

Kuhn's Incommensurability Thesis: What's the Argument?

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Abstract: In this paper, I argue that there is neither valid deductive support nor strong inductive support for Kuhn's incommensurability thesis. There is no valid deductive support for Kuhn's incommensurability thesis because, from the fact that the reference of the same kind terms changes or discontinues from one theoretical framework to another, it does not necessarily follow that these two theoretical frameworks are taxonomically incommensurable. There is no strong inductive support for Kuhn's incommensurability thesis, since there are rebutting defeaters against it in the form of episodes from the history of science that do not exhibit discontinuity and replacement, as Kuhn's incommensurability thesis predicts, but rather continuity and supplementation. If this is correct, then there are no compelling epistemic reasons to believe that Kuhn's incommensurability thesis is true or probable.

Keywords: incommensurability; lexical taxonomy; scientific change; scientific revolution; Thomas Kuhn

1. Introduction

Since Thomas Kuhn first introduced the incommensurability thesis in 1962, it has undergone several transformations, even in Kuhn's own writings (Hoyningen-Huene 1993: 206-222).¹ Basically, the thesis has been understood in at least two different ways:

- (TI) *Taxonomic Incommensurability:* Periods of scientific change (in particular, revolutionary change) that exhibit TI are scientific developments in which existing concepts are replaced with new concepts that are *incompatible* with the older concepts. The new concepts are incompatible with the old concepts in the following sense: two competing scientific theories are conceptually incompatible (or incommensurable) just in case they do not share the same "lexical taxonomy." A lexical taxonomy contains the structures and vocabulary that are used to state a

¹ Another version of the incommensurability thesis was proposed by Paul Feyerabend around the same time. As Sankey (2009: 196) notes, however, "It is widely recognized that Kuhn and Feyerabend did not mean the same thing when they originally spoke of the incommensurability of competing theories." For the purposes of this paper, I focus on Kuhn's version of the incommensurability thesis.

theory. (See Kuhn 2000: 14-15, 63, 92-97, 229-233, 238-239, and 242-244. Cf. Sankey 1997.)²

- (MI) *Methodological Incommensurability*: There are no objective criteria of theory evaluation. The familiar criteria of evaluation, such as simplicity and fruitfulness, are not a fixed set of rules. Rather, they vary with the currently dominant paradigm. (See Kuhn 1962: 94; 1970: 200; 1977: 322, and 331. Cf. Sankey and Hoyningen-Huene 2001: xiii.)³

Kuhn's incommensurability thesis has generated an enormous literature.⁴ In this paper, I wish to examine the evidence that is supposed to support Kuhn's incommensurability thesis. This is an important task, I think, for the following reasons. First, some authors use the language of "discovery" when writing about Kuhn's incommensurability thesis. According to Oberheim and Hoyningen-Huene (2013), for instance, Kuhn "*discovered* incommensurability in the mid to late 1940s" (emphasis added). This "discovery" was then followed by a change in career paths. Using the "discovery" language in talking about Kuhn's incommensurability thesis gives the impression that incommensurability is a fact about scientific change (revolutionary change, in particular). But what if one does not think that incommensurability is a fact about scientific change? Are there compelling epistemic reasons to think that the incommensurability thesis is indeed true or probable? In other words, are there good *arguments* for the incommensurability thesis?

Second, and perhaps as a consequence of the first reason, it seems that the literature has focused more on exploring the implications of Kuhn's incommensurability thesis than on evaluating the *arguments* for the thesis itself.⁵ This is not to say that no one has disputed Kuhn's incommensurability thesis (see, e.g., Scheffler 1982). Arguing that a thesis is false, however, is not the same as arguing that there is neither valid deductive nor strong inductive support for a thesis.

In what follows, I focus on (TI) rather than (MI). Here is how I plan to proceed. In Section 2, I argue that there is no valid deductive argument⁶ for (TI), since from the fact that the reference of the same kind terms changes or discontinues from one theoretical framework to another, it does not necessarily follow that these two theoretical frameworks are taxonomically incommensurable. In Section 3, I argue that there is no strong inductive argument⁷ for (TI) because there are rebutting defeaters against (TI). That is, there are episodes from the history of

² On taxonomic incommensurability as a special kind of conceptual incompatibility in terms of lexical taxonomies, see Kuhn (2000: 14-15) and the introduction to Sankey and Hoyningen-Huene (2001).

³ For yet another notion of incommensurability, see Bird (2007: 21-39).

⁴ See, e.g., Sankey (1994) and the essays collected in Sankey and Hoyningen-Huene (2001).

⁵ For recent examples, see the "special issue of *Social Epistemology* [vol. 17, issue 2-3, 2003] devoted to critical comments on Fuller's study of the philosophy of Thomas S. Kuhn and its context" (Gattei 2003: 89).

⁶ By 'valid deductive argument', I mean an argument whose premises, if true, necessitate the truth of the conclusion. An invalid argument, then, is an argument whose premises, even if true, do not necessitate the truth of the conclusion.

⁷ By 'strong inductive argument', I mean an argument whose premises, if true, make the truth of the conclusion more likely, but not guaranteed. A weak argument, then, is an argument whose premises, even if true, do not make the conclusion more probable.

science that do not exhibit discontinuity and replacement, as (TI) predicts, but rather continuity and supplementation. If this is correct, then there are no compelling arguments in support of (TI). In Section 4, I discuss and reply to an objection made by an anonymous referee.

2. Why there is no valid deductive support for (TI)

Is there deductively valid support for (TI)? That is, is there a valid deductive inference from reference change to incompatibility of conceptual content, and thus to (TI)? By appealing to the distinction between sense and reference, Scheffler (1982: 59-60) has already shown that variation of sense does not entail incompatibility of conceptual content.⁸ In that case, perhaps reference change is supposed to entail incompatibility of conceptual content, and hence taxonomic incommensurability. That is, “Distinct taxonomic structures (ones with different subsumption and exclusion relations) are inevitably incommensurable, *because those very differences result in terms with fundamentally disparate meanings*” (Conant and Haugeland 2000: 5; emphasis added). Indeed, in his later works, Kuhn talks about reference change as ‘redistribution’ of members among ‘taxonomic categories’ (Kuhn 2000: 30). Such “*referential changes*,” according to Kuhn (2000: 15), show that “scientific development cannot be quite cumulative,” that “[o]ne cannot get from the old to the new simply by an addition to what was already known,” and that one cannot “*describe the new in the vocabulary of the old or vice versa*” (emphasis added). Furthermore, as Sankey (2009: 197) points out:

Analysis of the reasoning employed by Kuhn [...] when [he] argue[s] for [...] reference change reveals that [he] assume[s] that reference is determined by description.

Accordingly, I take it that a deductive argument for (TI) would run roughly like this:

- (TI1) If competing theories were taxonomically commensurable, then terms would still refer to the same things in new theories (e.g., ‘mass’ in Newtonian Mechanics and ‘mass’ in Relativistic Mechanics would have the same referent).
- (TI2) Terms do not refer to the same things in new theories (e.g., ‘mass’ in Newtonian Mechanics and ‘mass’ in Relativistic Mechanics do not have the same referent, and some terms, such as ‘phlogiston’, are eliminated outright).

Therefore:

- (TI3) Competing theories are taxonomically incommensurable.⁹

⁸ On semantic incommensurability and variation of sense, see Sankey (2009: 197).

⁹ Cf. Stillwaggon Swan and Bruce (2011: 341-343).

In addition to ‘mass’, another example that is supposed to illustrate how reference change is evidence for incompatibility of conceptual content is the following:

Whereas Ptolemaic astronomers used the term “planet” to denote wandering stars, that is, those “stars” that are not fixed stars, Copernicus used the term “planet” to denote a celestial body that orbits the sun (Wray 2011: 25).

This variation in the referents of the term ‘planet’, then, is supposed to show that the Ptolemaic model and the Copernican model are conceptually incompatible.

For this argument to be deductively valid, however, (TI3) must follow necessarily from (TI1) and (TI2). The crucial premise is (TI1). The claim here seems to be that T_1 and T_2 are taxonomically commensurable if and only if T_1 and T_2 share the same lexical taxonomy. Sharing the same lexical taxonomy, as we have seen, means that the kind terms that are used to state T_1 and T_2 refer to the same things. That is to say, for (TI1) to be true, the following conditionals would have to hold:

- (C1) If kind terms t refer to X in T_1 and to X in T_2 , then T_1 and T_2 are taxonomically commensurable.
- (C2) If kind terms t refer to X in T_1 and to Y in T_2 , then T_1 and T_2 are taxonomically incommensurable.

As Hoyningen-Huene (1993: 99) explains, to ensure that they refer to the same things with the same kind terms, scientists need only share vocabularies that incorporate the same taxonomies. If two competing theories do not share the same lexical taxonomies, then those two theories are taxonomically incommensurable (Kuhn 2000: 63).

However, as a general claim about the conceptual incompatibility, and hence incommensurability, of theoretical frameworks,¹⁰ I think that (C2) is false. To see why (C2) is false, consider the following:

- (K1) In biological taxonomy, ‘kid’ refers to a young goat, whereas in folk taxonomy, ‘kid’ refers to a child.

Therefore:

- (K2) Biological and folk taxonomies are taxonomically incommensurable.

¹⁰ (C2) seems false as a general claim about conceptual schemes as well. On paradigms as conceptual schemes, see Henderson (1994: 174). See also Putnam (1981: 114). Cf. Feyerabend (1987: 75-81).

Since (K2) seems false, given that ‘kid’ refers to a young goat in the order Artiodactyla but not in the order Primate, and both orders belong to the class Mammalia, even as far as folk taxonomy is concerned, there must be something wrong with the inference from (K1) to (K2). In other words, if the inference from (K1) to (K2) is supposed to be deductively valid, but (K2) is false, and (K1) is true, then it follows that the inference must be invalid after all, since valid inferences with true premises cannot lead to false conclusions. If this is correct, then from the fact that the same kind terms refer to different things in different theoretical frameworks, it does not necessarily follow that these theoretical frameworks are taxonomically incommensurable.¹¹

To put it another way, if reference change were conclusive evidence for incompatibility of conceptual content, and hence for taxonomic incommensurability, then we would have to conclude that folk taxonomy and biological taxonomy are conceptually incompatible, since in the former ‘kid’ refers to one thing and in the latter ‘kid’ refers to something else. But folk taxonomy and biological taxonomy are not conceptually incompatible. Therefore, reference change is not conclusive evidence for incompatibility of conceptual content. That is, there is no valid deductive inference from reference change to incompatibility of conceptual content, which means that there cannot be a valid deductive argument for (TI) from reference change alone.

For Kuhn, revolutionary change consists in the replacement of a lexical taxonomy by another incompatible taxonomy (Wray 2011: 25). That is to say, revolutionary changes violate Kuhn’s (2000: 92) no-overlap principle, according to which:

no two kind terms, no two terms with the kind label, may overlap in their referents unless they are related as species to genus. There are no two dogs that are also cats, no gold rings that are also silver rings, and so on: that’s what makes dogs, cats, silver, and gold each a kind.

Individuating kind terms, however, is a rather tricky business. Is ‘puppy’ a kind term? If so, ‘puppy’ and ‘dog’ are two kind terms that overlap in their referents even though they are not related as species to genus. Be that as it may, for present purposes, the important point is that there are no conclusive epistemic reasons to think that scientific change, revolutionary or otherwise, involves abandoning a lexical taxonomy in favor of another incompatible one, given that reference change alone is not conclusive evidence for incompatibility of conceptual content, as the ‘kid’ argument shows.

Proponents of (TI) might insist that deductively valid support for (TI) comes from periods of scientific change in which kind terms are dropped entirely (e.g., ‘phlogiston’) rather

¹¹ Cf. Dupré’s (1981) ‘lily’ example. Note that although ‘lily’ refers to one thing in plant taxonomy and to another thing in ordinary language, it also refers to both in these conceptual frameworks, which is why there is no conceptual incompatibility, and hence no taxonomic incommensurability, in this case (Dupré 1981: 74). Similarly, although ‘kid’ refers to a young goat in animal taxonomy and to a child in ordinary language or folk taxonomy, it also refers to both in these conceptual frameworks, which is why there is no conceptual incompatibility, and hence no taxonomic incommensurability, in this case, either.

than change their meaning (e.g., ‘mass’). In other words, it is not reference change (i.e., when the referents of kind terms change) that entails incompatibility of conceptual content but rather reference discontinuity (i.e., when kind terms no longer refer because they were abandoned).¹² In that case, the claim expressed by (TI1) would be that T_1 and T_2 are taxonomically commensurable if and only if the same kind terms from T_1 are retained in T_2 . That is to say, for (TI1) to be true, the following conditionals would have to be true:

- (C3) If kind terms t are used in T_1 and retained in T_2 , then T_1 and T_2 are taxonomically commensurable.
- (C4) If kind terms t are used in T_1 but dropped in T_2 , then T_1 and T_2 are taxonomically incommensurable.

However, as a general claim about the conceptual incompatibility, and hence incommensurability, of theoretical frameworks,¹³ I think that (C4) is false. To see why (C4) is false, consider the following:

- (M1) In desktop environment, WIMP refers to a user interface that includes windows, icons, menus, and pointer, whereas in tablet environment, WIMP has been dropped entirely.

Therefore:

- (M2) Desktop environment and tablet environment are taxonomically incommensurable.

Since (M2) is false, given that the same theoretical principles of computing govern the operation of both desktops and tablets, there must be something wrong with the inference from (M1) to (M2). In other words, if the inference from (M1) to (M2) is supposed to be deductively valid, but (M2) is false, and (M1) is true, then it follows that the inference must be invalid after all, since valid inferences with true premises cannot lead to false conclusions. If this is correct, then from the fact that kind terms have been abandoned by a later theoretical framework, it does not necessarily follow that the earlier and the later theoretical frameworks are taxonomically incommensurable.

In fact, even in the case of ‘phlogiston’, it is not obvious that the two competing theories in question are indeed taxonomically incommensurable. Applied to the case of ‘phlogiston’, the alleged inference from reference discontinuity to taxonomic incommensurability would run roughly as follows:

¹² See Sankey (2009: 197) on semantic incommensurability and discontinuity of reference.

¹³ See footnote 10.

- (P1) In Stahl's theory of combustion, 'phlogiston' refers to the substance that is responsible for combustion, whereas in Lavoisier's theory of combustion, 'phlogiston' is dropped and 'oxygen' refers to the gas required for combustion.

Therefore:

- (P2) Stahl's theory of combustion and Lavoisier's theory of combustion are taxonomically incommensurable.

But (P2) does not necessarily follow from (P1). To see why, suppose that Stahl's theory of combustion is true, and so there is phlogiston, which is the substance that is responsible for combustion. But it could still be the case that there is oxygen as well. In fact, it might actually be a useful addition to Stahl's theory of combustion, since one of its problems was to explain why, when some metals were calcined, the resulting calx was heavier than the original metal. Some tried to explain this by saying that, in some metals, phlogiston has negative weight. Instead, they could have said that phlogiston is lost but oxygen is gained, which would have explained the heavier weight.¹⁴

If this is correct, then, contrary to the claim that Stahl's theory of combustion and Lavoisier's theory of combustion are taxonomically incommensurable, since old concepts, such as phlogiston, were replaced by new concepts, such as oxygen, which are incompatible with the old concepts, the new concepts can actually *supplement*, rather than *replace*, the old ones. If this is correct, then it is not the case that (TI3) follows necessarily from (TI1) and (TI2).

To sum up, then, the considerations put forth in Section 2, if correct, show that, just as variation of sense does not entail incompatibility of conceptual content (Scheffler 1982: 59-60), reference change also does not entail incompatibility of conceptual content. This is so because the fact that the referents of kind terms change from one theoretical framework to another does not entail that the two theoretical frameworks are conceptually incompatible or taxonomically incommensurable. Even discontinuity of reference does not entail incompatibility of conceptual content, since from the fact that a kind term from one theoretical framework no longer refers in a successive theoretical framework, it does not necessarily follow that these theoretical frameworks are conceptually incompatible or taxonomically incommensurable.

Some might think that Kuhn gives a better argument for (TI) in his (2000: 13-32), where Kuhn presents three examples of revolutionary change: Aristotelian and Newtonian conceptions of motion, the contact theory and the chemical theory of the Voltaic cell, and derivations of the law of black-body radiation. On closer inspection, however, I think it becomes clear that Kuhn simply gives another version of the arguments from reference change and reference discontinuity, albeit using three different examples. For, as Kuhn himself puts it, the first two examples are "revolutions [that] were accompanied by changes in the way in which terms like 'motion' and 'cell' *attached to nature*" (emphasis added). As such, these are instances of reference change.

¹⁴ On the phlogiston-oxygen episode, see also Kitcher (1978) who argues that 'dephlogisticated air' genuinely referred to samples of oxygen.

The third example involves what Kuhn calls a “vocabulary change,” e.g., from ‘resonator’ to ‘oscillator’. As such, this is an instance of reference discontinuity.

Moreover, in the spirit of Friedman’s (2011: 433) call “to *relativize* the Kantian conception of *a priori* scientific principles to a particular theory in *a given historical context*” (emphasis added) and “to *historicize* the notion of scientific objectivity” (original emphasis), one might question in what sense Aristotelian physics and Newtonian physics were “competing theories.” After all, Newton did not seek to replace Aristotelian physics. According to Westfall (1983: 14):

As far as men active in the study of nature were concerned, the word “overthrown” is not too strong. For them, Aristotelian philosophy was dead beyond resurrection.

Newton’s target was not Aristotelian physics, but rather Cartesian physics, as the title of his *Principia* suggests (Brown 2005: 153-154). Likewise, insofar as Copernicus’ heliocentric model had a competitor, it was not Ptolemy’s geocentric model, but rather Tycho Brahe’s hybrid model (Brown 2005: 156). When we take these historical facts into account, the transition from Aristotelian physics to Cartesian physics and then to Newtonian physics, and the transition from the geocentric model to Brahe’s hybrid model and then to the heliocentric model, do not seem all that “revolutionary” (in the Kuhnian sense).

Accordingly, the argument of Section 2 can be summed up as follows:

- (1) Reference change is conclusive evidence for (TI) only if reference change entails incompatibility of conceptual content.
 - (2) Reference change does not entail incompatibility of conceptual content.
- Therefore:
- (3) It is not the case that reference change is conclusive evidence for (TI).

A similar argument applies to discontinuity of reference as well. That is:

- (1) Reference discontinuity is conclusive evidence for (TI) only if reference entails incompatibility of conceptual content.
 - (2) Reference discontinuity does not entail incompatibility of conceptual content.
- Therefore:
- (3) It is not the case that reference discontinuity is conclusive evidence for (TI).

These arguments, if sound, show that there is no deductively valid inference from either reference change or reference discontinuity to (TI).¹⁵ Accordingly, if a compelling argument can

¹⁵ Cf. Sankey (2009). According to Sankey (2009: 198), “the threat of wholesale referential discontinuity between theories has been removed by rejecting the description theory of reference.” That is, if “reference is independent of

be made for (TI), that argument cannot be a deductively valid one. That leaves the option of a strong inductive argument. In the next section, then, I wish to see whether or not a strong inductive argument can be made in support of (TI).

3. Why there is no strong inductive support for (TI)

If the considerations put forth in Section 2 are correct, then there can be no valid deductive argument for (TI) from reference change and/or discontinuity alone. The other option, then, is a non-deductive or inductively strong argument for (TI). Accordingly, assuming for the sake of argument that there are episodes of scientific change in which competing theories exhibit taxonomic incommensurability, as Kuhn argues, the question is whether one can reasonably draw a general conclusion about the nature of scientific change (more precisely, revolutionary change) from such episodes. So, I take it that an inductive argument for (TI) would run roughly like this:

- (TI4) Some episodes from the history of science exhibit taxonomic incommensurability (e.g., the Newtonian-Relativistic Mechanics episode, the phlogiston-oxygen episode).

Therefore:

- (TI3) Scientific change (specifically, revolutionary change) is characterized by taxonomic incommensurability. (In other words, competing theories are taxonomically incommensurable.)

Now, even if we grant that Newtonian Mechanics and Relativistic Mechanics are taxonomically incommensurable, I think that it is a mistake to generalize from a few selected examples that competing theories in general are taxonomically incommensurable. In the interest of brevity, I will briefly discuss one episode of scientific change. This episode is sufficient to show that one can reach different conclusions about the alleged taxonomic incommensurability of competing theories depending on the examples one (cherry-) picks. It is important to note that the following episode is not supposed to be a counterexample against (TI). It is not meant to be a refutation of (TI). Rather, it shows that an inductive argument based on a few selected historical episodes of scientific change does not provide strong inductive support for (TI). Or, to put it another way, this episode—and others like it—counts as what Pollock (1987: 481-518) calls a *rebutting defeater*, i.e., a *prima facie* reason to believe the negation of the original conclusion; in this case, the negation of (TI).

description,” then “successive theories are not incommensurable due to discontinuity of reference” (Sankey 2009: 198).

3a. Anastomoses

Early Modern physiologists faced the following problems, which were an inheritance from Galen: (a) How does blood flow from the right ventricle to the left? (b) Does the venous artery contain blood or air? According to Galen, the veins and arteries are two distinct systems. The only point of communication is the permeable septum in the heart. The problem was that Galen's conviction that the septum is permeable is based more on speculation than empirical evidence. Apparently, Vesalius was aware of this problem. In his *De Humani corporis fabrica* (1543), he writes:

The septum of the ventricles, therefore, is [...] made out of the thickest substances of the heart and on both sides is plentifully supplied with small pits which occasion its presenting an uneven surface towards the ventricles. Of these pits not one (at least in so far as is perceptible to the senses) penetrates from the right ventricle to the left, so that we are greatly forced to wonder at the skill of the Artificer of all things by which the blood sweats through passages that are invisible to sight from the right ventricle to the left (O'Malley 1965).

And in the 1555 edition, Vesalius writes:

Nevertheless, howsoever conspicuous these pits may be, not one of them, in so far as is perceptible to the senses, penetrates through the ventricular septum from the right to the left ventricle. Indeed, I have never come upon even the most obscure passages by which the septum of the heart is traversed, albeit that these passages are recounted in detail by professors of anatomy seeing that they are utterly convinced that the blood is received into the left ventricle from the right. And so it is (how and why I will advise you more plainly elsewhere), that I am not a little in two minds about the office of the heart in this respect (O'Malley 1965).

For Galen, much of the blood in the left ventricle comes from the right ventricle through these pores or pits in the septum. As Vesalius points out, however, no such pores or pits could be detected in the septum.

Following Vesalius, other anatomists became increasingly dissatisfied with the Galenist account of such pits. To name one of them, Realdus Columbus describes the pulmonary route of the blood from the right ventricle to the left in *De re anatomica libri xv* (1559):

Between these ventricles there is placed the septum through which almost all authors think there is a way open from the right to the left ventricle; and according to them the blood is in transit rendered thin by the generation of the vital spirits in order that the

passage may take place more easily. But these authors make a great mistake; for the blood is carried by the artery-like vein to the lungs and being there made thin is brought back thence together with air by the vein-like artery to the left ventricle of the heart (Hall 1962: 273).

Although Juan Valverde da Hamusco was the first to publish experimental work on the minor circulation of the blood, the work of Columbus was widely read (Hall 1962: 274).

By the time William Harvey began his work on the circulation of the blood, the problems, if not the rudiments of a solution, were already set for him by his predecessors. After all, in addition to Valverde and Columbus' work on the minor circulation, Fabricius' *De venarum ostioliis* (1603) was known to Harvey. As a student of Fabricius at the University of Padua, Harvey was likely influenced by Fabricius' work, even though Fabricius did not publish his work until 1603 (Debus 1954: 63-65). For Fabricius, these *ostiola* are little doors that obstruct the flow of blood, so that the veins are not ruptured.

Although they would have made a very useful addition to his theory, Harvey was careful in his speculations and modest in his claims about the unobserved 'capillaries'. As Elkana and Goodfield (1968: 62) show, "Harvey was aware that the circulation was completed *by some means or other*; but throughout his life he was in considerable doubt as to how this was actually accomplished" (original emphasis). Harvey's reluctance to posit the existence of unobserved capillaries is evident in *Anatomical Disquisitions on the Circulation of the Blood* (1648), in which he replies to objections made by the French physician, Jean Riolan the Younger. As Harvey writes: "neither our learned author himself [i.e., Riolan], nor Galen, nor any experience, has ever succeeded in making such anastomoses as he imagines, sensible to the eye" (Bowie 1889: 107).

Harvey continues:

I can therefore boldly affirm, that there is neither any anastomosis of the vena portae with the cava, of the arteries with the veins, or of the capillary ramifications of the biliary ducts, which can be traced through the entire liver, with the veins (Bowie 1889: 108).

So, according to his own accounts of the discovery of the circulation of the blood, it seems that Harvey was looking for such anastomoses as Galen's theory required. In doing so, he was clearly working within the framework of the problems that were set for him by his predecessors. After Harvey's death, Marcello Malpighi, studying the lungs of frogs using a microscope, showed that veins and arteries are joined by anastomoses (Elkana & Goodfield 1968; Allchin 2005).

Contrary to (TI), then, the Anastomoses episode exhibits *continuity* in terms of the problems practitioners worked on, and *supplementation* in terms of old concepts that were abandoned for a while (e.g., anastomoses) but then rediscovered and added to the new theory, rather than discontinuity and replacement, as (TI) predicts.

The idea that some episodes of scientific change exhibit continuity is not new. For example, according to Friedman, there is continuity across scientific change at the level of what he calls “relativized *a priori*” principles. As Friedman (2001: 31) writes:

Relativity theory involves *a priori* constitutive principles as necessary presuppositions of its properly empirical claims, just as much as did Newtonian physics, but these principles have essentially changed in the transition from the latter theory to the former: whereas Euclidean geometry is indeed constitutive *a priori* in the context of Newtonian physics, for example, only infinitesimally Euclidean geometry—consistent with all possible values of the curvature—is constitutively *a priori* in the context of general relativity. What we end up with, in this transition, is thus a relativized and dynamical conception of *a priori* mathematical-physical principles, which change and develop along with the development of the mathematical and physical sciences themselves.

For Friedman (2001: 63), then, even though the concepts that constitute the relativized *a priori* principles change from one theoretical framework to another, and thus give rise to worries about incommensurability, there is still continuity, given that “earlier constitutive frameworks are exhibited as limiting cases, holding approximately in certain precisely defined special conditions, of later ones,” and that the “principles of later paradigms [...] evolve continuously, by a series of natural transformations, from those of earlier ones.” Indeed, some have explicitly drawn parallels between Friedman’s relativized *a priori* and structural realism. (See, e.g., MacArthur 2008. Cf. Ivanova 2011.)

If the Anastomoses episode is representative of scientific change, however, then it suggests that there can also be *conceptual continuity* (i.e., continuity in terms of concepts, such as Anastomoses), not merely *structural continuity* (i.e., continuity in terms of structures or principles, such as Euclidean geometry and the equivalence principle)¹⁶, across theory change. Adequately supporting this claim, however, requires a detailed examination of a greater variety of episodes of scientific change, which is beyond the scope of this paper. Indeed, one might complain that both Kuhn’s and Friedman’s accounts of scientific change are not representative of scientific change in general, since the case histories that are supposed to support these accounts are drawn from physics alone, to the exclusion of the social sciences and the life sciences. Be that as it may, my present aim is simply to show that there is no strong inductive support for (TI).

3b. Generalizing from the history of science

In light of the Anastomoses episode, which shows continuity and supplementation between competing theories, rather than replacement and incompatibility, as (TI) predicts, it seems fair to say that an inductive argument for (TI), construed as an inductive generalization, is an instance of hasty generalization. Indeed, as I have suggested above, the selected episodes which are supposed to support (TI), such as the phlogiston-oxygen episode, may even be unrepresentative

¹⁶ These are Friedman’s (2001: 71) examples.

of the nature of scientific change, since they are drawn exclusively from physics, thereby neglecting the life and social sciences. Again, the Anastomoses episode is not supposed to be a counterexample against (TI).¹⁷ It is not meant to refute (TI). Rather, it shows that a few episodes from the history of science, even if they do exhibit taxonomic incommensurability, do not provide strong inductive support for (TI).¹⁸

Accordingly, the argument of Section 3 can be summed up as follows:

- (1) There is a strong inductive argument for (TI) only if there are no rebutting defeaters against (TI).
 - (2) There are rebutting defeaters against (TI).
- Therefore:
- (3) It is not the case that there is a strong inductive argument for (TI).¹⁹

Accordingly, since there can be no deductively valid argument for (TI), as I have argued in Section 2, and there can be no inductively strong argument for (TI), as I have argued in Section 3, it follows that there can be no good arguments for (TI). In other words, since deductive and inductive support for (TI) exhaust all forms of epistemic support for a thesis, given the way I have characterized them, it follows that there are no compelling epistemic reasons to believe that (TI) is true or probable.

To this friends of (TI) might object that (TI) is not supposed to be a general thesis about the nature of scientific change (more specifically, revolutionary change).²⁰ Instead, friends of (TI) might argue, some episodes of scientific change exhibit taxonomic incommensurability, whereas others do not.²¹ This move, however, raises the following problems. First, if (TI) is not a general thesis about the nature of scientific change, then what is its *explanatory* value? How does (TI) help us in terms of understanding the nature of scientific change? On most accounts of explanation, an *explanans* must have some degree of generality, either because it counts as a ‘law of nature’ as in the Deductive-Nomological model (Hempel 1965: 248), or because it is statistically relevant as in the Statistical Relevance model (Salmon 1971), or because it provides a unified account of a range of different phenomena as in the unificationist model (Kitcher 1989).

¹⁷ Another episode from the history of science that runs contrary to (TI) is the scientific change from Wegener’s theory of continental drift to plate tectonics.

¹⁸ Just as it would be hasty to conclude from a few episodes that incommensurability is typical of scientific change, it would also be hasty to conclude from a few episodes that incommensurability is atypical of scientific change.

¹⁹ Given what I mean by deductive and inductive arguments, any non-deductive form of inference is covered by ‘inductive’ for the purposes of this paper, except abduction or Inference to the Best Explanation (IBE). I think it is safe to assume that incommensurability is not supposed to be the best explanation for scientific change, since (TI) is supposed to be a *mark of* scientific change, not an *explanation for* scientific change. Moreover, it is not clear what, if any, novel predictions (TI) makes. Cf. Mizrahi (2012).

²⁰ By “general thesis,” I mean a thesis that holds true of *most*—though not *all*—cases. So, if (TI) is not a general thesis, then it does not hold true even of *most* episodes of scientific change. This would be a problem, of course, if the argument for (TI) is supposed to be an inductive generalization.

²¹ See Kuhn (1962: 103). Cf. Bird (2012).

But if (TI) has no degree of generality, then it is difficult to see what the explanatory value of (TI) is.

Second, if (TI) is not a general thesis about the nature of scientific change, then what is its *predictive* value? If (TI) amounts to the claim that some competing theories are taxonomically incommensurable, then it is difficult to see how (TI) can help us make predictive inferences, such as “(TI); T_1 and T_2 are competing theories; therefore, T_1 and T_2 are taxonomically incommensurable,” which are the inferences we need to be able to make in order to understand scientific change. In other words, if (TI) amounts to the claim that some competing theories are taxonomically incommensurable, whereas others aren’t, then (TI) is nothing more than an interesting observation about some episodes of scientific change, not a theory about scientific change. In that respect, it is no more useful, as an attempt to understand scientific change, than the observation that some episodes of scientific change exhibit fraud. From the fact that some alleged “scientific discoveries” turned out to be fraudulent, such as the Piltdown man incident (Russell 2003), we cannot learn anything general and/or useful about the nature of science. I doubt that friends of (TI) would want to relegate (TI) to the level of an interesting observation about science that has no explanatory and/or predictive value.

To be clear, my response to this objection goes beyond the exegetical question of whether Kuhn meant (TI) to be a general thesis about scientific change (more precisely, “revolutionary change”) or not.²² My argument is that, as far as theory change is concerned, if (TI) is the exception rather than the rule, i.e., if it is not the case that (TI) holds true of *most* (though not *all*) episodes of theory change, particularly “revolutionary change,” then (TI) has no explanatory and/or predictive value. If (TI) has no explanatory and/or predictive value, then there are no “pragmatic” grounds for accepting (TI).²³

Now, if there are no compelling epistemic reasons to believe that (TI) is true or probable, given that there is neither valid deductive nor strong inductive support for (TI), as I have argued, and given that there are no “pragmatic” reasons to accept (TI), such as considerations of explanatory and predictive power, then it is difficult to see why anyone should believe that (TI) is true or accept (TI). Stripped of explanatory and predictive value, “it looks as if incommensurability is irrelevant to questions of theory choice” (Brown 2005: 158).

4. An objection and a reply

An anonymous referee objects that conceptual incompatibility is supposed to be evidence for variation of reference, not the other way around. If the referee is right, then the fact that kind terms from one theoretical framework are either replaced with incompatible kind terms or dropped altogether in a competing theoretical framework is supposed to be conclusive evidence that these kind terms refer to different things.

²² Though see Sankey (1993). Since (TI) is an integral part of Kuhn’s theory of scientific revolutions, and that theory is supposed to be a general account of scientific change, it follows that (TI) is supposed to hold generally, too.

²³ I use ‘pragmatic’ and ‘acceptance’ in the constructive empiricist’s sense. On ‘acceptance’, see van Fraassen (1980: 88). On the distinction between epistemic and non-epistemic (or pragmatic) values in theory choice, see van Fraassen (2007: 340).

I think that there are several problems with the referee's objection. First, it seems to be at odds with the characterization of (TI) in Section 1 and the textual evidence cited in Sections 1 and 2. This is not a serious problem, however, given that Kuhn's incommensurability thesis is notoriously difficult to pin down. More importantly, even if the referee is right, the question "What is the argument for (TI)?" remains unanswered. The referee argues that conceptual incompatibility is conclusive evidence for variation of reference. If so, then conceptual incompatibility is taken for granted as a premise from which variation of reference is supposed to follow. But the question raised in this paper is what the argument for *conceptual incompatibility* or *taxonomic incommensurability* is. To say that conceptual incompatibility is just taken for granted as evidence for something else is to miss the question that this paper raises.

In that respect, note that conceptual incompatibility cannot simply be gleaned from case histories of theory change. After all, (TI) is a theoretical claim, partly because concepts are unobservable. Hence, conceptual incompatibility has to be inferred from something else. What is that something else from which conceptual incompatibility is supposed to follow? The referee does not say if by "variation of reference," s/he means *reference change* (i.e., when the same kind terms refer to different things in competing theoretical frameworks) or *reference discontinuity* (i.e., when kind terms are dropped from one theoretical framework to another).

On the one hand, if by "variation of reference" the referee means *reference discontinuity*, then that means that kind terms from one theoretical framework have been dropped by a competing theoretical framework. In that case, however, conceptual incompatibility cannot simply be gleaned from the two competing theories, since the kind terms that are supposed to be conceptually incompatible have been dropped. That is to say, how can we tell if kind term K in T_1 is conceptually incompatible with K in T_2 if there is no K in T_2 ?

On the other hand, if by "variation of reference" the referee means *reference change*, then that means that the same kind terms have different referents. But in that case, too, conceptual incompatibility cannot simply be gleaned from the two competing theories, since the two competing theories *appear* to be compatible given that they share the same kind terms. That is to say, how can we tell whether T_1 is conceptually incompatible with T_2 or not if they share the same kind term K ? If one were tempted to reply as follows: "Yes, they share the same kind terms, but these kind terms refer to different things; hence, the two competing theories are conceptually incompatible," then my answer would be "Precisely." That is, reference change is supposed to be evidence for conceptual incompatibility.

It is also worth noting that even *reference change* cannot simply be gleaned from case histories of theory change. This is so because of the distinction between what words mean and what a speaker means in uttering these words (Grice 1969) or the distinction between *semantic reference* and *speaker's reference* (Kripke 1977). To illustrate, consider the case of 'planet' discussed above (Wray 2011: 25). From looking at the way Ptolemaic astronomers used 'planet' and comparing it to the way Copernican astronomers used 'planet', we might think that 'planet' refers to different things, and hence that the Ptolemaic model and the Copernican model are taxonomically incommensurable. But that does not *necessarily* follow, since, even if the speaker's reference of 'planet' is different in these two competing models, the semantic reference may be the same. Arguably, that was the case with 'planet'. The reverse may also be the case. That is, even if the semantic reference of kind term K is different in two competing theories, the speaker's reference may be the same. If this is correct, then the semantic/speaker's reference

distinction undercuts the alleged entailment from reference change to conceptual incompatibility just as the sense/reference distinction undercuts the alleged entailment from semantic change to conceptual incompatibility (Scheffler 1982: 59-60; Sankey 2009: 197).

5. Conclusion

In this paper, I have argued that there is no epistemic support, in the form of either a deductively valid or an inductively strong argument, for Kuhn's thesis of taxonomic incommensurability (TI). Reference change and/or discontinuity do not provide conclusive evidence for (TI), since there is no valid deductive inference from reference change and/or discontinuity to incompatibility of conceptual content. Moreover, there can be no strong inductive argument for (TI), since there are episodes from the history of science that do not exhibit discontinuity and replacement, as (TI) predicts, but rather continuity and supplementation. These historical episodes do not *refute* (TI). Rather, they are rebutting defeaters against (TI).

If, in an attempt to save (TI), friends of (TI) retreat to the position that (TI) is not supposed to be a general thesis about the nature of scientific change (specifically, revolutionary change), then they have to face the consequence that there are no "pragmatic" grounds for accepting (TI), either, since (TI) would be stripped of any explanatory and predictive value. If this is correct, then there are no compelling epistemic reasons for (TI), which means that no one should believe that (TI) is true or probable, and there are no "pragmatic" reasons for (TI), which means that no one should accept (TI).

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