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FOCAL POINT | W CENTRUM UWAGI

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Incommensurability Explained in the Terms of Presuppositions. A Comment to Kuhn's Thesis on Radical Meaning Variance

Abstract

Kuhn's radical meaning variance thesis implies that scientists, who work in different paradigms cannot understand each other. This, however, seems incredible. The air of paradox can be dispersed once the role of presuppositions in constituting a paradigm is acknowledged. Presuppositions function in the way of the Wittgensteinian ungrounded hinges and often are only implicitly assumed. In the face of recalcitrant puzzles some presuppositions can be made explicit and revised. The mechanism of possible revisions of presuppositions can be accounted for in terms of Hintikka's interrogative model of scientific inquiry with some amendments.

The model includes three possible reactions to an anomaly: (i) a conservative offer of an auxiliary hypothesis within the current paradigm, (ii) a reinterpretation of puzzling experimental results and non-revolutionary enrichment of the current paradigm with a novel hypothesis, and (iii) a revision of presuppositions that amounts to a full-fledged scientific revolution. The choice depends on the success or failure of more conservative alternatives and the scope of application of the theory under investigation. In the proposed approach, incommensurability does not hinder communication between the proponents of different paradigms. In addition, some other controversial points in Kuhn's views are explained, like Kuhn's losses, reproaching conservative attitudes towards anomalies, or the admissibility or inadmissibility of the coexistence of rival paradigms. Last but not least, a link between a paradigm shift and the strive for truth is established.

Keywords: *incommensurability*, *(non)-factivity of knowledge*, *paradigm*, *presupposition*, *scientific change*, *radical meaning variance thesis*

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Adam Grobler

Niewspółmierność wyjaśniona w kategoriach presupozycji. Komentarz do tezy Kuhna o radykalnej zmienności znaczenia terminów naukowych

Abstract

Głoszona przez Kuhna teza o radykalnej zmienności znaczenia terminów naukowych sugeruje, że zwolennicy różnych paradygmatów nie rozumieją się wzajemnie. Ten wniosek wydaje się jednak niewiarygodny. Powstały paradoks znika, jeżeli się doceni rolę presupozycji w konstytuowaniu się paradygmatu. Presupozycje funkcjonują na modłę Wittgensteinowskich zawiasów i często pozostają jedynie domyślne. W obliczu opornych łamigłówek niektóre presupozycje wychodzą na jaw i ulegają rewizji. Mechanizm rewizji presupozycji można zilustrować za pomocą pojęć ulepszonej wersji zaproponowanego przez Hintikkę modelu nauki jako gry w pytania i odpowiedzi.

Model, o którym mowa, przewiduje trzy możliwe reakcje na pojawienie się anomalii: (i) konserwatywne propozycje hipotez pomocniczych w ramach istniejącego paradygmatu, (ii) reinterpretację zagadkowych rezultatów eksperymentalnych i nierewolucyjne wzbogacenie istniejącego paradygmatu o nową hipotezę oraz (iii) rewizję niektórych presupozycji, która jest równoznaczna z rewolucją naukową w pełnym tego słowa znaczeniu. Wybór między członami tej alternatywy zależy od sukcesu lub niepowodzenia bardziej konserwatywnych strategii oraz zakresu zamierzonych zastosowań teorii będącej w centrum zainteresowania. W proponowanym ujęciu niewspółmierność nie zakłóca komunikacji między zwolennikami różnych paradygmatów. Ponadto, pozwala ono objaśnić i oddalić kilka innych kontrowersyjnych składników stanowiska Kuhna ze *Struktury rewolucji naukowych*, jak tezę o stratach eksplanacyjnych przy zmianie paradygmatu, potępienie konserwatywnych postaw wobec anomalii czy jego poglądu na temat możliwości współistnienia konkurencyjnych paradygmatów. Co ważniejsze, zostanie pokazany bagatelizowany przez Kuhna związek między zmianą paradygmatu a dążeniem do prawdy.

Słowa kluczowe: *niewspółmierność*, *(nie)faktywność wiedzy*, *paradygmat*, *presupozycje*, *przemiany w nauce*, *radykalna zmienność znaczeniowa terminów naukowych*

1. Posing the problem

The incommensurability problem that stemmed from Kuhn's *The Structure of Scientific Revolutions* is perhaps the most fascinating one in the philosophy of science of the last decades of the 20th century. For the present purposes, the problem can be briefly defined as follows. On the one hand Kuhn argues that scientific theories can hardly be logically compared in terms of their relative merits across scientific revolutions. This is what is called Incommensurability Thesis. Its most interesting and persuasive component is Radical Meaning Variance Thesis (RMV henceforth) possibly that says that scientific terms are differently used, and thereby differ in meaning, before and after a revolution occurs. Consequently, all theory comparisons across a scientific revolution that employ the idea of logical relations between the laws of the theories under comparison by commit the fallacy of equivocation.

On the other hand, however, the prevalent view is that a theory is replaced by its successor just because the latter has proved its merits as compared with its antecedent. In particular, it is held that one theory is better than the other when the former explains all the phenomena explained by the other and, in addition, explains some phenomena hitherto unexplained. Therefore, many philosophers have

believed that there are strong logical connections between subsequent theories. The Correspondence Thesis popular with Polish philosophers (e.g. Krajewski 1977) in the midst of the century says that the laws of the older theory are approximations of some laws of the newer one, or they are even their logical consequences, possibly under some additional assumptions. An interesting variation of this idea, combined with the hypothetico-deductive method, has been put forth by Michał Kokowski (1996) together with a criticism of Kuhn's views.

RMV is best illustrated with the example of the term 'mass'. In Classical Mechanics (CM henceforth) the term is used to denote a property of a body while in Special Relativity (SR henceforth) it denotes a two-place relation between a body and a frame of reference. Consequently, all the homonymous terms of the theories in question, that depend on the term 'mass', differ in meaning. Thus, the putative logical connections between the laws of the two theories suffer from the fallacy of equivocation. This conclusion must seem incredible for the reader of Albert Einstein's (1922). His narrative strongly suggests the opposite: the relativity principle is all one needs to derive most of the laws of SR from those of CM.

Thus, the alleged change in meaning of some scientific terms cannot be as radical as Kuhn might had it. Some of his claims – for instance, that scientific revolutions make scientists to move to another planet (*Postscript*), or that followers of different paradigms cannot understand each other – sound as fancy exaggerations. Others – e.g. that a scientific change involves a Gestalt switch – sound quite plausible. In fact, a switch from perceiving a duck to perceiving a rabbit in a famous Wittgenstein's figure does not preclude one to see on demand a duck again and even understand, which details of the drawing enable one to shift back and forth between the two images. By the same token, a scientist or student can maneuver between the classical and the relativist image of the world depending on the applications they may have in mind.

Still, RMV is challenging. Kuhn is surely right in claiming that scientific terms differ in *use* across scientific revolution. But he is possibly too hasty in passing to RMV. While Kuhn is correctly said to be a Wittgenstein's heir, unfortunately the popular maxim coined on this count, *meaning is use*, is not faithful to its alleged source. Consider the following quotations: "But what is the meaning of the word 'five'? No such thing was in question here, only how the word 'five' is used" (Wittgenstein 2009, 1). Or: "one can perhaps get an idea of how much the general concept of the meaning of a word surrounds the working of language with a haze which makes clear vision impossible" (*ibid.* 5). These expresses a sort of skepticism about the concept of the meaning rather than equating it with that of use. Wittgenstein is even more explicit about this in "For a *large* class of cases of the employment of the word 'meaning' – though not for *all* – this word can be explained in this way: the meaning of a word is its use in the language" (*ibid.* 43, original emphasis). Thus, at least in some cases meaning is not use. These may include the case of scientific terms, as the example of Einstein's strategy suggests.

The question has been much discussed in the literature and I am not in the position to review many attempts to explain away RMV. I confine myself to mentioning two characteristic attempts, that contest Incommensurability, to come to quite different conclusions. Both of them focus on the concept of reference rather than meaning, possibly to evade Wittgenstein's haze that surrounds the latter, and has not been cleared in the prolonged debate about conceptual change. The modesty of these self-restricted proposals is not unreasonable, given the fact, that reference is an important component of meaning, especially the meaning of terms.

One of the attempts was offered by Hartry Field (1973), who has argued that 'mass' in CM has indeterminate reference and refers either to rest mass or relativist mass, what has been discovered with the dawn of SR only. Consequently, there is no reason to consider the theories in question incommensurable. In contrary, Hilary Putnam (1973), along the lines of the then unpublished Saul Kripke's work (1980), advocated a causal theory of reference, on which the reference of a term is stable across theory change. This is warranted by the shared intention among the linguistic community members to use a term to refer to the same entity, regardless of whether they correctly identify its

referent. Unfortunately, neither here nor in further elaboration of his account in (1975), Putnam said anything about 'mass'. My guess is that he might, in accord with some physicists, have considered rest or invariant mass to be the proper reference of the term. Still, he has not been explicit on this question.

Given the ambiguous results of studies on conceptual change, whether in terms of meaning analyses or theories of reference, some, including me in (1986), considered the structuralist or non-statement view of scientific theories, as originated by Joseph Sneed (1971) and continued by Wolfgang Stegmüller (1976) and others, to be the most promising approach to the solution of the incommensurability problem. They looked at scientific theories as families of abstract structures, models rather than systems of sentences. Such an approach permitted to avoid the queries about meaning and, instead, put much weight on structural relations between elements of the theories under comparison. Unfortunately, in order to put the structural machinery in motion, one has to assume the identity of the entities that occur in the models under comparison. Consequently, the question of reference of scientific terms reappears.

2. The concept of presupposition and the dynamics of theories

Later, under the influence of Ryszard Wójcicki (1991), I have found a more promising line towards solving the incommensurability problem. Thus, in an unpublished draft, he suggested that proponents of outdated theories, even if they were wrong, still must have known something, because their error consisted in using wrong words to express their knowledge rather than in putting forth false claims. For example, Aristotle knew something when he claimed that heavy bodies fell down, while the very concept of the up-down direction was misplaced. Wójcicki described this kind of error as assuming wrong presuppositions. His remarks drew my attention to Strawson's concept of presupposition especially in view of the persistence of reference concerns indicated above. After all, in the famous example of the supposedly bald present king of France, Strawson's idea was to place the responsibility for the usage of the non-referring singular terms on the falsity of existential presuppositions[1].

In the context of the incommensurability problem, it is plausible to consider, apart from presuppositions about the existence of persons and objects, the presuppositions about the extension or arity of theoretical predicates. For example, in CM 'mass' is presupposed to be a unary predicate, while in SR '(relativist) mass' is presupposed to be a binary predicate. On this account, what CM presupposes about mass is, in the lights of SR, discovered to be false. Consequently, most of the laws of CM are, according to the logic of presuppositions, neither true nor false rather than just false. Now, adopting Wójcicki's suggestion, those laws still qualify as knowledge, albeit outdated knowledge. Epistemologists committed to the factivity of knowledge would oppose this contention, but, contrary to them, I find it highly plausible that outdated knowledge is to be clearly distinguished with regard to its epistemic status from superstition, myth or delusion.

Thus, following Wójcicki, I deliver an independent support for Allan Hazlett's (2010) rejection of the factivity of knowledge. In fact, as early as in Grobler (2001) I offered an analysis of knowledge that weakens the truth-condition of the tripartite definition of knowledge to that of non-falsity. On this account, a justified belief that presupposes a false presupposition is regarded as a piece of knowledge.

[1] Strawson opposed Russell's commitment to Bivalence. In Russell's (1905) theory of descriptions 'The present king of France is bald' is to be read as 'There is somebody that is the present king of France and is bald' and is false. In Strawson's account the existence of the present king France is presupposed rather than stated in the original formulation and its truth-value, or lack of truth-value, depends on the circumstance of the utterance of the sentence in question. In van Fraassen's (1968) elaboration it is the sentences themselves rather than their utterances that are truth-bearers. In particular, they are truth-value-gaps in case they presuppose untrue presuppositions. Actually, I rely on van Fraassen's version that is more suitable for the language of science, while Strawson's one applies to everyday speech. The idea that the propositions (rather than utterances) that contain non-referring terms have no truth-value comes from Gottlob Frege (1892). More recently, the concept of pragmatic presupposition is commonly entertained. This will be explained later.

At the time, I had not taken the question of what it is for a belief to be justified. In what follows I will suggest that justification, like knowledge, is also relative to accepted presuppositions. Be that as it may, I find the requirement of factivity much troublesome for the fallibilist. Think of the pioneering version of this position, Charles S. Peirce's (1902, CP 5.565–566) conception of truth as the ideal limit of inquiry. Assuming the factivity requirement one has to consider all that is less than the ideal limit of inquiry to be something less than knowledge. Consequently, granting that the ideal limit may be out of reach, one has to doubt whether we can have any knowledge at all.

A fallibilist is possibly better off adopting Karl R. Popper's (1972) idea that our knowledge may always be erroneous and is subject to possible corrections attainable through the falsificationist method. Hence, knowledge can be better or worse and, which is at odds with the Incommensurability, one can tell one from the other. Note that the notion of presupposition allows for some refinement of the falsificationist view or, more generally, an account of how knowledge is self-correcting enterprise. Apart from a straightforward falsification of a hypothesis one can falsify its presupposition(s). The latter strategy is much more interesting when it comes to the queries posed by Kuhn's work.

In the first place, the falsification of a presupposition of a law gives a strong hint about correcting the law in question. This explains why the shift from CM to SR, as described by Einstein (1922), is so smooth, without any sign of incommensurability. Suspending the presupposition about unarity of 'mass' permits a correct (or at least less erroneous) identification of the extension of the term. Knowing why this presupposition is false one can also find out how to systematically revise all the equations of CM and transform them into those of SR. Thus, the impression of incommensurability may arise only when one looks at the two theories in their final shape. Once the heuristics that guides the invention of a new theory is taken into account, the air of incommensurability is dispersed.

Second, the notion of presupposition permits a subtler, in comparison to Kuhn's, taxonomy of scientific change. Apart from normal science and scientific revolution one can distinguish an intermediate phase. The idea derives from an analysis of the growth of science in terms of Jaakko Hintikka's (1984) interrogative model of science. In his view, scientific inquiry can be modelled as a game of questions and answers between Scientist and Nature. Scientist, in performing experiments, asks yes-or-no questions and Nature gives answers through the experimental results. As the game is continued, a class of possible worlds that is supposed to include the actual world, call it the class of candidate-worlds, is narrowed down successively. This picture corresponds to the idea of normal science, where narrowing down the class of candidate-worlds represents cumulative change.

Now, in my work (Grobler 1997) I offered some refinements of this oversimplified picture. First, both the design of an experiment and its result are theory-laden and therefore involve a dose of interpretation. Second, some Scientist's questions may turn out to be ill-posed. A question is ill-posed when some of its presuppositions are false, where a presupposition of a question is a necessary condition for the question to have a true direct (possible and just-sufficient) answer. For example, the question 'Has Mary quit smoking?' presupposes that Mary has been smoking for some time. If this is the case, one of two direct answers, 'Yes' or 'No' is true. Otherwise, none of the two is true and the correct answer is the so-called corrective answer that denies the presupposition: 'Mary has never been smoking'. Note that presuppositions of yes-or-no questions coincide with those of its direct answers: 'Mary has/has not quit smoking' both presuppose 'Mary has been smoking for some time'. For the present purposes we may ignore the question of presuppositions of the questions other than those of yes-or-no variety.

Now, consider a situation where Nature's answers appear inconsistent, whether directly or indirectly. In such situations, Scientist is forced to abandon the hitherto explored line of inquiry and choose between two possible moves. A moderate move is to revise background knowledge to some extent in order to re-interpret some experimental designs or results. Such a move is intended to enforce Nature to change some its former answers so that the consistency of the overall set of its answers is restored. The more radical move is to decide that presuppositions of some of Scientist's questions are false. Consequently, these questions are withdrawn as ill-posed. As a result some new presuppositions

are adopted which re-define an initial position in the game, i.e. an initial collection of candidate-worlds. This can be called a revolution and, on this account, one can find a grain of truth in Kuhn's claim that after a revolution scientists move to another planet. In fact, they move to another playground of their interrogative game instead to start looking for the actual world in another repertoire of candidate-worlds. Still, I do not think of scientific revolutions as of such radical changes as Kuhn may have had it.

The above abstract characterization of this threefold mechanism of scientific change can be illustrated by a brief story of Michelson-Morley's (1881; 1887) null result. Their experiment was designed to arrive at an answer to the question about the velocity of Earth relative to ether. The null result was puzzling because other considerations suggested that Earth moves in the ocean of ether with non-zero velocity. This apparent inconsistency triggered the search for auxiliary hypotheses designed to explain the puzzle while preserving the initial question. In this way, the intermediary stage between normal science and revolution was open. The chief presupposition of the theory of light propagation through luminiferous ether was retained and the re-interpretation of Michelson-Morley's result was sought. One attempt assumed the ether partial drag hypothesis, supposedly decreasing with the distance from the Earth's surface. But the repetitions of Michelson-Morley's experiment at different heights above sea level gave still the same result. Next, Fitzgerald-Lorentz's contraction hypothesis (1889, 1892) was tried out. But its consequences were disconfirmed again. Finally, with the rise of SR (1905) the relativist revolution came about. It appeared that the question of Earth velocity relative to ether is ill-posed and the presupposition about the existence of ether is superfluous.

It must be admitted that the above story is historically oversimplified. The inquiry under consideration, as any inquiry in general, was not a sequence of precisely dated singular events. Instead, many attempts were repeated with some refinements and the ideas were being crisscrossed in time rather than being tried out consecutively. More importantly, the fundamental Einstein's paper (1905) did not refer to Michelson-Morley's result at all. Still, Einstein's problem of consistently accounting for the movement of electrified bodies had much to do with that of Michelson and Morley. And the consequences of SR were just such as if the theory was designed to solve their problem. Therefore, I think I am justified in offering this story as an illustration of my general claim about the dynamics of science.

3. Presuppositions and the nature of a paradigm

The notion of presupposition may also be helpful in explaining Kuhn's concept of paradigm. As Margaret Masterman (1970) suggested, Kuhn used 'paradigm' in at least twenty-one senses. In my view, there is a core sense of 'paradigm' that comprises (i) most general assumptions about the ontology of the world, (ii) an open repertoire of problems that are considered as scientific, (iii) the general methodological rules, (iv) the general criteria of evaluation of theories or hypotheses. All these can be accounted for as kinds of presuppositions, although not all of them are of the Strawsonian kind. The so-called pragmatic presuppositions will be discussed later.

The discussed earlier presupposition about the existence of luminiferous ether and its replacement, Relativity Principle, are good examples of presuppositions of the first kind in the above list of the components of a paradigm. The presuppositions of the second kind are about the relation between problems and their domain or, alternatively, about the categorization of problems. The third kind of presuppositions comprises both the general and specific methodological rules. Most general ones presuppose Uniformity of Nature that justifies induction and the *ceteris paribus* clause. More specific methodological rules of controlled experiment accommodate possible violations of the *ceteris paribus* clause, for example the requirement of repeating experiments with small variations in initial conditions or the rules of blind and double-blind experiments. The fourth kind of presuppositions include, apart from methodological ones, like Inference to the Best Explanation together with an explication of 'Best', some extra-scientific ones, e.g. the conformity with the general world-view (Laudan 1977, pp. 60–63).

For this reason I am inclined to propose that a paradigm is to be defined as a stock of presuppositions of scientific inquiry and a paradigm change or scientific revolution as a revision of presuppositions. Such a revision, however, does not result in incommensurability of theories because its motivations usually deliver a precise guide to modifying the hitherto existing theories. As I mentioned before, apart from Strawsonian presuppositions one has to take into account non-Strawsonian ones. In particular, in my work (Grobler 2005) I argued that idealizations and the *ceteris paribus* clause assumed in the formulation of a scientific law are presuppositions of a kind. When viewed from this angle, even if the scientific laws are not literally true – for their presuppositions may sometimes be violated – they, contrary to Nancy Cartwright (1983), do not lie either. As we shall see in the following paragraph, the question of whether this kind of presuppositions are Strawsonian or not is rather subtle.

One may object that idealizations and the *ceteris paribus* clause are not Strawsonian presuppositions because it is not the case that their truth is required for the law under consideration to have a truth-value. Actually, in Cartwrights view, because of false idealizations and the *ceteris paribus* clause, the laws are false rather than devoid of truth-value. On the other hand, in the interrogative game these assumptions are presupposed in Scientist's questions asked about the possible dependence between certain parameters. Such questions presuppose, as the necessary conditions for a question to have a true direct answer, that no other parameters interfere. This amounts to presupposing idealizations (the known extraneous interferences are negligible) and the *ceteris paribus* clause (the unknown extraneous factors do not interfere). Now, these are the presuppositions of yes-or-no question whether this or that hypothetical law about the regularity sought after is true (Hintikka 1988). By this token, in accord to what has been said earlier about the presuppositions of yes-or-no questions, they are the presuppositions of the hypothesis itself.

Still, even if they are Strawsonian presuppositions, idealizations are not the presuppositions that are constitutive for a paradigm. Instead, setting aside idealizations that are not valid in certain circumstances gives rise to special applications of a theory within the current paradigm. For example, the free fall law assumes the idealization to the effect that air resistance is neglected. But, this assumption has to be repealed in application to the fall of a parachutist. The problem of the fall of a parachutist, clearly internal for the paradigm of CM, is to be solved with the methods of that paradigm. And, its solution does not require any change in general ontology characteristic of the paradigm and does not involve standards of evaluation of its solution external for that paradigm.

Apart from these borderline ones, there are, however, some presuppositions that definitely are not of the Strawsonian type. Consider, for example, Uniformity of Nature. Nature needs not be uniform in the Millian sense in order to allow for some laws to hold. Some traits of Nature may display predictable behavior, while some other may be thoroughly capricious. Thus, even if Uniformity of Nature were generally false, it could be presupposed by some true laws. Consequently, this presupposition is non-Strawsonian, for it is not necessary for the law in question to have a truth-value. Such non-Strawsonian presuppositions are called pragmatic, insofar as they belong to the so-called common ground, i.e. to the stock of assumptions shared by all the participants of a conversation (Stalnaker 2002), in this case the participants of scientific exchange. And the common ground of a scientific exchange is precisely what Kuhn calls a paradigm. It goes without saying that pragmatic resuppositions are not necessarily non-Strawsonian; Strawsonian presuppositions may belong to the common-ground of an exchange as well.

4. Presuppositions and Wittgenstein's 'river-bed of thought' or 'hinges'

Recently, the concept of pragmatic presupposition is widely employed by the proponents of diverse epistemological theories, including Michael Williams (1996), Crispin Wright (2004), Duncan Pritchard

(2016), Annalisa Coliva (2015) and me (2016, 2019), that refer to Wittgenstein's *On Certainty*. Here are the key passages therefrom:

> 83. The truth of certain empirical propositions belongs to our frame of reference.
>
> 88. It may be for example that all enquiry on our part is set so as to exempt certain propositions from doubt, if they were ever formulated. They lie apart from the route travelled by enquiry.
>
> …
>
> 94. But I did not get my picture of the world by satisfying myself of its correctness; nor do I have it because I am satisfied of its correctness. No: it is the inherited background against which I distinguish between true and false.
>
> 95. The propositions describing this world-picture might be part of a kind of mythology. And their role is like that of rules of a game; and the game can be learned purely practically, without learning any explicit rules.
>
> 96. It might be imagined that some propositions, of the form of empirical propositions, were hardened and functioned as channels for such empirical propositions as were not hardened but fluid; and that this relation altered with time, in that fluid propositions hardened, and hard ones became fluid.
>
> 97. The mythology may change back into a state of flux, the river-bed of thoughts may shift. But I distinguish between the movement of the waters on the river-bed and the shift of the bed itself; though there is not a sharp division of the one from the other.
>
> 341. That is to say, the questions that we raise and our doubts depend on the fact that some propositions are exempt from doubt, are as it were like hinges on which those turn.
>
> 342. That is to say, it belongs to the logic of our scientific investigations that certain things are indeed not doubted.
>
> 343. But it isn't that the situation is like this: We just can't investigate everything, and for that reason we are forced to rest content with assumption. If I want the door to turn, the hinges must stay put.

Given that Kuhn is clearly influenced by Wittgenstein, no wonder that propositions that belong 'to our frame of reference' and 'the inherited background', that are 'exempt(ed) from doubt', albeit only temporarily, for 'the river-bed of thoughts may shift' correspond to what may count as components of a paradigm. They also clearly fit the characterization of presuppositions of inquiry in the account on offer. Some, e.g. Williams or Michael Blome-Tillmann (2014), being committed to Epistemic Closure – if one knows p and knows that p entails q, then one knows q – include presuppositions into knowledge. Others, like Wright, Coliva, and me, consider presuppositions as preconditions for knowledge. This, I think, is more faithful to Wittgenstein, as the above quotations make it quite clear, and also to Kuhn, whose paradigms resemble Kantian forms and categories, even if they are changeable rather than *a priori*.

Apart from general pragmatic of presuppositions, like Uniformity of Nature, there are also more specific non-Strawsonian ones that enable one to justify beliefs on the pattern of the theory or relevant alternatives originally proposed by Fred Dretske (1970). On this theory, one is justified in believing p iff one can exclude all relevant alternatives to p. Some alternatives count as irrelevant when there is no clue that suggests that they can be true. They can be ignored on the rule analogous to the *ceteris paribus* clause that licenses ignoring the interferences we do not know anything about. To illustrate this with the classical example, one may have a justified belief that there is a zebra in the zoo when one sees in the zoo an animal that looks like a zebra and the nameplate says 'zebra'. One's justification is in force even if one is not able to exclude that the animal behind the bar is a cleverly disguised mule, for normally it is presupposed that zoos do not cheat this way. Consequently, the alternative in question is not relevant. Suppose, however, that a zebra at large was seen in the vicinity of the zoo. In such circumstances one

might reasonably suspect that zoo employees painted a mule in order to hide from the public that the zebra snuck out. The presupposition to the effect that the animal is not a cleverly disguised mule is then suspended, the alternative in question becomes relevant and one is no longer justified in believing that there is a zebra in the zoo. By the same token, one is justified in accepting a scientific hypothesis when it is able to exclude relevant alternatives without being able to exclude, as Baconian Induction requires, all the alternatives. Or abandon a hypothesis in favor of an emerging alternative that appears relevant once the hitherto presupposed *ceteris paribus* clause is falsified. This kind of move represents an accommodation of incoming evidence, rather than a reaction to an anomaly.

By way of illustration let me mention one of Hazlett's (2010) examples that put into question the factivity of 'know'. It says: 'Everyone knew that stress caused ulcers, before two Australian doctors in the early 80s proved that ulcers are actually caused by bacterial infection.' Some defenders of factivity claim that 'know' is used here ironically. To my mind, the hypothesis about the causal role of microbes in developing ulcers was not a relevant alternative to that of the role of stress until the new bacteria was isolated to prove that it was able to live in the strongly acidic environment of stomach. Consequently, people really knew that stress caused ulcers, before their knowledge was updated. After all, stress is a factor contributing to developing ulcers. Moreover, infection is not responsible for all cases of peptic ulcers disease. Thus, the presence of non-Strawsonian presuppositions in science, in particular the *ceteris paribus* clause, calls for a further step against the factivity of science. In order to account for outdated knowledge it is not enough to weaken the truth-requirement to that of non-falsity, as it has been suggested in Sec. 2. It is necessary to admit that even false beliefs, like that of the causal role of stress, may count as knowledge, provided that they are justified on the pattern of the relevant alternatives theory of knowledge, and the substantial part of their content is preserved after some of their presuppositions are revised.

The question of the factivity of knowledge is not clear in the above excerpts from Wittgenstein. Kuhn notoriously claims that science is a puzzle-solving activity rather than the search for truth. Wittgenstein claims instead that in the course of inquiry 'I distinguish between true and false'. On the other hand, one does it only against 'the inherited background' (94) that is 'a kind of mythology' (95) that 'may change back into a state of flux' (97). Therefore, I think, like Coliva, that 'true' is used here in an anti-realist sense. But unlike Coliva, who adopts a deflationary view on truth, I am inclined to understand 'true' in this context epistemically, i.e. 'recognizable (justifiably) relative to "inherited background" or "adopted presuppositions"'.

Unfortunately, Wittgenstein, while putting emphasis on the indispensability of 'the mythology', says nothing about how it 'may change back into a state of flux, the river-bed of thoughts may shift' (97). To make up for his silence, Kuhn focuses on socio-psychological factors, especially scientist's disappointment because of the failures of the paradigm they work in, that force scientists to seek for a paradigm change. Kuhn's neglect of epistemic reasons for revising key presuppositions of inquiry makes an impression that a scientific revolution comes into effect out of the blue, as it were. This encourages, on Kuhn's part, Incommensurability, and on the part of his critics, accusations of irrationality. The metaphor of hinges (Wittgenstein 1969, 341–343) may prompt Kuhn's reading, but that of river-bed speaks against it.

5. Concluding remarks

On the present proposal, presuppositions are revised in order to reject questions as being ill-posed and thereby dismiss some recalcitrant puzzles rather than just abandon them in the face of prolonged failure to solve them. A non-Strawsonian presupposition can be revised in result of challenging the *ceteris paribus* clause involved in the hitherto prevalent hypothesis, which amounts to finding a novel relevant alternative to that hypothesis. Sometimes ill-posed questions are rejected as a consequence of a revision of presuppositions that has come about for independent reasons. If one looks at the present proposal as two-stage falsificationism, then one will not be surprised with this. After all, many author emphasize

the point that there is no falsification without a novel hypothesis. Recall, for example, the fates of the anomaly of Mercury's perihelion, the puzzle that long has been tried to be solved in terms of gravitational forces. It is not its persistency that has made scientists to seek a new paradigm out of their discouragement. Quite the contrary. They were inclined to abandon the puzzle rather than the existing paradigm of classical theory of gravitation. It is only the rise of General Relativity that falsified the presupposition about the rectilinearity of light propagation and ultimately terminated the search for mechanical (gravitational) causes of the anomaly in question. This explanatory gain, plus few other ones, decided about the paradigm change. Thus, a paradigm change is explained in the first place in epistemic terms, while the alleged scientist's fatigue with unsuccessful attempts at solving some puzzles and other socio-psychological factors are pushed into the background as responsible mostly for the timing rather than the substance of a scientific revolution.

Once an explanatory gain is mentioned, it is in place to comment upon an aspect of incommensurability that is known as Kuhn's losses. Kuhn claimed that scientific revolutions are not unambiguously progressive, for even if they deliver explanations for hitherto unexplained phenomena, they at the same time lose explanations that were available under the old paradigm. A flagship example is the gains brought about with the advent of Newtonian physics at the cost of giving up Cartesian explanation of coplanarity of the planet's orbits. On the present offer, Newtonian physics introduced action at a distance and thereby withdrew the presupposition that all movements were caused by a collision. This enabled Newton to propose the law of gravity that brought about explanations of lots of phenomena. Even if Newton did not explain the coplanarity of the planet's orbits at the time, his presuppositions did not prevent arriving at such an explanation in the future. Thus, an alleged loss was rather a loan taken in order to make profitable investments to be repaid with the development of Kant-Laplace nebular hypothesis.

Finally, let me come back for a while to the realist-antirealist issue. While I deny the factivity of knowledge, mostly for the sake of accounting for outdated knowledge or present-day knowledge that may turn out to be outdated in the future, I am nevertheless much sympathetic to the view that science is the search for truth rather than just a puzzle-solving activity. To make justice to the all-importance of puzzle-solving of science, in my sandwich theory of knowledge (Grobler 2016; 2019), unlike my above mentioned fellows in following Wittgensteinian motifs in epistemology, I put much attention to the role of application of knowledge (the top slice of the sandwich) in addition to its presuppositions (the bottom slice of the sandwich). On this view, in some applications falsified presuppositions may be more useful than their replacements. Thus, scientists who stick to an old paradigm may be not as irrational as Kuhn suggests. Besides, rival paradigms may well coexist.

Still, as I have suggested in the foregoing, I am inclined to think of introducing of the concept of the presupposition to refine falsificationism or the conception of science as self-correcting enterprise. Consequently, I accept Popper's (1972) notion of truth as the regulative idea of science. Moreover, in my opinion, late Kuhn could accept it as well. After all, among his values that guide theory-choice (Kuhn 1977), there are empirical accuracy, consistency and fruitfulness. These are precisely what one can take as symptoms of getting closer to the truth, or better, interesting truth. At any rate, the neglect of these values amounts to the disinterest in truth. With the reservation that, due to diversity of the applications of knowledge, there may be forked routes towards the truth and, given that the whole truth, though approachable, is unattainable, they need not converge.

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