the no-overlap principle – for their domains of application simply may not overlap. For incommensurable lexicons, however, there must a threat of overlap.

Incommensurable paradigms feature different structured lexicons that cannot be applied simultaneously and which resist translation. Incompatible structured lexicons are therefore at the heart of incommensurability, but their incompatibility is ultimately not to be explained by something like the incompatibility of semantic content – or an incompatibility of ‘senses’. The incompatibility is one in the expectations and subsequent actions in the users of the lexicon. The incompatibility between, say, phlogiston chemistry and modern chemistry, which incommensurability is supposed to illuminate, has a very different form from the practical difficulty of simultaneously doing neuroscience and general relativity. The former sciences overlap in domains of application, and attempting to use both creates an attempt to combine incompatible expectations and ways of acting, creating incoherence. The latter simply have different domains and scales of application. Commitment to neuroscience involves no repudiation of general relativity, and vice-versa. However, a commitment to modern chemistry does involve a repudiation of phlogiston-based chemistry. A commitment to the paradigm to the former paradigm in which a particular structured lexicon has its home requires a rejection of the latter paradigm. This cannot be understood solely by attending just to a structured lexicon as a set of terms, in abstraction from it use.

Having cleared up the relationship between incommensurability and speciation, we can now return to the issue raised in the passage from Lydia Patton. Running incommensurability together with speciation made it look like taxonomic incommensurability was just an observation about different scientific disciplines having different lexicons and different concepts. But Kuhn’s taxonomic development of incommensurability does not turn it into a merely semantic thesis, divorced from the practical issues that Patton rightly identifies in *Structure*, and which Kuukkanen rightly identifies in the speciation of scientific disciplines. Practical concerns shape structured lexicons, which in turn reinforce boundaries between specialties and create new disciplinary matrices. The incompatibility of incommensurable structured lexicons is not merely a matter of the difficulty (even merely the practical impossibility) of deploying different lexicons. Rather, an attempt to simultaneously use incommensurable paradigms creates incoherence.[[16]](#footnote-16) Therefore, while I am much in sympathy with recent work that identifies Kuhn as an important pre-cursor to the ‘practical turn’ in philosophy of science, I believe that too closely identifying the thesis of incommensurability with issues that arise in connection the speciation of scientific disciplines, and with the attendant difficulties of interdisciplinary work, is to run together two closely related, but nevertheless distinct, issues in Kuhn’s work.

1. **Conclusion**

Kuhn’s claim of incommensurability has often thought to have been watered down in his works after *Structure* to something as uninteresting as the observation that even if terms in different scientific paradigms co-refer, they still express different concepts. In this paper I’ve tried to suggest that viewing the meaning of a term in either the fashion of descriptivism, or of the causal theory, or of a hybrid account, makes Kuhn’s phenomena of incommensurability impossible to get into view. If all there was to meaning was sense or reference, then successful interpretation could overcome incommensurability completely, and not only facilitate comparison and communication. But becoming bilingual, for Kuhn’s still leaves one unable to simultaneously apply incommensurable structured lexicons The reason for our inability to do this Kuhn thinks, is that a normal science must make use of a taxonomic structure that obeys a no-overlap principle, and this, in turn, distinguishes incommensurability from just the speciation of scientific disciplines, and the attendant development of new lexicons. Meanwhile, from the perspective of the historian of science, insofar as we can produce an adequate translation of the earlier time, we will reproduce something like the rules or explication produced in a period of crisis and not the meaning of the kind term *in use* – as part of an open-ended system, continuously expanding to new instances according to non-inferential processes, such as analogy, or recognition by differentiae. To recapture that would take not just interpretation, but *actually* doing the past science with the full commitment required of the practitioner of a prevailing normal science.

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1. In section 1, I examine some detailed statements of this view from John Michael and Miles Macleod, David I Miller and Michael Levin, but other, briefer expressions of this point are widespread. Raatikainen argues that just attempting to limit semantic externalism by preserving some internalist common concepts is “sliding dangerously close to” “incommensurability … á la Kuhn and Feyerabend” (2020, 61). Bird argues that Kuhn’s semantic incommensurability follows from rejecting the first of two conditions held by the Positivists on the meaning of a theoretical term: “The meaning of a theoretical term is a product of two factors: the relationship of the theory or theories of which it is a part to its observational consequences and the role that particular term plays within those theories” (2022, 4.3). [↑](#footnote-ref-1)
2. I am very grateful to an anonymous reviewer for suggesting the phrasing of this sentence and this way of putting my point. [↑](#footnote-ref-2)
3. Another author who has discussed these developments in Kuhn’s view is Ian Hacking. I discuss Hacking’s “nominalist” reading of Kuhn’s taxonomic incommensurability thesis in section 3. [↑](#footnote-ref-3)
4. An anonymous reviewer asked, “is it even plausible to think that Kuhn had anything like a clear theory of meaning in the philosophical sense?”. I believe (as I believe the reviewer believes) that the answer is no.Past commentators have nevertheless read exactly one such theory into Kuhn, namely the descriptivist theory. That same reviewer goes on to suggest that because Kuhn was not offering “a theory of meaning in the philosophical sense” that reading Kuhn as a descriptivist may nonetheless be a justifiable ‘rational reconstruction’. I hope in this essay to have undercut much of the supposed justifications of such a ‘rational reconstruction’. As for the reviewer’s broader suggestion that in interpreting Kuhn one may have to ‘rational reconstruction’ “willy nilly”, where the point of such rational reconstruction is to rephrase Kuhn’s views in the idiom of some canonical philosophical theory or another of today, I simply find this to be anathema to Kuhn’s own approach to the interpretation of historical science. In this connection, I am strikingly reminded of Kuhn’s criticisms of Imre Lakatos’s ‘historical reconstruction’. Here is Kuhn on that point: “what Lakatos conceives as history at all is not history at all but philosophy fabricating examples. Done in that way, history could not in principle have the slightest effect on the prior philosophical position which exclusively shaped it. That is not to say that historical reconstruction is not intrinsically a selective and interpretative enterprise, nor that a prior philosophical position has no role as a tool for selection and interpretation. But it is to insist that, in the only sort of history that can hold philosophical interest, a prior philosophical position is not the only selective principle and also that it is not, as a selective principle, inviolate. When one’s historical narrative demands footnotes which point out its fabrications, then the time has come to reconsider one’s philosophical position” (1970, 143). [↑](#footnote-ref-4)
5. For a similar, but more detailed version of this objection to viewing scientific kind terms as name-like rigid designators see Curiel (unpublished). [↑](#footnote-ref-5)
6. See representative statements from both philosophers see Jackson (1998) and Lewis (1994, 424). [↑](#footnote-ref-6)
7. David Lewis outlined the method of using Ramsey-sentences (named after Frank Ramsey) in Lewis (1970). Using sentences of this type to define theoretical terms is now often called the Ramsay-Lewis method. [↑](#footnote-ref-7)
8. Part of Kuhn’s criticism of Ramsey sentences seems just to be a misunderstanding of what Lewis wanted them to do. Kuhn, for instance, says that “Lewis’ Ramsey sentences determine reference only on the assumption that the corresponding Ramsey sentence is uniquely realizable” (Kuhn, 2000a, 46). Lewis didn’t suppose his sentences would be uniquely realizable and this was a feature rather than a bug of his account as he understood it – because he was seeking precisely to understand terms that were multiply realizable either in the actual world or in other possible worlds. For that reason, I focus only Kuhn’s criticism of the idea that a ‘Ramsey’ sentence determines a sense. [↑](#footnote-ref-8)
9. An anonymous reviewer, correctly perceiving that I want to suggest a kinship between Kuhn and the later Wittgenstein’s famous formulation that “meaning is use”, suggested a possible tension here with my argument that Kuhn should not be read as a holistic descriptivist. As the reviewer put it, “if Kuhn is identified as a use theorist of meaning … the use theory is in most interpretations a holist theory of meaning.” Two points are worth making in this connection. First, although I intend a kinship between Kuhn and the later Wittgenstein, I do not intend any kinship between Kuhn and any of the (supposedly) Wittgenstein-inspired use-based theories of meaning that have been offered. I said previously that I did not see Kuhn as offering a theory of meaning, but as describing some of our linguistic practices so as to provide constraints on possible such theories of meaning. I read the later Wittgenstein as doing something similar – as primarily describing our practices with a view to showing that general theories of meaning won’t fit the various uses of language, and our craving for such a theory to explain our practice is bound to be disappointed. Second, although there is some sense in which Wittgenstein is holistic, I do not think that it is the global sense of holism that was wrongly read into *Structure*. For a useful discussion of how Wittgenstein’s holism differs from that of thinkers like Davidson, or from the target of Fodor and Lepore’s *Holism:a Shopper’s Guide*, see (Finkelstein 2007, 253 – 256, 260 – 262). [↑](#footnote-ref-9)
10. The closest Kuhn comes to endorsing a cluster theory of concepts is in fact in a 1951 lecture, where he offers “a rough picture” of the concept ‘dog’ as consisting of three layers of concentric “generalizations” – with the most central generalizations being the most certain (1951/2021, p. 143). Melogno and Giri (2023) have done an excellent job detailing how Kuhn was interested in questions of semantics and theories of meaning long before his more explicitly philosophical writings beginning in the 1990s. Melogno and Giri make clear that Kuhn had abandoned a cluster theory at least by 1980 (397), but I would argue that Kuhn had at least diverged from that view as early as *Structure*. I find much of Kuhn’s diversions from the cluster theory to have come from his reading of Ludwig Wittgenstein’s *Philosophical Investigations*, a reading which was no doubt inspired by discussions with his Berkeley colleague Stanley Cavell. This is an ironic source of divergence since the cluster theory is often said to have been “developed” or “popularized” by Wittgenstein (Melogno and Giri 2023, 392). Kuhn’s reading of Wittgenstein, which diverges from the cluster theory, is first clearly articulated in “The Priority of Paradigms” chapter of *Structure*, wherein Kuhn argues that the development of a scientific paradigm, or classification more broadly, need not be governed by anything like rules, even tacit ones with some vagueness – which are exactly what cluster theorists posit. I detail this view of Kuhn’s more fully in the present section. [↑](#footnote-ref-10)
11. A rare exception in the literature on rigid designation who has noted that Kuhn is more sympathetic to the notion than usually recognized is Joseph Laporte. Laporte correctly identifies Kuhn as holding not that terms fail to designate rigidly because of a descriptivist theory of meaning, but as holding that redubbing takes place between scientific revolutions causing scientific terms to change from designating one designatum rigidly to rigidly designating another (LaPorte 2013, 57). LaPorte himself argues that the notion of rigid designation by itself does not secure continuity of meaning or reference across theory change and is best separated from anti-descriptivism or causal theories of reference (49 – 53). The real work of rigid designation for LaPorte is in securing the metaphysical necessity of various identity statements. In what follows I will argue that Kuhn used the term ‘rigid designation’ in an importantly different way from Saul Kripke and the philosophers who followed him. LaPorte does not acknowledge these differences, as is understandable as he briefly discusses Kuhn’s view as part of a larger examination of the notion of rigid designation of philosophers following Kripke, of which Kuhn is not representative. LaPorte’s reading does leave Kuhn with a view according to which any kind term refers to an abstract property or universal – somehow existing independently of any instances. I will argue in this section that Kuhn should not be read as endorsing such a commitment. [↑](#footnote-ref-11)
12. See LaPorte (2013) for a sustained defense and development of this understanding of rigid designation regarding property terms. [↑](#footnote-ref-12)
13. Ultimately, I think, a traditional Tarski style referential semantics will not be the best setting for reconstructing Kuhn’s views about the meanings of scientific terms. Although, Tarskian views are the orthodoxy – there are some recent suggestions in the literature for alternative views, that I believe may prove more hospitable surroundings for a reconstruction of Kuhn. The first is the notion developed by Mark Wilson of a patchwork concept in his (2006). The semantics of such concepts are not settled by their referents – instead, one concept can comprise multiple ‘patches’ of application – each sitting over a different referent or physical property, or no physical property at all. What ties the concept together is instead a “bundle of directivities”. Phillipp Haueis (2024) has recently provided a concise summary of the idea which relates Wilson’s notion to other work in the philosophy of science. Haueis also does a nice job of summarizing how Wilson’s view incorporates elements of both semantic internalism and externalism, without being a standard ‘hybrid’ account. The second suggestion that might prove a useful alternative to Tarskian semantics comes from Erik Curiel who argues a theory’s semantics must be based upon pragmatic considerations he terms “propriety”, rather than primarily upon ontology. Curiel (forthcoming) focuses on theories which can be divided into a kinematics and dynamics and argues that the kinematics should be understood as describing the theory’s regime of propriety – constraints upon any physical system to which the theory can be applied in order to yield true or false statements. Detailing the relation of Kuhn to either one of these suggestions is well beyond the scope of this paper, but both seem to have some affinities with the suggestions I extract from Kuhn here. [↑](#footnote-ref-13)
14. An anonymous reviewer commented that an earlier draft left “in the dark … how the problems to be solved find their way into the structure of the respective "lexicon"”. This is a very worthy question, and I haven’t the space here to provide an exhaustive list, nor do I think that such a list could be given once and for all. I’ve included this paragraph to sketch how some such considerations might shape structured lexicons. [↑](#footnote-ref-14)
15. I am grateful to an anonymous reviewer for pressing me to say more about the relationship between incommensurability how “it relates to the speciation and specialization of the field, too, and covers the emergence of new discourse, departments, journals, and so on”. That same reviewer also pointed me to the Kuukkanen paper cited in this section. [↑](#footnote-ref-15)
16. I believe that a comparison again with Wittgenstein may prove helpful here. In discussing contradictions, Wittgenstein writes: “The idea is that when I give you an order, there are the words—then something else, the sense of the words—then your action. And so, with ‘Sit and don’t sit’, it is supposed that besides the words and what he does, there is also the sense of the contradiction—that something which he can’t obey” (1975, 138). Simultaneously using merely differently structured (but not incommensurable) lexicons may be something which one can’t do practically – we can here talk of sense and of practical impossibility. In the case of ‘both Sit and not sit’ and of incommensurable paradigms we have nonsense, rather than a senseful but practical impossibility. [↑](#footnote-ref-16)