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Kuhn and Wittgenstein: The Paradigm Priority Problem, Relativism and Incommensurability

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Abstract:

For dropping the incommensurability idea elaborated at the time of *The Structure of Scientific Revolutions*, Kuhn dismisses the concept of "revolution". The incommensurability involved the incomparability of theories. In this new environment, the revolution is replaced by conceptual reformulation and the incommensurability becomes occasional. The linguistic turn in Kuhn's thought involves conceptual changes whose aim is to get around the accusation of relativism that the former notion of incommensurability arouses. The most fundamental effect of these conceptual reformulations is the commitment to a traditional conception of semantics. It changes the comprehension of the historical and social nature of the foundations of the changes that scientific knowledge goes through. The comparison between the answer to the problem of paradigm priority attributed by Kuhn to Wittgenstein and Wittgenstein by himself shows that the basis of the normative nature of paradigm commitment is an essentialist concern. In the second half of this paper, I will evaluate Kuhn's manner of getting around the problems of incommensurability in contrast to Wittgenstein's view of philosophy dealing with similar issues in *On Certainty*. This enables one to essay answers to the problems of incommensurability without relativism or any commitment to a traditional conception of semantics. These contrasts show how far Kuhn's new theory of science departs from the Wittgensteinian inspiration and abandons the point of view of *The Structure of Scientific Revolutions.* The development of these two halves makes it possible to indicate reasons to believe that questions concerning the theory and history of science can benefit largely from a grammatical exploration, which gives rise to a theory of science inspired by Wittgenstein's thought, as Mauro Condé suggests.

Keywords: Kuhn, Wittgenstein, incommensurability, paradigm, grammar, certainty.

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For Bob Fernandes,
a paradigm.

Scientific activity can never be a lonesome enterprise. As much as the development of science involves the work of an extraordinary scientist, the results of his genialness can only be evaluated in the light of a community made up of a strong network of commitments that

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supports ordinary scientific work. These are conceptual, theoretical, methodological and instrumental commitments that take the form of rules, practices, procedures, operating principles, interactions, behaviors, habits. These commitments are a source of determination of scientific practices insofar as they define the margin of maneuver in which the work of science is developed. Whatever may be outside the defined limits may well have been denied the characterization of scientific, which means that the standards to which the knowledge produced is submitted, those built by the scientific community over time, allow the recognition of knowledge as such. Therefore, a well-established scientific specialty is equipped with "rules that tell the practitioner of a nature specialty what both the world and his science are like" (Kuhn 1970, 42). From Kuhn's point of view, commitments provide the scientist with the security in which he could not concentrate on solving the problems "that these rules and existing knowledge define for him" (Kuhn 1970, 42).

The rules define the genuinely scientific nature of problems that it is possible to clarify "the nature of normal scientific practice" from them. After all, normal science is defined by what scientists normally do and, as commitments are constitutive of the scientific community, then adherence to them defines belonging to a community while integrating it comprises the practice of science submitted to those same commitments. The community bond depends on the solidity of this network of commitments, which must manifest itself with each new act of the scientist and act as a blind spot in all his procedures. They reflect agreements in which practices are anchored and without which there is no need to talk about the scientific community. The normative nature of this network of commitments has a history that consists of its establishment throughout the practices, being its validity relative to the community to which these same practices belong. As there is no sense in having practices without the possibility of regulating actions and there is no such rule in the absence of standards for correcting actions, having such a network of commitments supposes the existence of a community built around them. The world's and science nature revealed by the rules determine the scientist's catch of sight, what his field of vision includes and what could only be outside of it. Everything suggests that these are natural consequences of the Kuhnian idea of science guided by scientific achievements instituted as models of problems and solutions. According to Kuhn, it seems to be inseparable from the science perspective the idea of scientific development as established in a dynamic in which continuities and ruptures take place, normal and revolutionary, as well as the idea that history of science does not consist of a linear path towards the truth. The incommensurability between rival theories takes place as a very natural result of this conception of science. After all, continuity is nothing more than the perpetuation of practices according to models established as normal, whereas the rupture consists in the abandonment of such models, being its most fundamental consequence to stop practicing the science defined by them (Kuhn 1970, 34).

This way of understanding scientific comprehension seems to assume the rules preponderance over scientific practices as fundamental to science development. After all, the regulation of scientific conduction would be essential to traditions construction without which science would not develop once they comprise the models improvement to which practices are submitted. However, this is not exactly how Kuhn believes things are going. For him, the highly determined character of science is not to be confused by science being "entirely determined by rules" (Kuhn 1970, 42). It is true that there is a set of rules to which the entire scientific community adheres, but it is not true that this set holds everything that the community has in common, nor that those rules are necessarily explicit. With this, Kuhn denies that the rules are the source of coherence of the tradition instituted by normal science. The rules, therefore, can play only a secondary role, because "derive from paradigms, but paradigms can guide research even in the absence of rules" (Kuhn 1970, 42).

Kuhn's consideration that a paradigm may well preside a scientific community in the absence of rules states more than the fact that paradigms do not need rules. Although it is a highly determined activity, normal science does not lack the determination of rules, which is

not to say that scientific practices can take place without guidance. Apparently, the question, for Kuhn, is how much practical references can guide the activities of a community giving it the determination that rules could also be able to check. The priority of paradigms over rules, denotes more than a secondary character of rules; it means that the determination of scientific practices can dispense with rules explanation capable of delimiting the worlds and science nature. So, scientists may well agree on the lasting character of certain scientific achievements and then assume them as guiding their specialty and "sometimes without being aware of it, about the particular abstract characteristics that make those solutions permanent" (Kuhn 1970, 44). Here, it is not just the absence of a body of rules that enjoys unanimity but the absence of a standard interpretation of the paradigm (See Kuhn 1970, 44). Both, according to Kuhn, would not represent an impediment to the proper fulfillment of the guiding function of the paradigm. After all, nothing prevents normal science from being guided "by the direct inspection of paradigms" (Kuhn 1970, 44).

The "Direct inspection of paradigms", whatever that means, seems to be the last resort to keep the typical guiding function of paradigm upright. However, it is a favorable field for disagreement on issues that should be a peaceful point due to intrinsic requirement to the same function exercised by the paradigm. The absence of an adequate body of rules paves the way for the commitments relativization that constitute the scientific community. It is not an effective remedy to affirm that the absence of paradigm reduction to rules that enjoy unanimity does not prevent the paradigm from fulfilling its orientation function for such relativization, as it seems to be at stake how to preserve community engagement in tradition on the absence of defining criteria for the same tradition. In other words, criteria are lacking to assess the fit between practices and the paradigm that guides them. Paradigms are, therefore, a source of coherence in tradition, but their preservation depends on that high determination that characterizes scientific activity. As there is no agreement on how the paradigm should be interpreted, there also does not seem to be a guarantee of determination, which must manifest itself in a shared understanding of problems and solutions. For this very reason, in this case, the question for Kuhn becomes (1) "what restricts the scientist to a particular normal-scientific tradition?" and (2) "What can the phrase 'direct inspection of paradigms' means?" (Kuhn 1996, 44).

The connection between Kuhn's history of science and Wittgenstein's philosophy is well known, and it is used to solve problems that the latter does not prove capable of facing alone. In this case, for example, as we know, Kuhn believes that Wittgenstein, "although in a very different context", presented "partial answers to questions". Everything happens as if Kuhn left the problem, he just identified to Wittgenstein, whose formulation is summarized in (1) and (2) and which fate of the main concept of his work seems to depend. It is not an exaggeration to say that this is one of the most fundamental philosophical problems formulated by Kuhn in *The Structure of Scientific Revolutions*. However, if this is the case, it is as if Kuhn gave Wittgenstein's philosophy the answer to the main question of his work. Kuhn presents the formulation of Wittgenstein's question which, according to him, is the starting point for Wittgenstein's exploration of the problem. As is natural, his formulation must have its own diction, which allows us to think that he does not leave the main problem of his work simply in the care of Wittgenstein.

Kuhn's thought linguistic turn about science seems to leave behind fundamental aspects of scientific rationality comprehension as a historical and social construction that could be understood as the main legacy of his work. The idea of incommensurability is undoubtedly an important indication of the fundamental character of what is left behind. The abandonment of this idea has the purpose of saving scientific rationality from relativism and dogmatism in which its effects immerse science. However, Kuhn's moderate version of incommensurability, as it were, is accompanied by a kind of essentialist commitment – this is my hypothesis. Everything suggests that there would be no trace of essentialism in *The Structure of Scientific Revolutions*. From the confrontation between what is typical of Kuhn's

way of formulating and answering his questions and Wittgenstein's perspective for himself, it must be possible, I believe, to make it clear that Kuhn's way of presenting Wittgenstein's perspective reveals philosophical concerns that are against Wittgenstein's thinking, insofar as they imply some essentialism. This is not to say that Kuhn's new ideas are latent in *The Structure of Scientific Revolutions*, but, at best, that future essentialism is not entirely foreign to him, although adherence to essentialism represents an undeniably radical upheaval in Kuhn's thinking.

I.

The task of the historian of science, to some extent, is to make explicit the rules that shape the commitments accepted by the scientific community. For Kuhn, the acceptance of the rules is not homogeneous, nor would it need to be. Moved by the intention of describing the guiding principles and rules of the community, the historian would elaborate general statements capable of describing the convictions on what scientific practice is based. As a matter of fact, some of these statements are taken for granted. Others may well be rejected by a portion of the community which does not recognize them. For this reason, both the task of describing rules and paradigms is unsatisfactory. As a result, it does not seem that the scientific community is not determined by rules. This means that only a typical difficulty in the historian's work of identifying what the community has in common as materialized in rules in which that same community would submit. The question, therefore, becomes the extent to which "the coherence of the research tradition" can be understood; without that, it is simply not possible to speak in community, in terms of rules.

The origin of this difficulty is that it is possible for scientists to disagree on the reasons that support the paradigm to which their practices are submitted without, therefore, deserting the community to which they belong.

> Scientists can agree that a Newton, Lavoisier, Maxwell, or Einstein has produced an apparently permanent solution to a group of outstanding problems and still disagree, sometimes without being aware of it, about the particular abstract characteristics that make those solutions permanent. (Kuhn 1970, 44)

This means that a scientist may well have his practices adhering to a certain paradigm without being able to explain it in terms of rules. And, furthermore, interpretation divergence in relation to which rules are representative of paradigm is not a sufficient obstacle to prevent paradigm from guiding research.

At this point, the difficulty acquires even more dramatic airs, as the criteria for defining belonging to the community are at stake. After all, if a group of scientists is engaged in defending a particular interpretation of the paradigm, how to define whether they are in tune with the tradition determined by the paradigm? This difficulty gains a more serious aspect because of the possibility, left by Kuhn, that the absence of a unanimous interpretation does not prevent the paradigm of exercising all its strength of determination to think of a community divided between those who share one or another interpretation. It is clear that the situation is different, since it is not a question of confrontation between paradigms, then the dissenting interpretation is not a cause of incommensurability. However, the situation is analogous to the conflict between two divergent traditions, each determined by a paradigm. Not having a standardized interpretation is the same as not having a reduction of the paradigm to rules that enjoy unanimity. That is why the difficulty formulated by Kuhn is about what "restricts the scientist to a particular normal-scientific tradition" in the absence of "a competent body of rules". "A competent body of rules", of course, means "rules to which the paradigm has been reduced and which enjoys unanimity". The question, in this case,

seems to cover up the real question. The question is about what gives unity to the community, that unity without which the community is not a community. The path to the solution, paved by Kuhn, seems to have no more than the virtue of opening up the problematic and unresolved character of the question since it is based on the distinction between interpretation and direct inspection of the paradigm. Kuhn himself recognizes the problematic character of the solution when questioning what "direct inspection of paradigms" means. The starting point for the solution is the notion of "family resemblance" attributed by Kuhn to Wittgenstein (Kuhn 1970, 45).

According to Kuhn, what we need to know to use terms like "chair", "leaf" or "game" in an unambiguous way, without provoking discussions, is explained by our way of using language and by the world in which we apply it (Kuhn 1970, 45). There would be no set of characteristics applicable to the members of a class and only to them, nor does there need to be one, because, for example, when faced with an unknown activity we appeal to the word "game", because "what we are seeing bears a close 'family resemblance' to a number of the activities that we have previously learned to call by that name" (Kuhn 1970, 45). As such, "games, and chairs, and leaves are natural families, each constituted by a network of overlapping and crisscross resemblances" (Kuhn 1970, 45). The existence of such a conceptual network, according to Kuhn, would be sufficient to guarantee the identification of hitherto unknown objects.

The coherence of the research tradition of a given scientific community, from this point of view, is safe in the absence of a body of rules exactly insofar as something similar holds for problems and techniques that take place in that same tradition:

> What these have in common is not that they satisfy some explicit or even some fully discoverable set of rules and assumptions that gives the tradition its character and its hold upon the scientific mind. Instead, they may relate by resemblance and by modeling to one or another part of the scientific corpus which the community in question already recognizes as among its established achievements. Scientists work from models acquired through education and through subsequent exposure to the literature often without quite knowing or needing to know what characteristics have given these models the status of community paradigms. (Kuhn 1970, 45)

What constitutes the scientific community is not a set of rules, but a way of proceeding assimilated from education. In this sense, it is inherent to the scientific activity not to call into question procedures accepted by the community and, therefore, the paradigms work in a tacit way, which explains (1) the difficulty for the historian of science to discover the rules that led specific traditions and can be clarified by considering the (2) nature of science education (Kuhn 1970, 46). Difficulty (1) would be identical to that difficulty faced by the philosopher when trying to discover "what all games have in common". Scientists (2) do not assimilate laws, theories and concepts abstractly or in isolation from the scientific activity itself. On the contrary, and this is a characteristic feature of scientific education, laws, theories and concepts are assimilated as they are applied. The introduction of scientists to a new theory, in this sense, is done in such a way that application is inseparable from theory. "A new theory is always announced together with applications to some concrete range of natural phenomena; without them, it would not be even a candidate for acceptance" (Kuhn 1970, 46). As such, the historian's frustration in seeking the body of rules defining a specific tradition is explained by the fact that scientific activity does without such a body of rules.

However, the question of rules is important for Kuhn, not only because it shows that the historian's frustration would be justified, but above all because the clarification of its origin brings to light the relationship between scientists and the paradigm that guides their practices, making the paradigm priority over visible rules. Scientific education, in this case, plays a key role, being decisive in the sedimentation of a tradition. In fact, the nature of

scientific education, as understood by Kuhn, is fundamental to the idea of scientific development as constituted by continuities and ruptures. Scientific development depends on the establishment of specific traditions whose construction is made by the scientific work carried out in the light of their models, and this work is responsible for improving these same models to the same extent that it sediments them. The indisputable character of models, concepts and theories thus plays an essential role in scientific development understood in this way. The role played by the paradigms depends on the community's silence about models, concepts and theories. "Normal science can proceed without rules only so long as the relevant scientific community accepts without question the particular problem-solutions already achieved" (Kuhn 1970, 47). In effect, scientists become interested in the rules to which the paradigms that conduct their activities could be reduced when these same paradigms are insecure, which would happen in moments of the history of science called by Kuhn as "pre-paradigm period", which are characterized "by frequent and deep debates over legitimate methods, problems, and standards of solution, although these serve rather to define schools than to produce agreement" (Kuhn 1970, 47-8).

The tacit character of the paradigms is an indication that scientific knowledge is guided by a kind of normative commitment that varies over time, which means that "scientific knowledge is thus relative to the variable normative commitments and changing of real scientific groups" (Doppelt 2001, 159). It is true, as Doppelt intends, that this conception of scientific knowledge implies that the changes that science historically goes through are incompatible with the positivist idea that it is timeless (Doppelt 2001, 160). As a consequence, scientific development is marked by variability in normative commitments. After all, over time, necessarily, the models to which the scientific activity of a community undergoes change. However, the way of understanding adherence to a new network of commitments seems to be what is most unique about Kuhn's thinking about science. If the debate, in pre-paradigmatic periods, on the legitimacy of methods, problems and solutions serve much more to define schools than to produce agreements, it is because the justification for adhering to a paradigm does not involve the presentation of reasons. Scientific schools, therefore, eventually in conflict, would not be in a position to prove for what reasons the paradigm they defend is better, "or even that any reasonable person at the time would be compelled to accept is as the better" (Barnes 1982, 55).

Scientific activity does not require scientists to be able to give reasons that justify what they normally do, nor does it lack it. After all, scientific action moves within the margin of maneuver left by what the community recognizes as a model of work, what it has in common. This means, among other things, that adherence to one model defines the community, distinguishing it from another, and the refusal to one model is always adherence to another. Such adherence can only look like a normative commitment, because only in this way is it able to guarantee the coherence between the choices made by the scientist and the tradition to which his work belongs, that is, only in this way the persistence of acts repeated over time in the absence of rules can be guaranteed. This commitment defines procedures, problems and solutions, that is why it defines the membership in a community, as it is also in the light of it that the scientific is distinguished from the non-scientific. "The reception of a new paradigm often needs a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely 'unscientific'" (Kuhn 1970, 103).

In the same sense, since joining a network of commitments does not involve the presentation of reasons, then an eventual incompatibility between theories guided by antagonistic commitments cannot also be resolved by presenting reasons. In a word, "the competition between paradigms is not the sort of battle that can be resolved by proofs" (Kuhn 1970, 143). Scientists, in a sense, are not in a position to present or understand them by virtue of their education. It is not an inability, but a characteristic of scientific work. Adherence to the network of commitments that connects the disciplinary development of science to a tradition is, above all, practical. The battle between paradigms is then the

confrontation between ways of practicing science, whose foundations do not require enunciation. It seems to be exactly this characteristic of the science responsible for leading Kuhn inevitably to defend the incommensurability of the paradigm. Observed from a point of view outside them, antagonistic paradigms only comprise "incommensurable ways of seeing the world and of practicing science in it" (Kuhn 1970, 4). Seen one from the perspective of the other, the ways of seeing the world and practicing science can, at the very least, give rise to the conclusion that this is a method error.

Incommensurability is a natural consequence of the priority of rule paradigms. Kuhn's argument that supports the priority of paradigms, as we have seen, has as its starting point the following question: "In the absence of a competent body of rules, what restricts the scientist to a particular normal-scientific tradition?" (Kuhn 1970, 44). Kuhn derives his argument from what he considers Wittgenstein's "partial answers" to questions of this kind. However, Kuhn makes it appear that Wittgenstein's philosophical interest, in this case, is guided by the question of what we need to know "in order that we apply terms like 'chair', or 'leaf'; or 'game' unequivocally and without provoking argument" (Kuhn 1970, 44-5). It was, according to Kuhn, from this question that Wittgenstein elaborated the "partial answers". It does not matter, here, to evaluate the correctness of Kuhn's interpretation, even because Kuhn himself warns the reader, stating in a note that "part of the point that follows cannot, therefore, be attributed to him", although, in my view, in this case, almost nothing can be attributed to Wittgenstein (Kuhn 1970, 45).

The attribution of this question to Wittgenstein, as made by Kuhn, reveals that Kuhn's argument makes the transition between the question about what guides the practice of normal science, in the absence of an adequate body of rules, to the question about what problems and research techniques of a normal science need to have in common to ensure that scientific work will follow the route determined by the current paradigm. In this sense, after presenting a paraphrase of Wittgenstein's answer to the question he is assigned, Kuhn begins his own argument by saying that "what these [the various research problems and techniques] have in common is not that they satisfy some explicit or even some fully discoverable set of rules and assumptions that gives the tradition its character and its hold upon the scientific mind" (Kuhn 1970, 45). The excerpt from Wittgenstein's work, indicated by Kuhn, comprises the part between pages 31 and 36 from the 1953 edition of *Philosophical Investigations*, which corresponds to the excerpt from § 64 to § 79 of any other edition. According to Kuhn, "for Wittgenstein, in short, games, and chairs, and leaves are natural families, each constituted by a network of overlapping and crisscross resemblances", which makes the identification of objects or activities successful. In the section between §§ 64-79 of the *Philosophical Investigations*, however, Wittgenstein presents the reasons why we call language a diverse and broad set of processes and phenomena without being able to recognize something in common in them. Wittgenstein's starting point, in § 65, is exactly the refusal to attribute something in common to everything we understand as language, so that the notion of "family resemblances" (Wittgenstein 2009 § 67), to which Kuhn refers is a key player in the critique of essentialism, which results in the question of what, after all, there is in common "to all these activities, and makes them into language or parts of language" (Wittgenstein 2009 § 65)

The tone of these considerations made by Wittgenstein that led his thinking to the notion of "family resemblances" is given in § 65 itself:

> Instead of pointing out something common to all that we call language, I'm saying that these phenomena have no one thing in common in virtue of which we use the same word for all a but there are many different kinds of affinity between them. And on account of this affinity, or these affinities, we call them all "languages". I'll try to explain this. (Wittgenstein 2009 § 65)

The elucidation promised by Wittgenstein is made from the contrast between the typically essentialist demand that there is something in common and how things really happen – between what should be like this and what we see for having abandoned that demand (Wittgenstein 2009 § 65). As paraphrased by Kuhn, Wittgenstein then explores similarities and dissimilarities of the activities we call 'game' and concludes that "we apply the term 'game' because what we are seeing bears a close 'family resemblances' to a number of the activities that we have previously learned to call by that name" (Kuhn 1970, 45). A network of overlapping and interlocking similarities is sufficient to identify something like a game. In the words of Wittgenstein himself, "we see a complicated network of similarities overlapping and crisscrossing: similarities in the large and in the small" (Wittgenstein 2009 § 66). And if, for Wittgenstein, there is no better characterization for these similarities than "family resemblances", it is because they "for the various resemblances between members of a family a build, features, color of eyes, gait, temperament, and so on and so forth an overlap and crisscross in the same way" (Wittgenstein 2009 § 67).

Kuhn's paraphrase of Wittgenstein's thought is, of course, limited to what concerns the analogy between 'game' and 'research problems and techniques'. Therefore, Kuhn is in a position to say about the existence of essential characteristics to what we call the 'game' that "Wittgenstein, however, concluded that, given the way we use language and the sort of world to which we apply it, there need be no such set of characteristics" (Kuhn 1970, 45). As is also natural, Wittgenstein's conclusion does not matter in analogy, according to which 'game' is an intrinsically vague concept. "For how is the concept of a game bounded? What still counts as a game, and what no longer does? Can you say where the boundaries are? No" (Wittgenstein 2009 § 68). After all, the vagueness that does not disturb the use of the word 'game' might not be desirable if applied to models that function as a source of guidance for scientific activity, on whose function the coherence of work depends on the scientist with the tradition to which he is linked, however flexible such models may be. Nor can the commitments that determine the leeway for activities reputed to be scientific be vague, just as science, from the Kuhnian point of view, could not survive without having something in common with the activities of scientists engaged in the same tradition.

At least at first glance, Kuhn's conception of scientific development would not survive this vagueness without experiencing an indeterminacy that would cause the collapse of the guiding function exercised by paradigm. It is true, however, that Wittgenstein admits that use of words is not entirely regulated, so that he experiences some indeterminacy. "But no more are there any rules for how high one may throw the ball in tennis, or how hard, yet tennis is a game for all that, and has rules too" (Wittgenstein 2009 § 67). On the contrary, for Kuhn, the coherence of the research tradition may well "not imply even the existence of an underlying body of rules and assumptions" (Kuhn 1970, 70-1). The confidence conferred to scientific practice by the paradigm allows scientific investigations to dispense with any discussion of the scientific problems and solutions legitimacy, as questioning it would imply to discard the paradigm to which scientists are committed. However, the fact that scientists do not argue about what reasons a problem or solution is considered legitimate may mean, for Kuhn, that "at least intuitively, they know the answer" (Kuhn 1970, 70-1). Although everything happens as if Kuhn subscribes to Wittgenstein's "partial answers", the development of his argument and its implications show that appearances are deceiving because his concern turns to what there is something in common or essential to scientific activity and which is capable of defining a tradition. Such concern, of course, determines your way of reading Wittgenstein. The normative character of the scientific commitment to the paradigm seems to excuse the scientist from presenting reasons that justify it. Perhaps, for this reason, it is closer to the way in which it understands that Wittgenstein's question has traditionally been answered than to the solutions that Wittgenstein's thought formulates to that same question: "That very old and has generally been answered by saying that we must

know, consciously or intuitively, what a chair, or leaf, or game is. We must, that is, grasp some set of attributes that all games and that only games have in common" (Kuhn 1970, 44).

II.

Incommensurability, according to Kuhn, is the main theme of *The Structure of Scientific Revolutions* (Kuhn 2000, 91). To that extent, themes such as scientific rationality and relativism would be far from having a central role in its reflection on science. Incommensurability plays a central role in Kuhn's philosophy of science, because for him it is "an essential component of any historical, developmental, or evolutionary view of scientific knowledge", in such a way that incommensurability would not be "the threat to rational evaluation of truth claims" as its critics claim (Kuhn 2000, 91). However, criticism of the idea of incommensurability does not necessarily imply understanding rationality or relativism as the main targets of Kuhn's thought. Relativism is understood as an effect of applying the notion of incommensurability. At least, this is the case in Friedman's criticism. According to him, the lack of agreement on the rules that govern the transition to a new scientific paradigm does not seem to leave any scope for understanding that such transition can be rational (Friedman 1947, 48). "Non-rational factors, having more to do with persuasion or conversion than rational argument, must necessarily be called in to explain the transition in question" (Friedman 1947, 48). Since it is not possible to explain the transition between one paradigm and another by appealing to rational factors, then the normative commitment that determines the link between scientific activity and research patterns can only have the nature of a profession of faith (See Kuhn 1970, 158). Thus, the very notion of scientific rationality would be left to the fate of the history of science development, becoming the result of an arbitrary choice.

Incommensurability plays an important role in the development of science, for Kuhn, mainly because it is linked to the idea of the scientific revolution, as a fundamental event for scientific development, which means that science does not develop without crises that lead to a rupture with the vision associated with the prevailing tradition. What makes these breaks possible is the fact that scientific development is guided by performance standards built over time. The idea that science, as Barnes intends, is not "a set universal standards, sustaining true descriptions and valid inferences in different specific cultural contexts" (Barnes 1982, 10) is linked to this way of understanding the history of science. On the contrary, scientific models are nothing more than a specific form of culture, so science could very well be the object of sociological study like any other form of knowledge or culture (Barnes 1982, 10). Interestingly, relativism seems to stem from what is perhaps the most distinctive aspect of Kuhn's history of science: the very conception of science. The relativism resulting from the idea of incommensurability, identified by Friedman and other critics, can be mitigated if it is considered that incommensurability does not imply incomparability. And, it seems, this is exactly what Kuhn will try to do, as Friedman shows (See Friedman 1947, 48-49).

The main consequence of the emphasis on the historical and social nature of the models that guide scientific practices and, therefore, determine the development of science, is to understand scientific knowledge as characteristically discontinuous. This, it is true, does not mean that science is characterized by a state of permanent revolution. The revolutions that mark the discontinuity of scientific knowledge, in a sense, have their conditions built from the work of normal science, done in continuity with the current scientific vision. It is not that normal science fuels revolutions but that the construction that characterizes normal science is complementary to the reconstruction of which revolutions consist. "Revolutions are responses to problems within traditions of research, not external disturbances" (Barnes 1982, 56). The fact that science proceeds based on criteria and standards shared by a community is perhaps the most important factor in the relationship between continuity and

discontinuity. Undoubtedly, the reconstruction in which revolutions consist implies changing criteria and research patterns, which is manifested in a radical transformation of culture, procedures and perception. For that reason, "if we examine a scientific field before and after a revolution, we observe what are essentially two distinct ways of life, maintaining two distinct systems of verbal culture" (Barnes 1982, 55).

Kuhn imagines that the construction of a new reference system, as a result of the revolutions, inaugurates a new way of seeing the world, this view of the world being transmitted tacitly by science education. The sociological nature of such a construction means, above all, that science is something practical, which seems very trivial. It ceases to seem trivial inasmuch as it implies that this implies refusing to understand scientific choices as determined by experimental logic, since the consideration of an experiment is itself made from the point of view of the worldview determined by the paradigm. It is precisely at this point that Kuhn's philosophy of science gives rise to the charge of relativism. After all, if paradigmatic determinations should prevail over the logic of scientific research, then the decision about which is the best paradigm cannot be determined by logic. Consequently, the decision is not based on evidence, nor could it be proved, which is an effect of the immeasurable nature of the paradigms (See Barnes 1982, 55-56).

The revolutions are a response to unpredictable problems from the point of view of normal science. The reorientation they promote involves, therefore, the construction of a new research model capable of solving them. The unpredictability of these problems allows us to characterize them as enigmas, since their eruption breaks with the regularity of research and undermines the security that allows the development of normal science. Thus, the construction of a new paradigm returns the essential assurance to the practice of science. In this sense, the solution to a scientific puzzle causes a new way of seeing the world flourish. It understands the break with that old point of view from which the anomalous problems would remain unsolvable. It is also in this sense that scientific knowledge is inseparable from historicity and social conditioning. However, too much emphasis on this may cover up the philosophical question that seems to guide Kuhn's way of thinking, which does not stop at answering which reasons arises from the solution of a scientific enigma to a way of seeing the world. The most fundamental philosophical question, for Kuhn, is associated with incommensurability because the most important aspect of incommensurability is the denial that scientific knowledge is merely cumulative. It does not seem to be for any other reason that Kuhn considers incommensurability to be a fundamental aspect of his thinking. Against the scientific knowledge perspective that it is a cumulative succession of ideas, Kuhn understands that disruptions are decisively important in science. For this reason, the philosophical question that guides his thinking concerns the nature of the change brought by ruptures that mark science development. And I do not think Kuhn's answer to that question is as clear and simple as Barnes makes it out to emphasize the social and psychological nature of adhering to paradigms (See Barnes 1982).

Incommensurability is the source of rupture that constitutes the scientific revolution. There is, therefore, no way to get rid of the undesirable effects of incommensurability, such as relativism and the unfounded character of theoretical choices, without leaving incommensurability behind. However, this also involves reviewing the conception of scientific knowledge linked to the idea of a paradigm and a scientific revolution. The linguistic turn through which Kuhn's thought goes is an attempt, as Condé has said, to solve problems arising from his theory of science, whose main elements are paradigm, incommensurability and scientific revolution (Condé 2020, 86). According to Kuhn himself, the reworking through which his thinking goes replaces discontinuity as a characteristic feature of scientific development with "significant reformulation" (Kuhn 2000, 87). Kuhn will claim that this is a conceptual review, but it is difficult to imagine the conceptual change resulting from this review as not being an abandonment of concepts central to his theory of science, such as

scientific revolution and incommensurability. With this, the very conception of scientific knowledge also seems to undergo reformulation.

The concept of "incommensurability" seems to appear only as the same word for a radically different concept, starting with the fact that it no longer implies or involves non-comparability between theories. It is true that the conceptual reformulation typical of the development of science may have the consequence of making statements that were previously accepted as possible descriptions of reality unintelligible, but this does not imply the impossibility of translating them into subsequent scientific terminology. It is all a matter of mastering the new and the old language. The incomparability makes sense only from a monoglot perspective. Therefore, faced with untranslatable statements, "the historian becomes bilingual, first learning the lexicon required to frame the problematic statements and then, if it seems relevant, comparing the whole older system (…) to the system in current use" (Kuhn 2000, 77). Without such a domain, it is impossible for the historian to understand the meaning of the statements or have access to the possible worlds that the conceptual system that accommodates such statements encloses.

The domain of the system is understood by Kuhn as a process of re-education that involves "the recovery of the older lexicon, its assimilation, and the exploration of the set worlds to which it gives access" (Kuhn 2000, 85). The linguistic turn seems to make incommensurability only mean the absence of a common language to which divergent theories can be reduced (See Kuhn 2000, 36). However, according to Kuhn, it is only a matter of correcting the understanding of the old notion, eliminating confusions produced by the metaphorical use of the term. Therefore, more modest than its critics suppose, incommensurability, according to Kuhn, is safe from all the criticism that consists of associating incommensurability with incomparability of theories.

The version of incommensurability that Kuhn characterizes as modest is restricted to regions of the theory, being, therefore, a question of linguistic significance, no longer a conflict between radically incompatible perspectives. However, it seems inconsistent that the notion of incommensurability leads Kuhn to state that "when Aristotle and Galileo looked at swinging stones, the first saw constrained fall, the second a pendulum" (Kuhn 1970, 121) does not involve just a dispute over meaning. It is for no other reason that Kuhn considers the application of the "demonstrations of a switch in visual gestalt" to what is happening with scientific revolutions so suggestive: "What were ducks in the scientist's world before the revolution are rabbits afterwards" (Kuhn 2000, 111). Change, in this case, cannot be understood as a conflict between interpretations, since interpretations presuppose a paradigm that determines them. Therefore, changes of this nature are not simply corrections of occasional errors in the old system to be replaced by the new one (See Kuhn 2000, 15). Instead, they involve not only changes in natural laws, but also changes in the criteria according to which those laws relate to nature. The character of this type of change explains why scientific development cannot be merely cumulative.

The world perceived by a scientist changes according to the paradigm shift because perception is determined by the paradigm. As a result, reactions, expectations and scientific beliefs also change. From this perspective, the rupture caused by scientific revolutions cannot be stopped in the realm of language. In refusing to call the Sun a "planet", according to Kuhn, Copernicans were not simply denying a meaning, but elaborating a new meaning of "Sun" that would make it possible to continue "to make useful distinctions in a world where all celestial bodies not just the sun, were seen differently from the way they had been seen before (Kuhn 1970, 128-9). For the same reason, later, Kuhn will say that the sentence "in the Ptolemaic system planets revolve about the earth; in the Copernican, they revolve about the Sun" is incoherent, as each occurrence of "planet" connects to nature differently (Kuhn 2000, 15). The definition of incommensurability compatible with this perspective, according to Kuhn himself, is "the impossibility of defining the terms of one theory on the basis of the terms the other", but it also concerns the "methods, problems-field, and standards of

solution" (Kuhn 2000, 34), not dwelling on the scope of language, so that it seems unequivocal that the linguistic turn causes the abandonment of this concept. According to the version defended by Kuhn, the main reason for incommensurability concept changes is the abandonment of theoretical changes with "gestalt switches". Such identification, according to him, would have been produced by the dubious use of the idea that divergent theories contained divergent worldviews (Kuhn 2000, 34). This would have resulted in an emphasis on the visual character to the detriment of the conceptual nature of theoretical change. The discontinuity that marks the development of science is intrinsic to the understanding of theoretical changes as being gestalt switches. From Kuhn's viewpoint, this characterization of theoretical changes would have resulted from his theory of science having understood scientific processes in the light of his "experience with the process by which historians move into the past" (Kuhn 2000, 87). As a general rule, scientific work is alien to the past, nothing more natural than the image of scientific knowledge as cumulative is more familiar to scientists. "The science is unique among creative disciplines in the extent to which they cut themselves off from their past, substituting for it a systematic reconstruction" (Kuhn 2000, 87). Understanding that that image of scientific development does not do justice to the past, nothing more natural also that the historian characterizes "experiences breakthrough as a gestalt switch" (Kuhn 2000, 88). Furthermore, this characterization would not have taken into account obvious differences between individuals and groups, abusing the metaphorical use of the "gestalt switch". "Communities do not have experiences, much fewer switches. As the conceptual vocabulary of a community changes, its members may undergo gestalt switches" (Kuhn 2000, 88), but not all of them equally or all the time. Although Kuhn strives to make it appear the opposite, his considerations about gestalt psychology, in *The Structure of Scientific Revolutions*, can hardly be understood as the result of a vocabulary dubiousness. They present themselves much more as a testament to the commitment of his theory of science to the philosophical perspective that this modality of psychology inspires. And it seems to be precisely from this commitment that his theory of science is undone by leaving behind the old notion of incommensurability.

The abandonment of the gestalt switch metaphor leads Kuhn's thinking to understand incommensurability as only punctual, no longer implying incommunicability or incomparability. The assertion that different theories and practices are not necessarily incommunicable or incomparable does not necessarily imply that there is "positive facts or transcendental metaphysics as the absolute basis of different scientific knowledge" (Condé 2014, 55). However, this is not exactly the path taken by Kuhn's theory of science, which starts to consider the primacy of the scientific community over its members as determined by a conceptual structure that gives unity to the community and sets it apart from other groups. One such lexical structure is "a module within the head of an individual group member" (Kuhn 2000, 104). The conceptual framework works in the way of "preconditions of possible experience", like the Kantian categories, but unlike them, exposed to the possibility of change. Lexicon's theory provides Kuhn with an answer to the question about what is essential to scientific activity capable of defining a tradition or what is in common with rival theories that allow comparison and communication between its defenders. This leads Kuhn's theory of science to abandon that old notion of incommensurability. However, the price to pay for this is the requirement that there be "something permanent, fixed, and stable", something like "Kant's *Ding an sich*", underlying the processes of change that characterize the development of science.

Kuhn's solution to the undesirable effects of incommensurability is clearly linked to a "traditional semantic conception of language based on the idea of representation of nature by means of categories, conceptual schemes, etc." (Condé 2020a, 376) of that is the consideration of what is underlying the transformations that science goes through as "ineffable, indescribable, undiscussable" (Kuhn 2000, 104). Thus, changes and transformations are based on a structure, which can be revised, but which is maintained as a

guarantee of stability without which changes and transformations would not be possible. Scientific revolutions, from this perspective, must be understood as "fundamental change in some taxonomic categories", as they are episodes that confront scientists "with problems like those the ethnologist encounters when trying to break into another culture" (Kuhn 2000, 94). Changes in the conceptual framework that determines scientific beliefs and limits them make familiar what appears strange in the absence of adequate taxonomic categories. Kuhn seems to be convinced that this is the best way to deal philosophically with the revolutionary changes intrinsic to the historical development of science, as it allows us to understand them simply as the transformation of the conceptual scheme constituting problems and solutions.

Even in the texts in which he elaborates what Friedman correctly called "Kuhn's late version of 'paradigm'" (Friedman 2002, 181), Kuhn's conceptual formulations, at the very least, allude to Wittgenstein's thinking, the most emblematic of which is a characterization of the confrontation between different conceptual schemes such as the clash between different forms of life. However, Kuhn's relativization of the *a priori* is against the perspective of Wittgenstein's thought. In *On Certainty*, Wittgenstein formulates a conception of certainty that if it cannot be understood as an alternative way to this way of dealing with revolutionary changes in the development of science, at least, it can be understood as questioning as to whether this is the only alternative. The question that leads Wittgenstein's thinking to this conception of certainty concerns how a certainty instituted in the course of human activities can become a constitutive condition of thought and language. Certainty is thought, in this context, as constitutive of a reference system that gives confidence to human practices in general, symbolic or non-symbolic. Insofar as this reference system determines understanding, the consideration of statements as true or false, as well as the validation of hypotheses, becomes a matter of understanding, which means that considering a given proposition as false can be proof of incomprehension.

Nothing can be more foreign to this conception of certainty than to understand the confidence that guarantees the stability essential to the development of human activities as "merely a constructed point to which some things approximate more, some less closely" (Wittgenstein 1969 § 56). This is because certainty is, above all, a matter of attitude. More than an interconnected network of beliefs, certainty is a form of action and thought. The confidence that allows the normal development of practices can materialize in propositions, but certainty does not consist of propositions assumed to be true no matter what. As the certainty that materializes in these propositions guides practices, such propositions may very well never be formulated or never be called into question. They are thus diverted from the "all inquiry on our part" route, remaining immune to questions and doubts. "They lie apart from the route traveled by inquiry" (Wittgenstein 1969 § 88), but not in isolation as if it constituted a kind of *a priori* propositions. After all, "we believe is not a single proposition, it is a whole system of propositions" (Wittgenstein 1969 § 88).

This conception of certainty implies that there is something out of the question as a condition for the possibility of research. For this reason, certainty has a normative character. Wittgenstein understands that it belongs "to the logic of our scientific investigations that certain things are indeed not doubted", but not in the sense that there is an absolute basis as a reference for scientific investigations (Wittgenstein 1969 § 342). The structure to which symbolic elaborations and human actions are submitted, for Wittgenstein, is not sublime, it is above all a construction made over time and established as a reference by the practices themselves. Beliefs form a system within which "all testing, all confirmation and disconfirmation of a hypothesis take place" (Wittgenstein 1969 § 105). It is not a question, for Wittgenstein, how much the facts can have priority or be independent of the beliefs, even because the system formed by them determines what we call in fact, having, on the other hand, an interaction between the facts and the system, so that if the facts were different, the system would not be the same.

Beliefs work in the same way as religious faith, because their determining character cannot be justified except by appealing to the practices determined by them. The confrontation between different reference systems may involve recourse to the presentation of reasons only to the extent determined by the similarities between them. In what they have to be irreducible, once the reasons are over, each one would remain "declare the other a fool and heretic." The recourse to reasons would then give way to persuasion (Wittgenstein 1969 §§ 611-12). Thus, the transition from one reference system to another has the character of "the conversion of a special kind", through which would go on to "to look at the world in a different way" (Wittgenstein 1969 § 92). The change brought about by adherence to a new reference system is practical in nature, as certainty is the norm of action and thought. The drastic nature of this change is due to the revision of what the practices guided by the old reference system established as out of the question. To that extent, the change implies a reorientation of practices. As the confidence that allows the development of human activities is a practical issue, the transition from one system to another does not experience the typical disorientation of the absence of a reference that guides the activities. Furthermore, for this reason too, Wittgenstein does not need to resort to a revisable *a priori*.

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Kuhn's commitment to a traditional semantic conception leads him to support an idea of scientific progress with characteristics typically linked to this type of semantics. Its late version of "paradigm", in which the most important feature is to replace the idea of revolution with that of reformulation, involves a softer incommensurability concept than that of *The Structure of Scientific Revolutions*. The new version of incommensurability has the advantage of allowing a comparison between rival theories and, consequently, a rational assessment that makes it possible to justifiably choose between one and the other, mainly because there is an area of intersection between different conceptual schemes. The difference between rival theories comes to be understood as taxonomic. Therefore, relative to the respective conceptual scheme to which each is subordinate. The consequence of this, in practical terms, is to understand that the difference between Copernican statements and Ptolemaic statements is not a matter of fact but of meaning. "The content of the Copernican statement 'planets travel around the sun', cannot be expressed in a statement that invokes the celestial taxonomy of the Ptolemaic statement 'planets travel around the earth'" (Kuhn 2000, 94). The term "planet" occurs as the same type in both statements, but "the two kinds overlap in membership without either's containing all the celestial bodies contained in the other" (Kuhn 2000, 94).

This conception of incommensurability evades the charge that the theoretical incomparability resulting from the old conception has the consequence that it is not possible to have reasons that justify the choice of the paradigm, because "what counts as a good reason is determined by the decision" (Shapere 1984, 47) to adopt the paradigm. From this point of view, relativism is inherent to incommensurability, since two paradigms "cannot be judged according to their ability to solve the same problems, or deal with the same facts, or meet the same standards", so that if scientific progress is characterized by the replacement of the paradigm, then "replacement is not cumulative, but is mere change" (Shapere 1984, 47). Since the paradigms are incommensurable and, therefore, incomparable, the evaluation of a paradigm can only take place from another paradigm, the denial of a paradigm is always the affirmation of another point of view. The criteria according to which a paradigm can be evaluated are internal to itself, "so that evaluation would inevitably question-beggingly favor the paradigm from which the evaluation was made" (Siegel 2001, 210). Kuhn will respond to this criticism by saying that he never denied the existence of reasons that justify the choice of paradigm, he only insisted "that such reasons constitute values to be used in making choices rather than rules of choice" (Kuhn 2000, 157). In any case, according to Shapere, if

the paradigm shift consists of a radical break, "a conversion experience", as Kuhn intends (Kuhn 1970, 151), then Kuhn "is thus led to deny, for example, that Einsteinian dynamics is an advance over Newtonian or Aristotelian dynamics", as there would be no continuity between them, to the point that it is impossible to speak of advancement (Shapere 1984, 47).

The change in the notion of incommensurability circumvents this imputation of relativism, but it does not seem to solve the questions that concern the rational character of scientific development. From Friedman's point of view, the problems of the old paradigm conception are reissued by the new conception. In particular, the question about what guides scientific research in the period of transition between paradigms. As there are no principles or rules in force in the period of transition to a new conceptual scheme, then the change must face a certain arbitrariness and cannot be guided rationally, which means that the paradigm shift would continue to be, for Kuhn, an experience of conversion (Friedman 2002, 182). This seems to be a natural consequence of the idea that scientific decisions should be subject to the orientation of the conceptual scheme. What is reasonable from the point of view of a conceptual scheme may well present itself as nonsense from the point of view of another. And that is what incommensurability means. But Kuhn deserves a fair assessment. The perspective of the late version of 'paradigm' allows the overlapping of conceptual schemes, which allows for the existence of "bridgeheads permitting a member of one to acquire the lexicon of the other" (Kuhn 2000, 104). In the absence of a wide-ranging overlap, says Kuhn, "would it be possible for the members of a single community to evaluate proposed new theories when their acceptance required lexical change" (Kuhn 2000, 104).

Kuhn's conceptual maneuver circumvents, at least partially, the imputation of irrationality to the process of a paradigm shift, but it has a price to pay for the commitment to a traditional semantic conception, as it consists of the requirement that there is something underlying the processes of differentiation and change. In the manner of the Kantian categories, the lexicon determines the possible experience *a priori*; unlike the Kantian categories, the lexicon can change. It is clear that Kuhn, despite his commitment to a kind of relativized *a priori*, does not leave behind the idea that scientific knowledge is constituted by ruptures, and is not mere accumulation. However, it is not so clear how much this commitment does not imply the abandonment of that conception of knowledge whose most important characteristics are related to the idea that the development of science depends on historical and social factors, as the emphasis on disruptions is greatly mitigated by means of Kuhn's understanding that there is something fixed and immutable in common with theories. Indeed, if, on the one hand, the truth emerges from scientific practices, on the other hand, this is at the expense of the conceptual scheme providing "ways of being-in-the-world" that do not play the game of truth. If the old paradigm conception gave rise to the idea that the changes that science goes through could not be rationally grounded, it is because that conception of paradigm assumed that scientific rationality was a construction made over time. If the paradigm is left behind and replaced by a new one, the standard of rationality related to it is also left behind, so to speak. As such, the scientific rationality related to the late version of 'paradigm', at least at first glance, is foreign to the idea that historical and social factors are preponderant in the development of science. Not because it is a problem that what may be essential in science is historically determined, in order to vary over time, but because the requirement that there is something fixed clashes with the idea of historical determination. It links the theory of science elaborated in this context to *The Structure of Scientific Revolutions* much more by the type of essentialist concern about the guarantees of individual scientific work's connection to a tradition than by way of understanding the reasons on which scientific changes are based.

Despite the sense that Kuhn gives to *a priori*, which he borrows from Kant, the linguistic turn that his theory of science goes through has features that allow approximations with Wittgenstein's philosophy, also in the sense of formulating questions that he would answer in another way. For Kuhn, the changes that science goes through comprise episodes

of history in which scientists are faced with alternative theories. The scientific decision, in these cases, is guided by values such as "accuracy, breadth of application, consistency, simplicity, and so on" (Kuhn 2000, 119). These values guide the change of beliefs. For Wittgenstein too, criteria such as simplicity are fundamental in solving conflicts between incompatible beliefs: "Remember that one is sometimes convinced of the correctness of a view by its simplicity or symmetry, i.e., these are what induce one to go over to this point of view. One then simply says something like: "That's how it must be" (Wittgenstein 1969 § 92). From Wittgenstein's viewpoint, fundamental beliefs that structure actions and thought compose a world-picture, "the inherited background against which I distinguish between true and false" (Wittgenstein 1969 § 92). The propositions that describe them cannot be considered neither true nor false, "their role is like that of rules of a game" (Wittgenstein 1969 § 95). In addition to considering that "the ways of being-in-the-world which a lexicon provides are not candidates for true/false", Kuhn (2000, 104) understands that "each lexicon makes possible a corresponding form of life within which the truth or falsity of propositions may be both claimed and rationally justified, but the justification of lexicons or of lexical change can only be pragmatic" (Kuhn 2000, 244). The linguistic framework that guides scientific practices, in this way, produces standards of correction and validity to which the problems and solutions of science are subordinate, which means that the evidence is always relative to the inherited background.

Kuhn and Wittgenstein, each in their own way, bring down both the idea of priority of the facts in relation to the beliefs whose evidence they would determine and the idea that the truth about the world, which emerges from scientific practices, would be an approximation of the truth and, as such, independent of mind and culture. According to Kuhn, both are fundamental to the philosophy of science. What separates Kuhn from Wittgenstein is that the latter leaves determination to the practices themselves, which takes shape in Kuhn's adherence to the revisable *a priori*. Having come to subscribe to the idea that divergent conceptual schemes have a transcendental reference in common, which enables them to understand each other from points of contact and perhaps is the main illustration of how much the development of Kuhn's theory of science undermines what can be considered his main legacy, as this brings his conclusions closer to those two fundamental ideas to the philosophy of science, which, according to him, constitute the science authority for a whole tradition of thought. Everything suggests that Kuhn's change, of course, has the purpose of saving the rationality of science from relativism that incommensurability and its effects have on scientific activity, even though he is not ready to admit it. As it is the old notion of paradigm that crushes those two fundamental ideas to the philosophy of science, so instead of consolidating the substitution of the scientific authority that they incorporate, the new route of Kuhn's theory of science offers reasons to subscribe to them.

The problems arising from the incommensurability idea, from the Wittgensteinian point of view, may well be solved without this expedient, solely based on the idea of a perspective without relativism. This does not mean that I am about to say that Kuhn's sin is the fact that he is not Wittgenstein. Just that thinking about the issues raised by Kuhn's work from the point of view of Wittgenstein's philosophy, as, for example, Condé does, opens a very fruitful way to think about alternatives to both Kuhn's essentialism and the *a priori* relativized of logical positivism. (See Condé 2020b, passim). The "grammar of science" that results from the exploration of this path, I believe, creates fertile conditions for thinking about a model of scientific rationality that removes the communicative rationality from the question, as thought by Friedman (See 2002, 184), without searching for inter-framework principles. For the same reason that it would refuse the search for common principles, it would be against the theory of science tested by Kuhn when drafting the lexicon theory, because such a grammar is marked by the abandonment of the dream with the transcendental, understanding any constitutive necessity as an instituted necessity. At worst, a Wittgensteinian-inspired theory of science shows how much the development of the

answers to questions formulated by Kuhn would benefit from grammatical exploration and how much Kuhn preferred another route, perhaps not so alien to *The Structure Scientific Revolutions* as it seems.

Conclusion

The linguistic turn in Kuhn's thought is more profound than a mere conceptual review. The changes are so profound that it seems as if the theory of science argued in *The Structure of Scientific Revolutions* has been abandoned. In my view, Kuhn's sketch of a new theory of science preserves some aspects of the early theory, but most importantly, the essentialist commitment which backs his thought about science is the same. In the first half of this paper, I undertake an analysis of Kuhn's work in comparison to Wittgenstein's thought in order to demonstrate that Kuhn uses the notion of family resemblance in an essentialist sense. Afterwards, in its second half, I compare Kuhn's new ideas and the view held by Wittgenstein in *On Certainty*, by means of which Kuhn's essentialist commitment appears even stronger. The point which is addressed by this comparison consists in illuminating the view according to which Kuhn's essentialist commitment both links his new ideas to *The Structure of Scientific Revolutions* and figure as the main reason why he abandons his former viewpoint. Therefore, the main issue in this paper is the evaluation of the extent of this change. As an alternative to Kuhn's theory of science, I think that a Wittgensteinian-inspired theory – as suggested by Condé (2014, 2020b) – can answer the problems of incommensurability without relativism, as far as it can tackle problems that arise from a traditional conception of semantics.

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