Kuhn, Nominalism, and Empiricism*

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In this paper I draw a connection between Kuhn and the empiricist legacy, specifically between his thesis of incommensurability, in particular in its later taxonomic form, and van Fraassen's constructive empiricism. I show that if it is the case the empirically equivalent but genuinely distinct theories do exist, then we can expect such theories to be taxonomically incommensurable. I link this to Hacking's claim that Kuhn was a nominalist. I also argue that Kuhn and van Fraassen do not differ as much as might be thought as regards the claim that observation is theory laden.

1. Introduction. Thomas Kuhn adhered to some kind of scientific anti-realism. But, what kind? Kuhn took his anti-realism to be linked with his claim that scientific theories are incommensurable. But what is the nature of this link? Are his anti-realism and incommensurability independent but harmonious parts of an overall package? Or does one view follow from the other?

Anti-realism covers a multitude of sins (or virtues if you prefer). The species with which I shall be primarily concerned, and which I ascribe to Kuhn, is epistemological, scientific anti-realism. As I shall understand it here, the core of epistemological anti-realism, as applied to science, is a scepticism about scientific theories: our adherence to preferred theories is not rationally grounded as far as truth is concerned; belief in our theories does not confer knowledge. While I shall discuss mostly the sceptical element of epistemological scientific anti-realism, that anti-realism typically adds to scepticism the view that the purpose of theories is not to describe some feature of the world, but is instead something else (instrumental utility or puzzle-solving power for example).

Kuhn's name is often associated with a kind of metaphysical anti-realism. Taken literally, the thesis that worlds change when paradigms change

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asserts a dependence of the way the world is on what we believe about it. Such a dependence is a mark of idealist or constructivist metaphysical anti-realism. However, Kuhn himself rejected such an interpretation, which is in any case inconsistent with passages neighbouring the one which gave rise to it (Kuhn 1970a, 150). Most commentators take Kuhn's term 'world' not to mean the world of things but a world of appearances or of subjective connections (e.g. Hoyningen-Huene (1993)). I shall go along with the rejection of a significant metaphysical anti-realism of this sort.

There is another common understanding of Kuhn, which regards his incommensurability as leading to an epistemological anti-realism. On this view, incommensurability is the view that theories cannot be rationally compared. Thus we cannot know that our favoured theories are better than rival but incommensurable ones. Hence there is no rational reason to take our favoured theories to be true and so knowledge of their truth is not possible. The conclusion, that beliefs in our theories are not rationally grounded and cannot amount to knowledge, is a form of epistemological anti-realism.

The commonly held view just sketched is however a misreading of Kuhn. Indeed Kuhn (1983) denied the premise that incommensurability means non-comparability. Howard Sankey (1997, 1998) analyzes the various forms of incommensurability adopted by Kuhn at different points in his career. Most of Kuhn's work on incommensurability was dominated by the conviction that incommensurability should be understood as some kind of untranslatability. There is no straightforward argument from untranslatability to non-comparability.[1] In particular, argues Sankey, the latest (and most plausible) version of incommensurability, taxonomic incommensurability, is consistent with scientific realism. The argument sketched from incommensurability to epistemological anti-realism fails for taxonomic incommensurability.

Nonetheless, there is no denying that Kuhn was indeed an anti-realist. This is masked to some extent by the fact that Kuhn did believe that scientific progress occurred and for rational reasons. But this is not a realist's conception of progress. Kuhn explicitly takes a sceptical attitude towards the claim that theories can be assessed for truth or truth-likeness (Kuhn 1970a, 206). Instead, science aims at puzzle-solving. And so progress for Kuhn is a growth in problem-solving power and is *not* a matter of accumulating knowledge or improving verisimilitude (Kuhn 1970a, 163ff). We are not entitled, according to Kuhn, to infer from an increase

1. Kuhn does seem to think that incommensurability does lead to a failure of certain specific kinds of comparability, for example what he calls 'point-by-point' comparison. Of course not all relevant comparisons need be of this kind. (See Kuhn 1970b, 266 and Bird 2000, 152–155.)

in problem-solving power that a later theory embodies more knowledge or is closer to the truth than its predecessor.

So although it is true that Kuhn was an epistemological anti-realist while adhering to a thesis of incommensurability, it is not the case, contrary to the common view, that his anti-realism follows from the thesis of incommensurability. The thesis of incommensurability is too weak to support such an inference. This then raises the question whether there is some other logical relationship between incommensurability and anti-realism that neither Kuhn nor his critics appreciated? The purpose of this paper is to show that there is. I shall argue that even if epistemological anti-realism does not follow from incommensurability, incommensurability does follow from epistemological anti-realism. There is a logical connection between the two strands in Kuhn's thinking, even if the logic is not quite as it is often taken it to be.

The most important kind of contemporary epistemological anti-realism in the philosophy of science is van Fraassen's constructive empiricism (van Fraassen 1980).[2] I shall start by assuming, along with the constructive empiricist, that for any given theory there is a distinct and logically incompatible theory that generates the same set of observational consequences. I shall argue that if this is true, we should expect the empirically indistinguishable competing theories to differ in their taxonomies. If constructive empiricism is right, then we would expect that cases where theory choice is most difficult and cannot be decided on straightforward empirical grounds will be cases of theories that have differing taxonomies, theories that, in Kuhn's terms, are taxonomically incommensurable. The conclusion is that for any given theory there exist *theories with different taxonomies* that generate the same set of observational consequences.

I shall further ague that we can tie this conclusion to a kind of nominalism that Ian Hacking detects in Kuhn. In brief, if every theory has a competing (actual or possible) theory that differs from it in taxonomy, then we cannot know that the universals posited by our theories correspond to the universals that there actually are. The traditional nominalist maintains that universals do not exist and that what we call properties are reflections of our naming practices. I shall ascribe to Kuhn (or at least Hacking's version of Kuhn), a weaker, epistemological version of this view: Even if (scien-

2. There is debate as to whether constructive empiricism should be regarded as having the sceptical element which I attribute to it. Officially the core of van Fraassen's philosophy is a claim about the aim of science (science aims not at truth but at empirical adequacy). Nonetheless, the underdetermination thesis described in this paragraph is clearly also part of constructive empiricism, and this is what is what will be put to work in what follows. Furthermore, it is questionable whether van Fraassen's core position is tenable in the absence of a commitment to scepticism.

tific) universals do exist, we cannot know which there really are. The properties our theories talk about are thus not identical with actual universals but instead are reflections of (truth-independent) scientific practices.

My purpose is not to argue that the conclusions concerning incommensurability are true, nor do I wish to suggest that I adhere to constructive empiricism. Rather, my purpose is to show that the one entails the other, that if one is a constructive empiricist, then one is committed to certain theses of incommensurability. This is an interesting result in itself. Additionally, even though I am not claiming to reconstruct Kuhn's thinking, I do nonetheless think that the case helps bolster a certain thesis concerning Kuhn. Kuhn regarded himself as the scourge of positivism and certainly he helped considerably in hastening its demise. Yet, it has been claimed, Kuhn's philosophy of science betrays a continued commitment to many empiricist, even positivist assumptions, e.g. a theoretical-context view of meaning and an internalism about knowledge and rationality. (In Bird 2002 I argue for this view at length. See also Newton-Smith 1981, 151–154.) If it turns out that Kuhnian incommensurability is entailed by empiricism then we have yet further reason to believe that Kuhn's rejection of empiricism was not quite as revolutionary or thoroughgoing as it may have seemed to him to be.

2. Constructive Empiricism and Taxonomic Incommensurability. According to constructive empiricism our theories are radically underdetermined by the data. There are many, typically infinitely many, possible hypotheses that are consistent with our evidence. Certain rational principles will allow us to reduce the range of viable hypotheses. But since any rational argument for or against a hypothesis is based on observed data, our principles of theory preference can be sensitive to the truth only of the observational portions of our hypotheses. Therefore such principles can at best only select those theories which are likely to be *empirically adequate*—those theories that have true observational consequences (but may have false unobservable consequences). Van Fraassen rejects as rationally ungrounded (indeed as rationally objectionable) principles such as Inference to the Best Explanation that would allow us to pare down the number of viable hypotheses to a small number. We are not entitled to regard as true even our favourite, best confirmed theories if they have consequences regarding unobservables.

Constructive empiricism leads to *taxonomic incommensurability,* which is the sort of incommensurability that in his later work Kuhn attributed to theories. Two theories are taxonomically incommensurable when there is no straightforward translation between taxonomies of the two theories. This is exhibited, for example, by chemical theories before and after Lavoisier. Eighteenth century chemists talked of 'principles,' a term that has no translation in the language of nineteenth century chemistry, while the latter referred to 'elements,' meaning by that term something quite different from the same word as used by Priestley and others.

I shall be arguing for the following proposition:

> (CE→TI) If, as constructive empiricism tells us, there exist pairs of empirically equivalent (possible) theories, then we may expect of any given such pair that the two theories are taxonomically incommensurable; that is they have differing taxonomies that cannot be translated one into the other or into some shared taxonomy.

The argument from constructive empiricism to taxonomic incommensurability proceeds as follows. First, we may expect that empirically equivalent but distinct theories have differing taxonomies. This I show in the next subsection (§2.1). Secondly, differing taxonomies are typically not translatable into one another, as I discuss in the subsequent subsection (§2.2). Hence if two distinct theories are empirically equivalent, we may expect their taxonomies to lack a translation between them—they are taxonomically incommensurable.

Before proceeding it will be helpful to introduce a caveat. In (CE→TI) there is a reference to '(possible) theories.' The force of 'possible' here is to remind ourselves that constructive empiricism does not claim that the theories in question are actually entertained by anyone. Indeed it is surprisingly hard (as we shall see) to come up with convincing non-trivial examples of empirically equivalent theories. So the existence of theories here is best understood as a matter of the existence of sets of propositions, not a matter of the existence of sets of beliefs held by real scientists, or even just entertained by them. This is important for what follows. For I shall be discussing what properties such theories must have in order for constructive empiricism to be true. Some of these properties would be easily discerned were the theories actually entertained. But since they are not typically entertained, the discussion has to be more abstract. For example, taking the conclusion asserted in the final sentence of the preceding paragraph, we should be able to tell whether two actually entertained theories are taxonomically distinct or not by an inspection of their vocabularies. But constructive empiricism requires us to consider not only actually entertained theories but also sets of propositions that have not been entertained. It is about the structure of these that I will making inferences. So the conclusion I draw may be stated thus: 'If constructive empiricism is true, and every theory has an empirically equivalent but distinct theory, then were we to entertain both theories, we should see that they are taxonomically different.'

2.1. Empirical Equivalence and Taxonomic Difference. Theories may be akin to one another—or unlike—in a variety of degrees.

(1) Theories are *structurally* similar yet distinct if they posit the same kinds of things and quantities, linked in structurally similar equations, differing only in that they give different values to constants appearing in those equations. So quantum theory A and quantum theory B would be structurally similar if they differed only in the value they attributed to Planck's constant.
(2) Theories are *taxonomically* similar but *structurally* dissimilar if they hypothesise different equations or mechanisms, but would nonetheless posit the same set of quantities and kinds. Below we shall see that competitor theories to Newton's law of gravitation, from Clairaut and Euler show this sort of difference.
(3) Theories are *taxonomically* different when they posit different basic kinds and quantities. Priestley's phlogiston theory and Lavoisier's oxygen theory of combustion are taxonomically different.

In this section I shall argue that we have good reason to think that if van Fraassen is right, that two theories may be distinct but empirically equivalent, then they will exhibit the *greatest* degree of difference—taxonomic difference. At first blush this might seem odd. Empirical equivalence is a kind of similarity between theories. Would not one expect theories that are similar in respect of their empirical consequences to be similar also in the ways I have discussed (i.e. structurally—and hence taxonomically—similar)? I shall argue that quite the opposite is the case, on the ground that differences short of taxonomic difference are the differences between theories that are most likely to show up as empirical differences.

Let us start by considering theories of kind (1) above, those that are structurally similar, yet distinct. If the distinct but structurally similar theories are mathematical then they hypothesize the same quantities related in identical equations but attribute different values to some constant employed in those equations. Should we expect these theories to be empirically equivalent?

One way we would get something like empirical equivalence would be where the values attributed by the two theories were extremely close. Since the values are different, the theories are distinct, but since the values are so close the difference in the observational consequences are so small as to be observationally undetectable.

It is clear that van Fraassen does not have this sort of case is mind when he thinks of empirically equivalent but distinct theories that would make constructive empiricism true. That very similar theories might generate undetectably similar consequences is not an interesting thesis. Furthermore, if this was all there was to constructive empiricism then it would allow for the knowable truth of some theory, where the value of a constant is not given exactly but within a margin for error.

It is significant that it is the limits of *experimental* accuracy and sensitivity that determine the margin for error. This is because the value of a constant is the province of experimental determination. Once the structure of quantum theory is fixed, it is up to experiment to find the value of Planck's constant. Given the structure of a theory, the requirements of empirical adequacy will be sufficient to determine the value of the constant (within a margin for error determined by the limits of experimental accuracy). Van Fraassen himself discusses Millikan's experiments to measure the change on an electron. Atomic theory at the time, he says, had blanks in it (such as the value of electronic charge), and experiment "shows *how the blank is to be filled if the theory is to be empirically adequate*" (van Fraassen 1980, 75; van Fraassen's italics). In general, the fixing of quantitative features of a theory will be determined by empirical considerations, while the qualitative aspects of a theory will be answerable to what in van Fraassen's terms are non-empirical explanatory considerations.

In a theory with more than one constant, changes in the value of one constant might compensate for changes in the value of another. But if this can be done in a mathematically precise way, this suggests that the theory contains, in effect, one constant not two. In the simplest possible case: cF=kG allows infinitely many pairs of values for c and k, but really what we have is F=hG where h is fixed. In other cases the relationship between constants is not so much mathematical as scientific. So in the early- and mid-nineteenth century determination of the atomic weights of elements was fixed, in part, by beliefs about the ratios in which they combined to form compounds, while at the same time the latter depended on the former. Disagreements about the structure of one compound would lead to disagreements about the atomic weights of its constituent elements, which in turn would lead to differences about the constitution of some other compound. One might think of this as a case where there could be different sets of atomic weights and molecular structures each of which fitted the empirical data. Early on it was indeed the case that the attribution of weights and structures was underdetermined by the empirical data. But they were underdetermined by the actual data possessed at the time, not underdetermined by possible data—and it is the latter that is relevant to empirically equivalence. And as it turned out no further theoretical assumptions were required to permit a consensus to be reached; all that was required was the careful collection of ever more empirical data. Given that one believes in the atomic hypothesis and the consequent molecular account of substances, the empirical evidence, once there is enough of it, will fix the weights and structures one attributes to those atoms and molecules.

We may conclude at this stage, therefore, that if constructive empiricism is true and so theories have empirically equivalent alternatives, then we should expect those different but empirically equivalent theories to differ in more than just the values they attribute to constants and the like. The theories will have at least to be structurally dissimilar.

We must now turn attention to pairs of theories falling into category (2) above, those that are structurally different yet taxonomically similar. If the theories are mathematical, both theories will employ the same quantities but in different equations. Is it possible for theories differing in this way to be empirically equivalent? The same considerations at work in the preceding discussion operate here.

There are historical cases that hint at the possibility of structurally dissimilar equations being empirically equivalent. It was observed that the Moon's perigee precesses around the Earth. Newton himself could account for at best only half of the observed precession. So difficult was it to reconcile the observations with Newton's account of gravitation that mathematicians began to speculate that Newton's $F = m_1 m_2 / r^2$ is not strictly accurate. Euler and Clairaut considered alternative formulae involving the same quantities: $F = m_1 m_2 / r^{2+c}$ and $F = [m_1 m_2 / r^2].[1+(k/r^2)]$ (with small c and k), both of which yield a small precession in a positive direction. Might not we find ourselves in the position of being unable to choose empirically between the competing hypotheses? The two sorts of case where in practice this might arise are parallel to the cases considered with respect to different values of constants. If the alternative formulae give exactly the same values for F for the same values of the arguments m_1, m_2, and r then the supposed alternatives are after all equivalent and should not be regarded as genuine competitors. Thus the competition between the alternative matrix mechanics and wave mechanics versions of quantum mechanics was deflated once these were proved to be mathematically equivalent. On the other hand the formulae may not be equivalent, yielding different values for F. Yet the values for F may be so close that there is no detectable difference in the orbits that the alternatives predict. While this might indeed make for empirical indistinguishability in practice, it would not count as a genuine case of empirical equivalence, any more than minute observational differences due to a very small difference in the value attributed to a constant would make for empirical equivalence. Minute differences in observational consequences are still differences and theories that differ even only slightly in their empirical consequences are not empirically equivalent.

It looks then that so long as they are taxonomically similar (they relate the same kinds and quantities) even structurally dissimilar theories may be expected to be empirically dissimilar. If the theories generate identical predictions, then that will be because the theories are in fact mathematically equivalent—in which case the theories are not genuinely distinct. Or, if they are mathematically not equivalent, then the differences will be reflected in differences of observational consequence, however small.

We may conclude that taxonomically similar but genuinely distinct theories will be empirically distinct. From this it follows that if two distinct theories are indeed empirically equivalent, then they will be taxonomically dissimilar. This is not to endorse van Fraassen's view that for every theory there is a distinct but empirically equivalent theory. Rather it is to say that *if* van Fraassen were right, *then* the competing but empirically equivalent theories will differ taxonomically. (If one could show that even taxonomic differences would show up as empirical differences, then that would with the forgoing amount to a refutation of constructive empiricism.)

Since van Fraassen does not supply much in the way of example and because his view is in any case highly controversial, it is not possible to give an uncontentious illustration of the claim that empirically equivalent theories will be taxonomically different with a case of particular scientific theories that meet these specifications. I will shortly examine one example from van Fraassen, concerning the motion of the centre of the universe. Unfortunately this turns out to be a good example for neither constructive empiricism nor taxonomic difference. A better scientific example for both is the debate between Berthollet and Proust on the nature of chemical reaction and chemical compounds. I will come to the latter in the next subsection. However a good example of how sensory equivalence (identity of sensory effect) between two distinct states requires taxonomic difference is to be found in philosophy. Arthur has the experience as of seeing a yellow narcissus while in a room with yellow walls. One theory that will explain his experience is that Arthur is looking at a yellow pigmented flower of the genus narcissus while in a yellow painted room while his senses are functioning normally and so on. This story can be spelt out with more scientific detail. What other hypotheses will explain the datum? So long as there is no explanatory redundancy (and I shall return to this issue), slight changes to the hypothesis will render it unsatisfactory. For example, if we suppose that the flower is white not yellow, then the theory no longer predicts that Arthur will have the experience as of a yellow flower. To accommodate that sort of change we would have to change the theory in deeper respects—for example, by adding the additional hypothesis that Arthur is suffering from jaundice. The latter addition means that the overall theory is taxonomically different from the one with which we started. We have introduced a new kind (jaundice) that even belongs to a type of kind (disease) not mentioned in the original theory. Even in this case we might expect the change to the theory to show up as a difference in sensory experience once the range of sensory data is widened. But of course there is one theory that will predict exactly the same sensory experiences as the initial theory—it is the theory that Arthur is in fact a brain in a vat being manipulated into having the sensory experiences as of seeing a yellow flower in a yellow room. Notice that this theory is taxonomically very different indeed from the first. When spelt out fully it introduces new kinds such as brains in vats, computers, mad scientists etc., and can do without the kinds mentioned in the first theory (since it does not need to hypothesize kinds like flowers, pigments, and so forth in the explanation of the how the computer generates the relevant sensory experiences).

The last example shows that the theories that predict the same sensory states in Arthur are those that differ *most* in their taxonomies (the quantities and kinds of thing they hypothesize), while theories that differ slightly are likely to differ in their predictions. An important proviso is that the theories in question should not contain elements that are theoretically redundant. For these can easily be changed, in non-taxonomic ways, to generate empirically equivalent theories. Returning to the example of Arthur's sensory experience, the original theory could have asserted that the day on which the experience occurred is a Monday. That addition plays no role in generating the prediction and so a hypothesis that differs by saying that the day is a Tuesday will predict the same experience for Arthur. But we should not regard this as a counterexample to my thesis, for the claim that theories have empirical equivalents must be restricted to theories that do not have theoretical redundancy—otherwise van Fraassen's claim becomes trivial.

I do not believe that van Fraassen has himself entirely respected this requirement in the one example he does give of empirical equivalence. This is important because his example is a prima facie counterexample to my thesis, since it involves a set of theories that differ only in the values that they attribute to a constant. The only explicit case of empirically equivalent theories provided concerns differing hypotheses about the velocity of the centre of gravity of the universe within a Newtonian framework which includes absolute space (van Fraassen 1980, 46). Let TN(v) be Newton's laws of gravitation and motion with the axiom that the absolute velocity of (the centre of mass of the) universe is v. Here the distinct theories for different values of v do not differ with regard to taxonomy (or structure)—they differ only with regard to a constant. This seems therefore to go against my claims. The example, however, depends on theoretical redundancy. The theories in question can be reformulated so that they are equivalent to TGM$\wedge$T(v) where TGM is Newton's theory of gravitation and motion and T(v) is the hypothesis that the universe is travelling at velocity v. TGM is common to all the theories and makes no assumption about the velocity of the universe. But TGM is sufficient to generate all the empirical consequences of the theories. Adding T(v) to TGM generates no additional observational consequences and plays no role in generating the observational consequences that TGM$\wedge$T(v) has. The competing hypotheses, the various T(v), are just redundant add-ons, not organic parts of empirically significant theories. So the hypotheses TN(v)

are empirically equivalent for the trivial reason that the respects in which they differ are theoretically redundant aspects of the theories. As Mach showed we can excise from classical physics concerns about the absolute nature of space without loss—a fact of which we are all aware anyway, because no-one ever takes the velocity of the universe into consideration when carrying out exercises in Newtonian mechanics. Hence van Fraassen's example is not a good example for his case concerning empirical equivalence and so it is not a counterexample to my claim that if there are empirically equivalent theories, they will differ taxonomically.

The conclusion we have reached is this. There is good reason to suspect that non-taxonomic differences are likely to show up in empirical differences. And so, if two theories are genuinely distinct but are also empirically equivalent, then we may expect them to be taxonomically different.

2.2. Taxonomic Difference and Taxonomic Incommensurability. To reach the conclusion that distinct empirically equivalent theories are likely to be taxonomically incommensurable, I need to show that if two theories differ taxonomically then they are taxonomically incommensurable. Kuhn makes clear, as does Feyerabend when defending a similar view, that taxonomic incommensurability means the lack of the possibility of translation between the taxonomies. It does not mean that a practitioner or adherent of one of the theories cannot understand the other. What is at issue when discussing translation is this: a term *t* of theory A cannot be translated within the taxonomy of theory B if there is no term in that taxonomy that has the same meaning as *t* and if no simple compound expression framed within the taxonomy of B has the same meaning as *t* (e.g. *t* is not equivalent to a conjunction or disjunction of two predicates of the taxonomy of B). It may be possible nonetheless to give an explication of the meaning of *t* using the resources of B. But, as Kuhn was well aware (1970b, 267), an explication of meaning may well fall short of providing a translation.

To show, for empirically equivalent theories, that taxonomic difference entails taxonomic incommensurability is straightforward, given the discussion in the previous subsection. Let us suppose that theories A and B are empirically equivalent but not taxonomically incommensurable. Since they are not taxonomically incommensurable, there must be translations of both theories into some common language with a single taxonomy (which may be the language of A or B). Let the translations of A and B be A* and B*. Since A and B are empirically equivalent A* and B* are also empirically equivalent. They also share the same taxonomy. But in the last subsection we saw that we should expect empirically equivalent theories to have *distinct* taxonomies. So the assumption that A and B are not taxonomically incommensurable is inconsistent with the argument of the previous subsection. That is, distinct empirically equivalent theories may be expected to be not only taxonomically different but also taxonomically incommensurable.

In *The Structure of Scientific Revolutions* Kuhn calls our attention to the controversy between Claude-Louis Berthollet and Joseph-Louis Proust (Kuhn 1970a, 132). While both men used the same expressions ‘mixture’ and ‘compound,’ they had quite different views concerning the theoretical significance of these terms and their extensions. Although Kuhn only developed the taxonomic notion of incommensurability rather later, this is a case where, projecting backwards, we can say that Kuhn takes the two men to have theories with distinct and incommensurable taxonomies. Berthollet adhered to the older affinity theory of chemical reaction. In Bethollet’s theory a ‘compound’ (let us use the term ‘compound$_B$’) is the product of a chemical reaction$_B$, where a reaction$_B$ is what causes and explains certain changes in the substances: effervescence, the production of light or heat, a change of state, etc. A mixture$_B$ by contrast would produce none of these effects, being a matter of the spatial proximity of many small quantities of each material. Mixtures$_B$ could in principle be separated by mechanical means. Proust’s theory, in place of the affinity theory, was founded on the law of constant proportions, which was regarded as the sign of a chemical reaction$_P$. And so on this theory a mixture$_P$ was any means of combining substances in a way that could violate that law. We have distinct theories and what Kuhn takes to be distinct taxonomies. Yet the theories look to be empirically equivalent. For example, Berthollet held that the fact that dissolving could produce heat showed that such solutions are compounds. But since those solutions could be created with a continuous range of strengths, those solutions were presented as refutations of the law of constant proportions. From Proust’s point of view these solutions were not refutations precisely because they were not compounds at all but mixtures (on the grounds that they did not obey the law of constant proportions). From a neutral perspective both theories can be seen as consistent with the apparently contentious intermediate cases (not only solutions but also alloys and combinations of gases), which could be classified under one taxonomy as mixtures$_P$ and under the other as compounds$_B$.

The Proust-Berthollet controversy is a useful illustration of how distinct theories that are empirically equivalent (or close to empirically equivalent) are taxonomically different. But I would not want to claim that it is a perfect instance of empirical equivalence. History may seem to suggest that when further theoretical developments are added (such as Dalton’s atomic hypothesis) the two theories can be pulled apart (in this case in a way that favours Proust). Even so, history doesn’t tell us that there is no hypothesis that when added to Berthollet’s affinity theory cannot give it an empirical advantage over Proust’s theory. Furthermore, history cannot tell us that there is no hypothesis *h* such that (Proust’s theory + the atomic hypothesis) and (Berthollet's theory + *h*) are empirically equivalent, matching the equivalence at the level of Proust's and Berthollet's theories alone. This is an issue for constructive empiricism that goes beyond the scope of this paper.[3] Even so, the arguments given above suggest that theories that are empirically equivalent in a given context of theory or of background knowledge, or are close to be empirically equivalent, will also be likely to be taxonomically incommensurable. As I remarked above, since my aim is to draw the logical connections between constructive empiricism and thesis of incommensurability (and not to support either), I am not obliged to present a convincing example of incommensurable empirically equivalent theories. (Indeed since I reject constructive empiricism I doubt that such theories exist.) The Proust-Berthollet controversy indicates the kind of perspective a constructive empiricist might be expected to take and why it involves taxonomic incommensurability.

2.3. Conclusions. To conclude this subsection I will briefly review the argument before placing it in context. I have been considering two alleged phenomena—the existence of a distinct empirically equivalent theory for any given theory and the existence of taxonomic incommensurability. My claim is that if the first phenomenon does indeed exist then so does the second (without my yet making any commitment to the existence of either). Constructive empiricism says that for any given theory there exists a second, distinct, and empirically equivalent theory. We may not actually possess that second theory—indeed we rarely do—but the relevant set of propositions nonetheless exists. The discussion has been about the character of any such second theory. The conclusion is that if it is genuinely distinct but non-trivially empirically equivalent to the first, then we may expect the two theories to differ taxonomically.

It was then a short step from arguing that they are taxonomically distinct to the conclusion that they are taxonomically incommensurable. That is, the relationship among theories required to make the sceptical element of constructive empiricism true will involve the relationship that Kuhn finally chose as his preferred account of incommensurability. An illustration of the argument can be found in comparing the languages required to describe empirically equivalent 'real' and 'sceptical' scenarios. Clearly we should not expect the reverse relationship to hold—not every case of taxonomic incommensurability will involve empirical equivalence, not even every revolutionary case. It may be that the terminologies of Newtonian and Einsteinian mechanics cannot be mutually translated; that fact on its own

3. An excellent discussion of the relevant issues, particularly the relationship between underdetermination and confirmation holism, is Okasha 2002.

does not rule out empirical differences between the theories. That said, the dispute between Proust and Berthollet nonetheless gives some indication of how in a real case a difference in taxonomic structure might well go hand in hand with a lack of empirical difference.

Furthermore, the relationship of taxonomic incommensurability is a weak one, one that is easily satisfied. Let *t* be a term of the language L which has no synonyms in L. Let L* be L minus the term *t*. Let it also be the case that *t* has no non-circular analysis into necessary and sufficient conditions expressible in L. Then *t* has no translation in L* and so L is incommensurable with respect to L*.

Let us call terms such as *t* the *atomic expressions* of L. The prevalence of the phenomenon of taxonomic incommensurability will depend on what proportion of a language is made up of atomic expressions. This is a question concerning which views have changed considerably during the twentieth century. Inter-war analytic philosophy held that the number of atomic expressions is quite small, limited to an important class of expressions, typically held to be ostensively defined, which provide the stock of basic expressions of a language in terms of which all the others can be given proper analytic definitions. In particular the theoretical expressions of science were held to be non-atomic, deriving their meanings from atomic expressions, which get their meanings by a correlation with observable things and properties. This view came under pressure from a number of directions. One source of trouble was the difficulty philosophers had in finding analytic equivalents for terms that could not be ostensively defined. The theoretical expressions of science proved a particular case of difficulty, most especially those terms ascribing dispositions, powers, or capacities to things. To some extent the double-language model preserved the intuition that observational terms provide the basis of all meaning in science. But this was at the cost of introducing a holism into the meaning of theoretical terms which in turn blurred the distinction between the analytic and synthetic propositions of a language. In due course Quine's all-out attack on that distinction undermined the very distinction between atomic and non-atomic expressions of a language. In effect every expression becomes atomic because no analysis into a synonymous equivalent is available. (It is no wonder that Quine's philosophy was, at first, attractive to Kuhn.[4]) Other reasons for thinking that atomic expressions are the rule

4. But only at first, since on the one hand this strict conception of translation, as requiring synonymy, renders incommensurability a trivial thesis (because translation is an impossibility for any pair of terms from any languages). On the other hand a weaker conception of translation suffers from an indeterminacy of translation, yielding too many translations, not the lack of translation required by incommensurability.

rather than the exception have been provided by Wittgenstein, Putnam, and Kripke. Wittgenstein drew our attention to the existence of family resemblance concepts. Terms such as 'game,' it is claimed, cannot be given necessary and sufficient conditions for their application, not even conditions using extended disjuncts, since the future application of such a term is in principle open-ended, being determined by a perceived similarity to some (but not all) previous uses. Consequently there is no adequate translation of family-resemblance terms within the rest of a language, and so such terms must be regarded as atomic in the current sense. For different reasons Putnam and Kripke have shown that theoretical and kind terms also lack analytic definitions. Just as in the case of proper names a certain indexicality in the process of reference fixing prevents that reference being captured by purely descriptive means. A natural conclusion is that such terms do not have analytic definitions and so are also atomic.

So the claim that there exists incommensurability looks to be a daring and worrying claim against the background philosophy of fifty years ago when Kuhn was working on *The Structure of Scientific Revolutions*. But by the time he had come to settle on taxonomic incommensurability as his preferred articulation of the idea, the background assumptions in philosophy had changed, so that today it would be widely accepted that new terms of a language will mostly not have an exact translation into the remainder of that language. Consequently we should expect the terms of a taxonomy of one theory to be incommensurable with the terms of the taxonomy of a competing theory.

One reason why this connection has not hitherto been seen is that Kuhn's incommensurability relation is typically billed as a *diachronic* relation, while it is natural to think of van Fraassen's empirical equivalence claim as being asserted of *synchronous* theories. Taking the latter first, the concern about underdetermination is a concern that for any theory under consideration there exists 'at the same time' another theory (even if existing only in the abstract) that is empirically equivalent. But turning to incommensurability, Kuhn came to believe in the phenomenon as a result of the initial incomprehension he had on reading Aristotle; the concept was then developed to describe the difference in world-views a scientist has before and after a paradigm-shift (Kuhn 1987) . In linking incommensurability and empirical equivalence I am in effect asserting that incommensurability also applies synchronically. While that is perhaps unusual against the background of Kuhn's primary application of the concept, it is nonetheless not in any tension with Kuhn's views. First, it should be remembered that we are dealing with *taxonomic* incommensurability. As I have explained, this is a fairly weak form of incommensurability that Kuhn developed later in his thinking, and may be expected to have a wider range of application than earlier, stronger versions of the idea. Secondly, even his earlier notion of incommensurability may be applied to synchronous theories. Scientific revolutions do not happen instantaneously. Kuhn describes how the nature of a paradigm-shift means that the debate between proponents of the new theory and more conservative adherents of the old theory may be beset by incommensurability (of more than one kind). While from a distance we may naturally think of one theory as the later theory and the other as the earlier theory, at the time of the debate they are synchronous. Kuhn gives many such examples in his discussion of incommensurability One such conflict was the Proust-Berthollet controversy discussed above; others include the synchronous incommensurable differences between Lavoisier and Priestley and between the Aristotelians and Galileo (Kuhn 1970a, 118).

3. Varieties of Nominalism. Ian Hacking interprets Kuhn's taxonomic incommensurability as a kind of nominalism. In order to understand properly what kind of nominalism it is appropriate to attribute to Kuhn and how it links to the forgoing discussion of incommensurability and constructive empiricism, it will be helpful to make a pair of distinctions among different kinds of nominalism:

(i) *naturalistic* nominalism *v. constructivistic* nominalism. This distinction turns on whether the nominalist in question thinks that the nominalist alternative to universals (e.g. resemblance classes) are answerable to genuine difference between things or thinks that our use of predicates instead reflects a division of things that is imposed upon the world by us.

(ii) *metaphysical* nominalism *v. epistemological* nominalism. This distinction depends on whether the nominalist rejects the existence of universals or rejects knowledge of universals.

Allow me to elaborate on these distinctions. Traditionally the debate between nominalists and realists has been a metaphysical matter: what is the nature of properties? Realists believe that properties are universals while the various kinds of nominalist are agreed that all entities are particular—there are no universals. Nominalists disagree among themselves however on what they think properties actually are if they are not universals. Many nominalists are what I shall call *naturalistic* nominalists. According to naturalistic versions of nominalism, the way particulars divide up into different kinds reflects real divisions in the world. For example, one version of nominalism, *natural class* nominalism, asserts that things happen to fall into different natural classes—this is a brute fact. Each natural class is a property. Another kind of nominalism *(natural) resemblance* nominalism says that properties correspond to the different groups things fall into in virtue of their resembling one another. The fact of two things resembling one another is a brute, unanalysable, natural fact. Both these kinds of nominalism are *naturalistic*. (For more on kinds of nominalism, see Armstrong 1978, 11–57).

Nominalists need not be naturalistic—they can be what I shall call *constructivist*. Naturalistic nominalists think of properties as being *natural* classes or *natural* resemblances among things and hence not dependent on our way of thinking of them. A constructivist nominalist will deny that properties are in any way natural. Rather, the only properties there are, are those that correspond to categories formed by our conception of things. According to Ian Hacking, Kuhn holds just this view (Hacking 1993). For Kuhn, there are no natural properties, nominalistically or realistically construed, which our scientific categories aim to match. Kuhn, says Hacking, "believes that the classifications, categories and possible descriptions that we employ are very much of our own devising" (Hacking 1984).

Although the nominalism adverted to is a metaphysical thesis, it is often motivated by epistemological concerns. Now the merit of arguments that have epistemological premises but which draw metaphysical conclusions is doubtful. Furthermore, one might have metaphysical reasons for thinking that universals must exist (because, for example, only with universals can there be laws and causes). Yet one might have epistemological reasons for doubting that we can be in any position to know which universals there are. In the light of this we need to identify a version of nominalism that remains exclusively within the realm of the epistemological—an epistemological nominalism.[5]

Within epistemological nominalism there may be different views, depending how far the underlying scepticism goes. So one might be an epistemological nominalist about some classes of universal but not others. For example, some epistemological nominalists might concede that if any universals reveal themselves in the observable world, then we may get to know of their existence. If colours are universals, for instance, then we may know these. But, such a view may also contend, when it comes to the level of theoretical remove that is the province of science, universals would be unknowable. Thus we have no reason to think that the classifications we actually use *in science* are closely related to the way the world is structured by universals. Instead our classifications are the products of our minds, and of our social and scientific inheritance.

As Hacking says, Kuhn is a nominalist. My claim is that he is an epistemological rather than a metaphysical nominalist and that he is a constructivistic rather than a naturalistic nominalist. His view is that we do not

5. An epistemological nominalist might be a metaphysical realist (a position with a Kantian flavour)—or might be an agnostic about the metaphysical question. Metaphysical nominalism entails a trivial epistemological nominalism.

know which universals there are; the properties we actually talk of are constructed by us, our psychologies and our scientific practices. This form of nominalism links, on the one hand, with epistemological anti-realism and, on the other, with incommensurability.

3.1. Kuhn's Epistemological Nominalism. If Hacking is right that Kuhn is any kind of nominalist, then a reason for thinking that Kuhn is an epistemological rather than metaphysical nominalist is the dearth of metaphysical argument in Kuhn. And even when Kuhn does engage with metaphysics, for example in his rejection of truth, verisimilitude, and the notion of what is 'really there,' his arguments are epistemological. (Kuhn 1970a, 206–207. Kuhn's arguments are elaborated by Hoyningen-Huene (1993, 263–264).) Kuhn is concerned with the development of scientific beliefs. His view is that these are not sensitive to what is really there but instead evolve in accordance with the need to maximise puzzle-solving power (constrained, at least in normal science, by the disciplinary matrix). Since our theories are primarily not about individual particulars but instead about the kinds and properties that differentiate particulars, the rejection of a match between our scientific beliefs and reality will be a rejection of the proposition that we have reason to think that the kinds and properties (the universals) posited by our theories exist.

In responding to Hacking's nominalistic interpretation Kuhn expressed concerns to the effect that it does not fully capture his view (Kuhn 1993, 315). On reason is that Kuhn wants to allow kinds to be at least potential referents of our expressions. Now, it is not impossible for a metaphysical nominalist to allow for reference to kinds, so long as what is being referred to is not a universal but is something else, e.g. a class. Nonetheless, a metaphysical nominalist will typically prefer to reject the notion of reference altogether, except as applied to concrete particulars. Kuhn clearly wants to distance himself from that sort of view: "I need a notion of 'kinds'. . . that will populate the world as well as divide up a preexisting population" (Kuhn 1993, 316). This is why I do not construe Kuhn as a typical metaphysical nominalist. The epistemological nominalist, by contrast, does regard the role of kind terms to be that of referring to kinds construed as universals. It is just that we cannot know whether we have succeeded in referring—for all we know, our attempts at reference are empty. Added to this is the constructivist element—what explains our current range of purportedly referring kind-terms is a history of revolutionary shifts in concept-application (which, although constrained by the nature of the world, is not however correlated with it).

3.2. From Constructive Empiricism to Epistemological Nominalism. We have already seen how constructive empiricism leads to taxonomic incommensurability. The same argument leads to epistemological nominalism. Constructive empiricism says we are not entitled to infer that a theory is true. Our beliefs (such as they are) that the world includes certain universals (kinds such as species and elements, properties such as being a compound etc.) are consequences of our favoured theories. But if we accept the advice of constructive empiricism and withhold belief in the *truth* of such theories we should correspondingly withhold belief in the existence of those universals. So constructive empiricism leads straightforwardly to an epistemological nominalism.

The forgoing argument needs some bolstering. Imagine that all the theories empirically equivalent to a theory T posit the same universals as T, and that we could know this. In such circumstances, even if we could not know that T is true, we could know that T's universals exist. So although our theories would be underdetermined, our beliefs in universals would not be. This is where the argument that constructive empiricism requires distinct empirically equivalent theories to be taxonomically distinct is important. For it shows that we should expect it *not* to be the case that all theories equivalent to T will posit the same universals. If constructive empiricism is right then our beliefs in scientific universals are as underdetermined as our beliefs in the theories that posit them.

4. Kuhn and van Fraassen. In the remainder of the paper I will look at three further but related topics where a comparison of Kuhn and van Fraassen is fruitful. These will help us judge whether the links already discussed are genuinely revealing about Kuhn (and perhaps van Fraassen also). First, I will consider theory choice. I shall suggest that Kuhn's picture of the development of scientific belief fits well into a view of theories which takes them to be underdetermined by the evidence. It is no coincidence that both he and van Fraassen offered an evolutionary view of progress. Secondly I will look at the relationship between theory and observation. This might seem to be an area where there is a wide gulf between Kuhn and van Fraassen. While acknowledging a divergence I will show that their views are not so far apart as some might initially suppose and that furthermore what genuine differences there are do not vitiate the comparisons made thitherto. And finally I shall look at the comparison between Kuhn and van Fraassen in the light of the neo-Kantian interpretation of Kuhn.

4.1. Theory Choice. If underdetermination of theory by evidence is not a genuine problem, then theory choice has a straightforward explanation. Background knowledge restricts viable hyphotheses to a manageable number. Tests, experiments, and observations are devised that will decide between them, and in due course enough evidence is gathered that will rationally constrain choice to a single hyphothesis.

On the other hand, if the evidence always does underdetermine theory choice, the range of hyphotheses constrained by background knowledge plus any amount of new evidence will remain potentially infinite. Even so, it is the case that scientists do typically consider themselves to be faced with a limited range of hyphotheses, and often the problem is not that they have too many hyphotheses that fit the data, but that they have too few or none at all. If evidence and reason are not all that limits theory choice, there must be other factors that do. Although such factors, since they do not rationally constrain theory-choice, are not rationally compelling, they are not necessarily irrational either. Van Fraassen's official view is a permissive one: it is not that we are forbidden from believing our favoured theories; rather it is that nothing compels us to.

So what rationally non-compulsory factors are they that, for those of us who do choose to believe, fix our choices? Or which, for those who desist from believing but nonetheless 'accept' and work with a favoured theory, selects that theory as one we accept? Kuhn's answer is of course that is the paradigm that plays this role. As van Fraassen notes (1980, 112) it is precisely the role of the paradigm to constrain choice of puzzle solution that would otherwise be rationally underdetermined.

It is important to recognise that Kuhn does not eschew rationally compelling techniques. Logic and mathematics do play a part in a scientist's thinking. What Kuhn rejects however is a logic of justification (as exemplified by Carnap's inductive logic) which would provide rules whereby a scientific conclusion could be shown to be required by the evidence. Paradigms operate in a manner akin (or related) to pattern recognition. A pre-eminently successful puzzle-solution becomes an exemplar. Subsequent researchers seek to replicate the exemplar in the solutions to their puzzles; similarity to the exemplars is also a criterion of the quality of a proposed puzzle-solution. Similarity between exemplar and puzzle-solution cannot be reduced to rules (which is not to say that the use of rules is absent from the operation of exemplars and puzzle-solutions). Recognising the similarity will always involves non-rule-governed judgments of the kind we make when we say that a child has a smile similar to the smile of one of its parents. So for Kuhn, despite what his critics have often accused him of, the idea of a paradigm does not amount to a rejection of rationality in scientific judgment but a rejection of the sufficiency of rules in scientific judgment.

Although van Fraassen seems to be willing to endorse the belief by a scientist that an empirically well-supported hyphothesis is empirically adequate, there is of course no guarantee that it is (if the underdetermination thesis is correct). On the contrary, it is reasonably likely that some current theories are not only wrong but empirically inadequate. An accepted theory that is empirically inadequate will in due course generate anomalies and ultimately a crisis. The theory will be rejected and will need replacing in a scientific revolution. Again, given the underdetermination thesis, there will be potentially infinitely many hyphotheses that will be consistent with the observable data. But now the paradigm/exemplar that previously constrained theory choice is no longer performing that function. In which case a rather greater range of hyphotheses will be available that are ruled out neither by rule-governed inference from the evidence nor by the constraint of similarity to the agreed exemplars of the paradigm. (Strictly speaking the old paradigm does continue to play some role, because it is the source of the anomalies that need solving and because any replacement paradigm must retain a large part of its predecessor's puzzle-solving power. These constraints may explain why even revolutionary theory-choice is not completely open-ended and anarchic.)

One move to make at this point is to appeal to 'revolutionary' modes of promoting a theory—propaganda, power, persuasion and so on—as the mechanism whereby the range of possible hypotheses is ultimately limited. Kuhn's one brief remark to this effect has since mushroomed in significance in the hands of sociologists of scientific knowledge. The latter have explicitly taken on board the thesis of the underdetermination of theory by data and have proposed social mechanisms to explain actual theory choice, not only in revolutionary cases but in the course of normal science also. As we have seen, this is not Kuhn's view as regards normal science. Nor does it properly represent his view of revolutionary science either. Even in revolutionary science past exemplars exert some force over the choice of their successors. Since the operation of exemplars is a psychological phenomenon, a conscious rejection of much of the theoretical import of an existing theory does not mean that a revolutionary scientist faces the data with a psychological tabula rasa. What a scientist takes to be the general form of a puzzle worth solving or to be the general form of a good solution to such a problem will still be the products of his or her training. And while revolutions bring revisions to existing theory, not everything is discarded. Thus Copernicus' revolutionary *De Revolutionibus* retains much of the theoretical content of Ptolemy's *Almagest* (circular motion, epicycles, Aristotelian physics) as well as its form (the structure of the chapters of *De Revolutionibus* follows that of the *Almagest*). Kuhn held that even for revolutionary science most of the factors influencing theory choice are mostly internal to science. Either way though, in the context of an empiricist gulf between data and theory, something needs to explain how theory choice actually comes about. Van Fraassen says little on the topic, but does recognise that simplicity and explanatory power do play a part. Kuhn offers a theory, the theory of paradigms-as-exemplars that shows how such factors operate.

It is worth adding that both van Fraassen and Kuhn advocate an evolutionary account of the progress of science: “Can we not account for both science’s existence and its success in terms of evolution from the community’s state of knowledge at any given time?” asks Kuhn (Kuhn 1970a, 171); “science,” says van Fraassen, “is a biological phenomenon,” (van Fraassen 1980, 39). They both reject the realist idea that the accumulating success of science in solving its puzzles is a sign of the increasing truth-content of science. In place of the realist explanation both provide evolutionary pictures. Van Fraassen tells us that we should not be surprised at the success of science since scientific theories are selected with view to their being successful in solving puzzles (van Fraassen 1980, 39–40). At greater length Kuhn portrays science as developing not by aiming towards the truth but rather by seeking mostly local but sometimes revolutionary improvements and changes in order to increase problem-solving power (Kuhn 1970a, 160–173).

4.2. Theory and Observation. I will now turn to an area where there seems to be considerable divergence between van Fraassen and Kuhn. Van Fraassen makes essential use of an observation-theory distinction, while Kuhn is widely thought of as undermining the distinction. Coming to a view on this matter will be important if it is to be accepted that the links between constructive empiricism and incommensurability are indeed revealing of a closer affinity than has been hitherto recognised. While there are important differences, I will argue that these differences do not vitiate the perspective I am presenting here. In particular Kuhn’s view of the relationship between theory and observation could be accepted by a modified constructive empiricist.

To understand where Kuhn and van Fraassen differ and where they agree concerning theory and observation requires careful attention to differences among the alleged phenomena that go under the title ‘the observation-theory (O-T) distinction’ as well as the phenomena that allegedly refute the proposed distinction(s). In particular we need make the following distinctions concerning the O-T distinction:

(a) a semantic O-T distinction from an epistemic O-T distinction;
(b) the claim that the (epistemic) O-T distinction does not exist from the claim that it is not fixed and invariable.

To orientate ourselves in this discussion it will be useful to remind ourselves of the logical positivist/empiricist background in which the distinction was introduced. The logical positivists and logical empiricists drew upon a perceived empiricist heritage according to which experience is held to be the fount (i) of all knowledge and rational belief; and (ii) of all meaning. As far as (i) is concerned the O-T distinction is important in order to distinguish what provides the justification (i.e. observation or experience) from what receives it (i.e. theory), that is, to distinguish the foundations of knowledge from the scientific edifice erected upon those foundations. As regards (ii), the idea that experience is the origin of all meaning, this was cashed out by the positivists in the claim that theoretical propositions are reducible to observational propositions, or, in the later double-language model, in the claim that the meanings of theoretical propositions are derived from (if not reducible to) the meanings of observational propositions by correspondence rules. So (i) gives us an epistemic O-T distinction while (ii) gives us a semantic distinction.

How exactly Kuhn's discussion of observation impacts on these distinction is not immediately clear from his own texts. He himself rarely referred to the distinction as such and never in any analytic detail. So it is up to us to 'reconstruct' Kuhn's philosophy on these points. The point on which most reconstruction is required relates to the semantic O-T distinction. While Kuhn makes clear his adherence to a contextual account of theoretical meaning (the meaning of a theoretical term is dependent on its role within the paradigm theory), he is less clear as regards the meaning of observational terms. Nonetheless, it is plausible to understand part of what is going on in his earlier discussions of incommensurability as being an extension of this account of meaning to observational terms.[6] All scientific terms depend for their meanings on the roles the play in a paradigm. As we shall see Kuhn held that paradigms could affect the content and nature of experience and observation. To the extent that paradigms influence observation they thereby influence the meanings of observational terms. This is not precisely the same as saying that the meanings of such terms are theory-dependent, for a paradigm goes beyond just a theory (amongst other things it involves techniques for applying the theory, techniques for making observations, and learned similarity relations). But since the theoretical component cannot be divorced from the paradigm of which it is a part the import of the paradigm-dependence of observational meaning is the same. There is no useful semantic O-T distinction where that distinction is supposed to demarcate a portion of scientific language that gets its meaning from observational experience entirely independently of any theoretical commitment.

In the current context, the precise import of Kuhn's philosophy for the semantic O-T distinction is not so important. For van Fraassen *also* rejects a semantic O-T distinction, in unequivocal terms: "Can we divide our language into a theoretical and non-theoretical part? . . . On [this question] I am in total agreement [with Grover Maxwell, who says *no*.] All our

6. See Newton-Smith 1981, 151–5 for such an interpretation.

language is thoroughly theory-infected.'' (Van Fraassen 1980, 14) The semantic (or meaning) dependence of observation on theory is perfectly consistent with the possibility of dividing truths into those that may be known by observation and those that cannot be. It will mean that the truths of the former class may not be expressible in terms independently of theoretical expressions. But the presence of a theoretical expression in a sentence does not make the proposition expressed unknowable by observation. ''This blood sample is infected with a bacterium that is a spirochete'' is a sentence containing theoretical terms whose truth may be ascertainable by observation.[7] Van Fraassen regards planets and stars as observable, even those not observable with the naked eye from Earth, since a suitably placed spaceman would be able to observe them. Thus supernovae are observable, even if the meaning of 'supernova' is semantically dependent on theories of stellar evolution.

So Kuhn's contribution to the undermining of the semantic O-T distinction does not differentiate him from van Fraassen. The significant question is whether they differ as regards the epistemic O-T distinction, and if so how. While Kuhn says much that is related to this question neither he nor subsequent commentators have been sufficiently careful in delineating precisely what the impact of his claims on the epistemological O-T distinction ought to be. The positivists/empiricists needed a distinction that fulfils the following condition concerning the Epistemological Observation-Theory Distinction:

> (EOTD) There is a class of observational propositions that is distinct from the class of theoretical propositions such that our judgments concerning the latter are ultimately dependent on our judgments concerning the former.

An empiricist or positivist may well believe rather more about the distinction between observational and theoretical judgments than is contained in (EOTD). But as far as that distinction is concerned, (EOTD) is sufficient for the more negative aspect of constructive empiricism.

Kuhn's central claim about observation may be summed up as a claim about the phenomenal dependence of observation on theory:

> (PDOT) The phenomenal nature of observational experience (the content of observation) can depend on the theoretical commitments of the observer.

Kuhn (to a large extent following Hanson) claimed that the observational experiences of the Aristotelian and Galileo on looking at a pendulum are

7. If microscopes do not preclude observation (which they do, for van Fraassen).

different, thanks to their differing background beliefs (Kuhn 1970a, 118–119). He points to psychological evidence in favour of this claim, for example gestalt images, and the Bruner and Postman experiments, which showed that subjects' phenomenal experience of doctored playing cards was strongly influenced by their background expectations (1970a, 62–64).

I will ignore the question whether (PDOT) is true. What is important is whether (PDOT) undermines (EODT). It is often assumed that it does. But that is mistake.

Note that (PDOT) is a claim about the nature of experience, while (EODT) is a claim about classes of propositions. (PDOT) says that observational experience varies with theoretical commitment. That could be so while leaving a distinction between observational and theoretical propositions. It could be that although observational experience varies, there is a class of observational propositions unaffected by that variation. For example, let it be the case that the Aristotelian and Galileo have different experiences when looking at the pendulum, thanks to their differing theories. It might remain the case that there is sufficient similarity between their experience for certain propositions to be available to both of them, for example propositions concerning the period of the swing, the length of the pendulum and so forth. For there to be no such class of propositions available to all normal observers, whatever their theoretical commitments, would require that every aspect of observational experience is potentially variable thanks to differences in theoretical commitment. (PDOT) does not go as far as that and Kuhn does not present any argument that it does. If true (PDOT) only undermines the claim that *all* observational claims made in good faith can be relied upon as evidence that is independent of the theories we entertain; it does not undermine the claim that there are *some* such observational claims.

Perhaps Kuhn did think that all bona fide observational claims might be rendered sensitive to theoretical commitment, should the theoretical commitment be sufficiently unusual. That is, for any putatively observational proposition, there is always some theory such that acquiring a commitment to it would make a sufficient difference to one's observational experience that one would change one's belief in that proposition. Even so, that is consistent with there being a distinction between observational and theoretical propositions. Xs can depend Ys in various ways while Xs and Ys remain distinct. In this case one might want to distinguish observational propositions from theoretical ones on the basis of the causes of our commitments to them. Observational propositions would be those we are inclined to believe primarily on the basis of direct experience, theoretical propositions are those one in inclined to believe on the basis of some kind of inference from the observational propositions. While I am not suggesting that this is a satisfactory distinction, nor even that it is Kuhn's distinction, it remains that case that such a distinction satisfies (EOTD) while being consistent with (PDOT). For one might believe a proposition on the basis of experience even though that experience, and perhaps as a consequence that belief also, are sensitive to one's theoretical commitments. More generally the existence of an O-T distinction is consistent with the boundary between the two sets of propositions being changeable, even when such changes can be brought about by changes in theoretical commitment.

So there is no need to suppose that Kuhn's claims concerning the O-T distinction do undermine the claim that there is a distinction between observational and theoretical propositions. Nor is there reason to suppose that Kuhn intended to undermine that claim. What he was concerned to undermine was the positivistic claim that there is always a set of observational propositions we can rely on to be a final court of appeal in scientific disagreements, whatever the nature of the disagreement. (PDOT), if true, casts doubt on that claim, although it does not show it be definitely false. When Kuhn states that "the scientist can have no recourse above or beyond what he sees with his eyes and instruments" (1970a, 114), he makes clear his belief that there is indeed a class of observational judgments distinct from theoretical ones.

So, to conclude this section, we have seen that Kuhn is at most committed to a moveable O-T boundary, not the non-existence of such a boundary. Where that boundary lies depends on the theoretical commitments of the subject. What is needed for constructive empiricism is that there should be some O-T boundary, not that the boundary should be the same for all subjects. Indeed van Fraassen himself does not think that the O-T boundary is the same for all subjects. His view is that its location depends on the physiology of the subject. So a Kuhnian has all the materials at hand for an empiricist scepticism about our theoretical claims. (PDOT) does not take away from that but only adds some reason to be less confident than the empiricist concerning observational claims.

4.3. Kuhn, Kant, and Empiricism. Paul Hoyningen-Huene presents Kuhn as a dynamic neo-Kantian. (Hoyningen-Huene 1993) According to this interpretation the *phenomenal world* has two sides. One is the *world-in-itself,* which is that portion of the world whose nature and existence is by and large independent of our thinking about it. The other part of the phenomenal world is made up of what Hoyningen-Huene call 'subject-sided moments.' These are the aspects that are analogous to Kant's appearances, and whose nature is dependent on people and their minds (and their communities). Corresponding to this metaphysical divide is an epistemological gulf. The subject-sided moments are all that is epistemically accessible to us; the world-in-itself is unknowable. Since Hoyningen-Huene is keen to use this account to explain Kuhn's notion that the world changes when paradigms change, we may be tempted to focus on the metaphysical aspects of the positions; but if we emphasise the epistemological dimension we will be able to see the similarities with constructive empiricism. (Correspondingly, Hoyningen-Huene's textual evidence for his view may be used to defend this proposal for understanding Kuhn's position.)

The most pertinent and most obvious similarity is the division of the world into that which is epistemically accessible (the observable/subject-sided moments) and that which is not (the unobservable/the world-in-itself). Furthermore, the division is made in roughly the same place. The world-in-itself in inaccessible since, as noted, Kuhn says, "the scientist can have no recourse above and beyond what he sees with his eyes and his instruments" (Kuhn 1970a, 114). So long as Kuhn is not too generous as regards what may be seen with an instrument, this accords with van Fraassen's unobservable/observable distinction.

Of course, we should not downplay the differences too much. The first set of differences between Kant, van Fraassen, and Kuhn concern space, time, and causation. Kant takes these to be forms of intuition and a category, and so not applicable to things-in-themselves. Van Fraassen accepts the objectivity of space and time, but is ambivalent about causation. He thinks that we have no satisfactory philosophical account of it; the notion may even have no sense. And if causal relations do exist, we know nothing of them. Kuhn is the most realist of the three: as Hoyningen-Huene says "Kuhn stipulates this world [the world-in-itself] to be *spatiotemporal, not undifferentiated,* and in some sense *causally efficacious*." (Hoyningen-Huene 1993, 34, his italics.) Secondly, van Fraassen's conception of the observable differs from Kuhn's and Hoyningen-Huene's subject-sided moments. The core of the difference lies in a divergence of opinion on whether our theoretical commitments affect the nature of observation. Kuhn takes this to be the case, van Fraassen does not. For van Fraassen, the nature and extent of observation is fixed by our nature as human beings. In this he is closer to Kant than to Kuhn.

5. Conclusion. Kuhn's taxonomic incommensurability is too weak to support an argument from it to anti-realism. But by the same token anti-realism in the form of constructive empiricism is strong enough to entail taxonomic incommensurability. The theories that must exist to make constructive empiricism true (distinct but non-trivially empirically equivalent theories) will typically be taxonomically incommensurable. I linked this to Hacking's claim that we should understand Kuhn as a nominalist, identifying an epistemological version of nominalism as the most appropriate to attribute to Kuhn. Similarly, the constructive empiricist should be an epistemological nominalist: we have no reason to believe that the predicates, kind terms and so on employed by an empirically successful theory pick out the universals or kinds there actually are. From nominalism to taxonomic incommensurability is a short step (some additional details of which are discussed in Hacking 1993).

What should we learn from his connection between van Fraassen and Kuhn? The details of the argument illuminate what exactly is involved in a commitment to constructive empiricism. If two theories are empirically equivalent but differ in non-redundant respects, then we should expect those non-redundant respects to have sufficient theoretical significance for the two theories to be taxonomically incommensurable.

I think that even more revealing is the perspective leant to our view of Kuhn's relationship with the empiricist tradition. Connections between Kuhn and van Fraassen have not previously been made because they are widely thought of as belonging to different parts of the philosophical spectrum. Van Fraassen is an heir to an empiricist tradition that Kuhn is seen as undermining. And in particular it is thought that they have diametrically opposed views on the distinction between observation and theory.

Once the kinds of theory-dependence of observation are disentangled we can see that they agree on the significant points, as far as this discussion is concerned, if not on the detail. Constructive empiricism can be thought of as a modern variant on Kantian scepticism about knowledge of noumena, a version that emphasises the metaphysical similarity between the noumenal and phenomenal, but maintains the important distinction in knowability. This supplies the anti-realism appropriate for underpinning Kuhn's thought, which may be seen as a version of constructive empiricism to which is added the claim that what counts as observable is potentially variable. Kuhn described himself as a Kantian with moveable categories; I have described him as a constructive empiricist with moveable observability.

From the realist point of view, empiricism is an anti-realist philosophy, albeit a science-friendly one. Kuhn's achievement is often seen as that of undermining that kind of anti-realism while at the same time, through the thesis of incommensurability, instituting a new and different kind of anti-realism, one which according to Kuhn's critics and some of his sociologically minded supporters is rather less science-friendly than the empiricist anti-realism it replaced. But the true picture is in fact rather more complicated than that. I have argued elsewhere that Kuhn's philosophy is at least as much a continuation of certain empiricist and positivist assumptions as it is an attack on them (Bird 2002). And the present discussion illustrates that more general point. Kuhn's final, taxonomic form of incommensurability is not enough to support anti-realism. But Kuhn remained an anti-realist; indeed, in my view, he became more anti-realist insofar as he explicitly adopted a Kantian anti-realism about the world-in-itself. So Kuhn's anti-realism has a source independent of incommensurability. That source is an epistemological concern with the truth of theories, expressed in an argument in the Postscript 1969 to the second edition of *The Structure of Scientific Revolutions*. The argument claims that we cannot know theories to be true in a realist sense, because that would require a theory-independent access to reality in order to compare theory and reality. This argument is an old one, and goes back at least to Kant, but is also to be found in the writings of the positivists. It is not the same as the underdetermination argument that is behind constructive empiricism, but it is related to it. More importantly, it shares the same epistemological conclusion, that theoretical knowledge is not possible.

So if Kuhnian incommensurability does not support anti-realism while he does have an independent, epistemological argument for anti-realism, then it becomes sensible to ask: What is the relationship between these views? Is it that the anti-realism supports the incommensurability thesis rather than the other way round? The argument of this paper has been that it certainly could. And it is not difficult to reconstruct a Kuhnian route following my argument. In normal science the range of theories under consideration is constrained; but it is not limited to a unique, fully determined theory; for we may be committed to a form of some key equation but without agreement on the value of an important constant (as in the case of the gravitational constant in the Newtonian paradigm before Cavendish). As Kuhn makes clear, it is one of the tasks of normal science to fix the precise nature of the key equation by finding the value of the constant. And that is something that normal science achieves by careful experimentation and observation. Kuhn even allows that the form of a key equation may be debated within normal science. He seemed to think that there was nothing revolutionary about Euler and Clairaut considering alternatives to Newton's equation. That might seem odd, since such alternatives are clearly revisionary with respect to a key theoretical commitment. But the reason why this is still normal science is that the observational techniques (in this case, backed up by mathematical techniques) available within that normal science tradition permit a choice between the alternatives. The permissible alternatives within normal science are ones that are empirically distinguishable. These alternatives are those that are structurally similar and those that are structurally dissimilar but taxonomically similar. Crisis arises when none of the permissible alternatives can resolve significant anomalies. At that point we need to look to previously impermissible alternatives, ones that are taxonomically different from (and so taxonomically incommensurable with) those that were permitted. That such alternatives do exist is what is guaranteed by epistemological anti-realism (if true). And this is precisely the point at which the primarily empirical practices of existing normal science can no longer be relied upon to provide a decision between competitor theories. In short, if one starts with epistemological anti-realism and a distinction between an empirically-driven normal science and revolutionary science, where the empirical practices of normal science are insufficient, one will conclude, as Kuhn did, that taxonomic incommensurability among revolutionary competitors (one of which will often be the old theory) is to be expected.

REFERENCES

Armstrong, David (1978), *Nominalism and Realism: Universals and Scientific Realism,* vol. 1. Cambridge: Cambridge University Press.

Bird, Alexander (2000), *Thomas Kuhn.* Princeton: Princeton University Press.

——— (2002), "Kuhn's Wrong Turning", *Studies in History and Philosophy of Science* 33: 443–463.

Hacking, Ian (1984), "Five Parables" in Richard Rorty, Jerome Schneewind, and Quentin Skinner (eds.), *Philosophy in History.* Cambridge: Cambridge University Press, 103–124.

——— (1993), "Working in a New World: the Taxonomic Solution" in Horwich 1993, 275–310.

Horwich, Paul (ed.) (1993), *World Changes.* Cambridge, MA: MIT Press.

Hoyningen-Huene, Paul (1993), *Reconstructing Scientific Revolutions.* Chicago: University of Chicago Press.

Kuhn, Thomas S. (1970a), *The Structure of Scientific Revolutions.* Chicago: University of Chicago Press.

——— (1970b), "Reflections on My Critics", in Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge.* Cambridge: Cambridge University Press, 231–278.

——— (1983), "Commensurability, Communicability, Comparability", in Peter Asquith and Thomas Nickles (eds) *PSA 1982,* vol. 2. East Lansing: Philosophy of Science Association, 669–688.

——— (1987) "What are Scientific Revolutions?", in Lorenz Krüger, Lorraine Daston, and Michael Heidelberger (eds.), *The Probabilistic Revolution,* vol. 1. Cambridge: Cambridge University Press, 7–22.

——— (1993), "Afterwords", in Horwich 1993, 311–341.

Newton-Smith, William H. (1981), *The Rationality of Science.* London: Routledge and Kegan Paul.

Okasha, Samir (2002), "Underdetermination, Holism and the Theory/Data Distinction", *Philosophical Quarterly* 52: 303–319.

Sankey, Howard (1997), "Incommensurability: the Current State of Play", *Theoria* 12: 425–445.

——— (1998), "Taxonomic Incommensurability", *International Studies in the Philosophy of Science* 12: 7–16.

van Fraassen, Bas (1980), *The Scientific Image.* Oxford: Clarendon Press.