

## CHANGE AND ROUTINE IN SCIENTIFIC RESEARCH. THE POPPER-KUHN DEBATE

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**Abstract:** *A new debate has started in the philosophy of science between Karl Popper's followers and the representatives of the New philosophy of science. We may suppose that some of the commentaries made by the two mentioned philosophers were the result of their need to place themselves more explicitly in their debate, which was in full swing at that time. And if we agree that the core of the debate between the two lies in the way we understand the concept of "normal science", then it should be noted that Popper himself mentions in his "Preface" to the first edition of LSD that a scientist works in a research framework understood as an organized structure.*

**Keywords:** Karl Popper, Thomas Kuhn, normal science, extraordinary science, philosophy of science, the Duhem-Quine problem, rationality,

### **A short introduction: the stakes and challenges of the debate**

In the 1960s, after *The Structure of Scientific Revolutions* was published by Thomas Kuhn, a new debate has started in the philosophy of science between Karl Popper's followers and the representatives of the *New philosophy of science*. We may suppose that some of the commentaries made by the two mentioned philosophers were the result of their need to place themselves more explicitly in their debate, which was in full swing at that time<sup>1</sup>. Thomas Kuhn had mentioned certain points of agreement between them, including the fact that both emphasize "the intimate and inevitable entanglement of scientific observations with

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<sup>1</sup> See, as the best example, the book *Criticism and the Growth of Knowledge* edited by Lakatos and Musgrave, 1970.

scientific theories"<sup>2</sup>. And if we agree that the core of the debate between the two lies in the way we understand the concept of "normal science", then it should be noted that Popper himself mentions in his "Preface" to the first edition of *LSD* that a scientist works in a research framework understood as an organized structure<sup>3</sup>.

Therefore, it could be noted that the two philosophers were trying to identify an area of theoretical consensus and closeness. But which was the theoretical stake felt by each of the two and what effects resulted from this for their views on science and scientific development?

If we take into account the relationship between theory and observation as a component of a general framework accepted at least implicitly by Popper and Kuhn for a debate between realism, objectivism and convergence to truth, on the one hand, and relativism, subjectivism and under-determination, on the other hand, then it will be easier to understand their debate too.

Anyway, Gupta proposed a set of four theses in connection with which Popper and Kuhn clash:

1. the non-existence of conclusive verification;
2. the non-existence of conclusive falsification of specific theories if they cannot be tested in isolation from auxiliary assumptions (the Duhem-Quine problem);
3. the necessity of conventionalism about basic statements;
4. the theory-ladenness of all observation"<sup>4</sup>.

Taken together, all these implies that the observation cannot be a neutral arbitrator in theory choice.

Kuhn thinks that different paradigms transform observation and experience<sup>5</sup> and that "paradigms determine large areas of experience at the same time"<sup>6</sup>. Kuhn argues that "the commitments that govern normal science specify not only what sorts of entities the universe does contain, but also, by implication, those that it does not". From it follows that a discovery of something like that of oxygen does not just add one more item to the scientist's world, but also shift "the network of theory through which it

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<sup>2</sup> Kuhn, "Logic of Discovery or Psychology of Research?", 1970, p. 2.

<sup>3</sup> See Popper, 2002, p. XV, and his comments in "Normal Science and hid Dangers", 1970, p. 51.

<sup>4</sup> See Gupta, 1993, p. 322.

<sup>5</sup> Kuhn, *The Structure...*, 1970, p. 7, also pp. 111-112.

<sup>6</sup> *Ibidem*, p. 129.

deals with the world. Scientific fact and theory are not categorically separable, except perhaps within a single tradition of normal- scientific practice. That is why an unexpected discovery is not simply factual in its import and why the scientist's world is qualitatively transformed as well quantitatively enriched by fundamental novelties of either fact or theory"<sup>7</sup> The transformations are irreversible and looks like a switch in visual *gestalt*. I think that this approach to scientific activity and change in science is the starting point of the debate between Popper and Kuhn.

### **Two meanings for “normal science”? *Popper's view on Kuhn's theory***

It is generally accepted by consensus that the central point of the controversy between Popper and Kuhn was the problem of the rationality of normal science, the latter being a concept introduced by Kuhn and criticized by Popper and his followers. Normal science is understood as a research based and guided by a paradigm and aimed at solving puzzle-like problems, a research based on a fine articulation of the paradigm within it, without in any way questioning the theoretical foundations of the field. Normal science carries out the exploratory promises of the paradigm in several ways, “by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm 's predictions, and by further articulation of the paradigm itself”<sup>8</sup>. As for the puzzle problems, which appear not only in sciences, they are characterized by the fact that the existence of a solution is guaranteed and that finding the solution requires creative imagination and persistent effort<sup>9</sup>.

The critical reactions of those on Popper's side were different in intensity, between a radical criticism that leads to rejection of Kuhn's view on normal science, and a critical reception, sometimes moderate, the result of

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<sup>7</sup> Kuhn, *The Structure...*, p. 7. Kuhn develop this idea in chapter X of *SSR* where he points out that “Paradigm changes do cause scientists to see the world of their research-engagement differently. (...) After a revolution scientists are responding to a different world”. (Kuhn, *The Structure...*, p. 111)

<sup>8</sup> Kuhn, *The Structure...*, 1970, p. 24.

<sup>9</sup> Kuhn picks up on this common use of the term “puzzle”: “Puzzles are, in the entirely standard meaning here employed, that special category of problems that can serve to test ingenuity or skill in solution”. (Kuhn, *The Structure...*, 1970, p. 36.)

which would be an agreement or proposal for a revision, possibly from both sides. Thus, John Watkins expresses the most virulent point of view in his paper “Against ‘Normal Science’”. Foreshadowing the interpretation according to which there was a communication breakdown between the followers of Popper and Kuhn, Watkins claims that the term “paradigm” is just a substitute for “scientific theory”<sup>10</sup>, and that, therefore, what Kuhn says about the paradigm must be applicable to scientific theory. Or, if normal science does not critically investigate the foundations, this means from the perspective of Popper’s theory about science understood as a rational activity, *i. e.* critical, that the practice of normal science does not fall within the norms of rationality and does not serve the progress of knowledge. Rather, only what Kuhn calls “extraordinary research” would correspond to authentic science defined as the daring activity of formulating hypotheses and deductive testing of theories. Finally, Watkins thesis is that “a new paradigm never could emerge from Normal Science as characterized by Kuhn”.<sup>11</sup>

Popper himself adopts a moderate position, even looking for convergences or agreements, but under the recognition of some irreconcilable differences emerged from the concept of normal science. First, Popper admits that he neglected those aspects of research that fall under the concept of normal science and that Kuhn really opened his eyes to the important distinction between “normal science” and “extraordinary science”, Popper himself being devoted to the latter way of doing science as a bold and heroic enterprise. But the way Popper describes normal science shows that he sees it differently than Kuhn, namely, from the perspective of his own privileged concept of “heroic science”. Popper thinks that normal science is the non-revolutionary or not-too-critical professional activity carried out by those “who accepts the ruling dogma of the day; who does not wish to challenge it; and who accepts a new revolutionary theory only if almost everybody else is ready to accept it – if it becomes fashionable by a kind of bandwagon effect. To resist a new fashion needs perhaps as much courage as was needed to bring it about”.<sup>12</sup> Normal science understood in this way is for Popper a phenomenon that deserves to be studied by the history of science, even though he personally dislikes like it, even considering it a “danger to science”.<sup>13</sup>

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<sup>10</sup> Watkins, 1970, p. 26,

<sup>11</sup> *Ibidem*, p. 34.

<sup>12</sup> Popper, “Normal Science...”, 1970, p. 52.

<sup>13</sup> *Idem*.

Moreover, Popper suggests that the researcher who practices normal science “is a person one ought to be sorry for”<sup>14</sup> and that it is a mistake to think that the greatest scientists were “normal” in the sense prescribed by Kuhn. Popper describes normal scientific activities in an opposite way to Kuhn. In his view, a scientist is “normal” if he has been taught “that all teaching on the University level (and if possible below) should be training and encouragement in critical thinking”<sup>15</sup>, while the “normal scientist” described by Kuhn “has been taught in a dogmatic spirit: he is a victim of indoctrination. He has learned a technique which can be applied without asking for the reason why (especially in quantum mechanics). As a consequence, he has become what may be called an *applied scientist*, in contradistinction to what I should call a *pure scientist*”<sup>16</sup>. Therefore, all the kuhnian “normal scientists” have to do is to solve “puzzles”. Popper thinks that the choice of this term by Kuhn seems to suggest that he wants to stress that the “normal scientist” isn’t trained to solve fundamental problems but just routine problems, understood as problems of applying what one has learned based on a dominant paradigm (or theory). Popper claims that if in his view the success in science means to test and to reject hypothetical theories, Kuhn’s view is very different: “The success of the ‘normal’ scientists consists, entirely, in showing that the ruling theory can be properly and satisfactorily applied in order to reach a solution of the puzzle in question”<sup>17</sup>. Popper thinks that this is a downgrading use of the term “puzzle”, similar to Wittgenstein’s thesis that there are no genuine problems in philosophy, but only puzzles, namely, pseudo-problems related with the improper use of language. Popper concludes that, anyway, “the use of the term ‘puzzle’ instead of ‘problem’ is certainly indicative of a wish to show that the problems so described are not very serious or very deep”<sup>18</sup>.

This description of normal science reminds Popper of a conversation with Philip Frank in the 1930s about the uncritical approach used by Engineering students who are satisfied with little theoretical knowledge and who want just to “know the facts” and how to do applied science with devotion, “without heart-searching”<sup>19</sup>. Popper remarks that the same

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<sup>14</sup> *Idem.*

<sup>15</sup> Popper, “Normal Science...”, 1970, p. 53.

<sup>16</sup> *Idem.*

<sup>17</sup> *Idem.*

<sup>18</sup> Popper, “Normal Science...”, 1970, p. 53, footnote 1.

<sup>19</sup> Popper, “Normal Science...”, 1970, p. 53.

attitude is also common among the people trained directly in pure science and its symptomatic presence consists in refusing to discuss theories that aren't "generally accepted" or are considered problematic. His diagnosis is put in the same worrying way, considering this type of normality a danger not only for science but also for our civilization<sup>20</sup>.

I think that here, in the definition of what is normality in science, we have the main divergence between the two philosophers with effects on the understanding of the relation between pure and applied science, of scientific practice and changes in science. Popper thinks that "Kuhn is mistaken when he suggests that what he called 'normal' science is normal"<sup>21</sup>. This isn't just a quarrel about the term, but a difference about the description of science. Popper suggests that if we take a look at the history of science then we will find that few or none of the scientists were 'normal' scientists in Kuhn's sense.

Therefore, we may conclude that Popper disagrees with Kuhn both about the imagine of science derived from the historical facts and about what is characteristic or normal for science. The example given by Popper is that of Charles Darwin who became a "reluctant revolutionary" (an expression used by Pearce William as description for Max Planck), only after the publication of *The Origin of Species*. There was nothing like a "conscious revolutionary attitude" in *The Voyage of the Beagle*, but it is already full of new and fundamental problems, questions, observations, and of ingenious conjectures which challenged Darwin to think comparatively about them and to find a solution. Even in the case of descriptive botany, a less revolutionary science because its descriptive approach, the botanist have to solve all the time genuine problems regarding different botanical items like distribution, characteristic locations, species of sub-species differentiation, symbiosis, characteristic enemies and diseases, resistant and more or less fertile strains, and these genuine problems are both theoretical and experimental ones. If we accept this approach proposed by Popper, then it becomes obvious that the history of science does not support the idea of "normal science" as it was described by Kuhn. The history of science pictured by Kuhn as a sequence of dominant paradigms that generate "normal science" and revolutionary periods of "extraordinary science" clashes with the historical facts.

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<sup>20</sup> See Popper, "Normal Science...", 1970, p. 53. Another great danger mentioned by Popper is the increase of specialization.

<sup>21</sup> Popper, "Normal Science...", 1970, p. 53.

The opposition between the two approaches of normality in science is explained by Popper by the contrast between a so called *logic of methodological universalism* and a so called *logic of historical relativism*. Popper believes that “science is essentially critical; that it consists of bold conjectures, controlled by criticism, and that it may, therefore, be described as revolutionary”<sup>22</sup> On the contrary, Popper believes, Kuhn’s theory about science is based on the logical thesis of relativism: “the rationality of science presupposes the acceptance of a common framework. He suggests that rationality depends upon something like a common language and a common set of assumptions. He suggests that rational discussion, and rational criticism, is only possible if we have agreed on fundamentals”<sup>23</sup>

But both methodological universalism and historical relativism presuppose logically a certain form of dogmatism, but a different one in each case. From the perspective of methodological universalism, we will subject a theory to criticism in a systematic way and in stages, step by step, first giving it all good wishes because “if we give in to criticism too easily, we shall never find out where the real power of our theories lies”<sup>24</sup>. Kuhn prefers and supports another kind of dogmatism whose main characteristic is given by the fact that the scientist believes in the dominant paradigm and is ready to do all is possible to save it. In Popper’s view, Kuhn is a philosopher placed on the other side: “He believes in the domination of a ruling dogma over considerable periods; and he does not believe that the method of science is, normally, that of bold conjectures and criticism”<sup>25</sup>

### ***Kuhn’s comment to Popper’s criticism***

In “Logic of Discovery or Psychology of Research?”<sup>26</sup>, his contribution to the volume *Criticism and the Growth of Knowledge*, Thomas Kuhn try to offer a “disciplined comparison”, and not a sanguine confrontation, between his view of scientific development and that proposed by Popper, so that to

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<sup>22</sup> Popper, “Normal Science...”, 1970, p. 55.

<sup>23</sup> Popper, “Normal Science...”, 1970, p. 56.

<sup>24</sup> Popper, “Normal Science...”, 1970, p. 55.

<sup>25</sup> *Idem*.

<sup>26</sup> See Kuhn, “Logic of Discovery...”, 1970, pp. 1-23. It was also published in Schilpp’s volume devoted to Popper’s philosophy, and Popper reply to it. See Popper, “Replies to My Critics”, in (ed.) P. A. Schilpp, *The Philosophy of Karl Popper*, Open Court, La Salle, Illinois, 1974

produce a “peculiar enlightenment”<sup>27</sup>. Kuhn confesses that he has discovered some puzzling characteristics of the relation between his view and Popper’s approach even before to publish his *The Structure of Scientific Revolutions*.

Surprisingly, Kuhn notes that each time throughout their theoretical developments they dealt with the same problems, and that their views were “very near identical”<sup>28</sup>. Kuhn advances an unexpected perspective that erases the distinctions that Popper himself and his followers have highlighted. Kuhn claims that they are both “concerned with the dynamic process by which scientific knowledge is acquired rather than with the logical structure of the product of scientific research”<sup>29</sup>. Kuhn’s statement surprises and proves that the two have different readings of each other. Popper, on the contrary, emphasizes from the beginning, even in *The Logic of Scientific Discovery*, in the paragraph “Elimination of Psychologism”, that he is interested in logical problems, not factual ones, that he isn’t concerned with questions of fact, but with questions of justification or validity<sup>30</sup>, a distinction that then took the form of the one between the context of discovery and the context of justification. Furthermore, Kuhn admits that both have undertaken actual research to ascertain the authenticity of scientific research and “both of us turn often to history”<sup>31</sup>. From all these data, the two philosophers would have drawn, in Kuhn’s view, the same conclusions: “Both of us reject the view that science progresses by accretion; both emphasize instead the revolutionary process by which an older theory is rejected and replaced by an incompatible new one; and both deeply underscore the role played in this process by the older theory’s occasional failure to meet challenges posed by logic experiment, or observation”<sup>32</sup>. This description is, indeed, a very popperian one, especially since Kuhn does not even use the concept of paradigm, leaving it to be understood that an equation between “theory” and “paradigm” would be unproblematic. However, aren’t the reasons why Kuhn prefers the term paradigm a symptom of a difference in vision? Kuhn emphasizes in *SSR* and will do so in “Logic of Discovery...” the differences

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<sup>27</sup> Kuhn, “Logic of Discovery...”, 1970, p. 1.

<sup>28</sup> *Idem*.

<sup>29</sup> *Idem*.

<sup>30</sup> Popper, *Logic of Scientific Discovery*, 2002, p. 7.

<sup>31</sup> Kuhn, “Logic of Discovery...”, 1970, p. 1.

<sup>32</sup> Kuhn, “Logic of Discovery...”, 1970, pp. 1-2.



between theory and paradigm as cognitive entities, including from the perspective of the relationship with the framework (or the context) and with the aspects regarding the problem of theory ladenness.

Thus, Kuhn believes that him and Popper are on the same side against classical positivism, such as, among other things, revising the rigid distinction between observations and theory. Kuhn underlines some major family resemblances: "We both emphasize (...) the intimate and inevitable entanglement of scientific observation with scientific theory; we are correspondingly sceptical of efforts to produce any neutral observation language; and we both insist that scientists may properly aim to invent theories that *explain* observed phenomena and that do so in terms of *real* objects, whatever the latter phrase may mean"<sup>33</sup>

Kuhn also highlights two points of divergence that were noticed by his popperian readers, but he considers them secondary. These are the emphasis on the importance of deep commitment to tradition and the discontent with the implications of the term 'falsification'<sup>34</sup>. As far as the influence of tradition is concerned, Kuhn believes that it is only a difference in accent, not in content. Popper recognized the importance of tradition and Kuhn quotes fragments in which Popper discusses its role before him<sup>35</sup> As regards the problem of falsification, here we really have a matter of divergence regarding the concept of scientific change, the logic of scientific change, the entities that are changed and their relationship with experience.

Kuhn offers a suggestive picture of the similarities and differences between him and Popper. They start from the same data, but, because they use different glasses, they arrive at different configurations: "Though the lines are the same, the figures which emerge from them are not. That is why I call what separates us a gestalt switch rather than a disagreement"<sup>36</sup>. Kuhn believes that one of the most fundamental issues on which Popper and him agree is that an analysis of the development of scientific knowledge "must take account of the way science has actually been practiced"<sup>37</sup>.

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<sup>33</sup> *Ibidem*, p. 2.

<sup>34</sup> *Idem*.

<sup>35</sup> See Kuhn, "Logic of Discovery...", 1970, p. 2, footnote 2. For example, Popper writes: "I do not think that we could ever free ourselves entirely from the bonds of tradition. The so-called freeing is really only a change from one tradition to another." (Popper, *Conjectures and Refutations*, 1968, p. 122).

<sup>36</sup> Kuhn, "Logic of Discovery...", 1970, p. 3.

<sup>37</sup> *Ibidem*, p. 4.

The standard statement of Popper's view is at the very beginning of *The Logic of Scientific Research*: "A scientist, whether theorist or experimenter, puts forward statements or systems of statements, and test them step by step. In the field of the empirical science, more particularly, he constructs hypotheses, or systems of theories, and test them against experience by observation and experiment"<sup>38</sup> Noting the ambiguity between "statements" and "theories" as goals of testing, Kuhn agrees that we test statements or hypothesis, but we do this to solve puzzles and this means to do "normal science". Then, the scientist connects the result of the test with the corpus of accepted scientific knowledge. But, Kuhn admits, Popper has in mind another kind of tests because he thinks that science grows not by accretion, but by the replacement of accepted theories with better ones. Popper thinks that there are two main ways in which the growth of science may be explained, either by the accumulation of knowledge, or by criticism. The last means that science grows revolutionary by "a method which destroys, changes, and alters, the whole thing including its most important instrument, the language in which our myths and theories are formulated"<sup>39</sup>. Thus, our epistemological task and the end of any research aren't the construction of definitive and closed deductive systems, but to examine them critically, to try to refute them by new and more severe tests. But these tests are very rare. Therefore, Kuhn claims, Popper "has characterized the entire scientific enterprise in terms that apply only to its occasional revolutionary parts"<sup>40</sup>. Kuhn asserts that Popper ignore normal science and the fact that science is normally practiced most of the time and that those severe tests are only rare occasions, when the scientist must choose between competing theories.

This normal practice is also the demarcation between science and other enterprises. Popper describes the so called critical tradition in philosophy, but this critical approach happens in science only when we have to test competing theories. In normal science, when we aren't able to solve puzzles, "only the practitioner is blamed, not his tools"<sup>41</sup>. The difference between science and other enterprises and that in science we solve puzzles. For example, Kuhn suggests, though astrologers are able to make testable predictions and they also recognize that these predictions

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<sup>38</sup> Popper, *The Logic of ...*, 2002, p. 3.

<sup>39</sup> Popper, *Conjectures...*, 1968, p. 129.

<sup>40</sup> Kuhn, "Logic of Discovery...", 1970, p. 6.

<sup>41</sup> *Ibidem*, p. 7.

sometimes failed, they cannot engage in the sort of activities that characterize all recognized sciences. Kuhn concludes: "With or without tests, a puzzle-solving tradition can prepare the way for its own displacement. To rely on testing as the mark of a science is to miss what scientists mostly do and, with it, the most characteristic feature of their enterprise"<sup>42</sup>.

### **Popper's reply to Kuhn's commentaries**

In his "Replies to My Critics", section "Kuhn on the Normality of Normal Science", Popper answers to Kuhn's commentary. He agrees with Kuhn that their views of science have something in common, but he warns us that Kuhn was somewhat influenced by the so-called "Popper legend", a false image of Popper as a positivist philosopher<sup>43</sup>.

First, regarding the differences between them, Popper mentions again that Kuhn has discovered the so-called "normal science" and "normal scientist", a phenomenon ignored by him. Popper admits this phenomenon, but he believes that it isn't a scientific virtue, but, on the contrary, a danger for science. Popper is also aware of the fact that his interpretation could be considered inadequate in relation to what Kuhn understands by "normal science": "one might say that I look at this phenomenon through spectacles very unlike those used by Kuhn"<sup>44</sup>. As a consequence I will try a discursive placement in a neutral area, between the legend and the improper spectacles, that will allow a non-partisan evaluation.

It is obvious that Popper believes that the periodization of the history of science in two different periods, one normal, another extraordinary, is mistaken. Popper uses the key word "routine" in order to characterize the two periods: "(1) *normal periods*, in which there is an *established 'routine' of puzzle solving*; this routine unites the community of normal scientists, and their activities constitute 'normal science'; (2) *extraordinary periods*, consisting of a crisis followed by a revolution, which consists in the overthrow of the old routine and in the establishment of a new one"<sup>45</sup>.

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<sup>42</sup> *Ibidem*, p. 10.

<sup>43</sup> See for details, Popper, "Replies...", 1974, pp. 963-965.

<sup>44</sup> Popper, "Replies...", 1974, p. 1145.

<sup>45</sup> *Idem*.

Popper explains that he admits the existence of “routines” in science and the existence of “normal science”, but he thinks that the idea of a routine isn’t characteristic to science and that the so called “normal science” isn’t normal, but uncharacteristic. Popper explains Kuhn’s preferences for this picture of science as an illegitimate generalization of his own experiences regarding the scientific activity and the history of science. Popper thinks that the phenomenon of a “routine” in science has become prominent only recently, as a consequence of the so called “mass production of scientists”. Kuhn projected “a comparatively recent phenomenon which he has personally experienced, not only upon earlier periods but upon the whole long history of science”<sup>46</sup> Popper clearly highlights this difference between them: “I believe that routine played very little role in science until this century, or more precisely, until the First World War. Kuhn believes that routine characterize the essence of science”<sup>47</sup>.

Popper adds another argument regarding the difference between him and Kuhn starting from the evolutionary theory. “Normal science” or “routine” is for Popper nothing but the way in which animals “know”, namely, they are able to adapt themselves to their environment, while the humans, creators of the descriptive and argumentative language, replace *routine* by *critical approach*, and science is “the most advanced application of this critical approach to the growth of human knowledge”<sup>48</sup>. Thus understood, routine may supersede science and it is a danger for science if Kuhn’s criterion of science, as “a community of workers held together by a *routine*”, become a practice. “If so, this will be the end of science as I see it”<sup>49</sup>. Popper points out that although Kuhn considers that astrology is not a science, it would fulfil Kuhn’s criteria since there is a community of practitioners of astrology who share a routine, and who are deeply engaged in puzzle solving. Popper thinks that this is a consequence of the sociological approach proposed by Kuhn. But to recognize astrology as science is only a minor disaster, because “the major disaster would be the replacement of a rational criterion of science by a sociological one”<sup>50</sup>.

Popper asserts that if we consider science in an evolutionary context, then science has to be seen as “the conscious and critical form of an

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<sup>46</sup> Popper, “Replies...”, 1974, p. 1146.

<sup>47</sup> *Idem.*

<sup>48</sup> *Idem.*

<sup>49</sup> *Idem.*

<sup>50</sup> Popper, “Replies...”, 1974, p. 1147.

adaptive method of trial and error”<sup>51</sup>. Therefore, from amoeba to Einstein we learn from our own mistakes. The explanatory principle that “science is revolution in permanence” doesn’t mean that there aren’t periods of stagnation or that science is revolutionary all the time in the sense in which we speak about a Copernican, a Galilean, a Newtonian, or an Einsteinian revolution. Popper explains that for him the catchphrase “revolution in permanence” means that even a minor discovery is revolutionary and that many engineers and technologists are revolutionaries just because usually “established beliefs (or routines) are overthrown every day. Sometimes these are major discoveries: more often they are minor discoveries”<sup>52</sup>. For example, the heating engineer who install a central heating system solve some practical problems. He works like an applied scientist and he made some minor discoveries by critical thinking, by critical rejection of erroneous solutions. Kuhn would say that this engineer solves just puzzles and that he is like a normal scientist. But Popper adds that “when he works by trial and the elimination of error, and when he eliminates the error by a *critical* survey of tentative solutions, then he does not work in this routine manner; which for me makes him a scientist”<sup>53</sup>.

An interesting comment regarding the concept of ‘normal science’ and the comparison between Popper’s and Kuhn’s theories is proposed by J. O. Wisdom. Like Popper and Watkins, Wisdom equates the paradigm in the Kuhnian sense with the theory that dominates and guides research in a certain field. Thus, normal science, as puzzle solving, is nothing but a research based on a paradigm (theory), without in any way questioning the fundamental theoretical aspects. Normal science is a detailed investigation of the facts, a perfecting of the calculations, a more precise derivation of the predictions, that is, a so-called fine articulation of the theory by the day-to-day work of the scientist. On the contrary, Popper believes that science is “revolution in permanence”, that the scientist is always concerned to refute hypothesis by testing them. Wisdom describes the difference between the two philosophers in these terms: “On the view accorded to Popper, every theory is all the time in all circumstances being tested, no matter what applications are going on. Thus the daily work of science, if not revolutionary, aims at small-scale rebellion (which might blossom into a revolution at any moment). According to Kuhn, the situation is quite

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<sup>51</sup> *Idem.*

<sup>52</sup> *Idem.*

<sup>53</sup> *Idem.*

different: you are not attempting to test a theory at all; in certain circumstances you are concerned simply to exploit it, use it, extend it, apply it. The Popperian might, to this account, reply, 'Of course, you may be extending the theory; but, if the extension goes wrong, this is taken as a test of the theory'<sup>54</sup>. Wisdom thinks that there is no clash between the two views if we interpret them correctly. Kuhn's distinction between "normal science" and "extraordinary research" is equivalent to the distinction between the articulation and application of theories and respectively, the change and replacement of theories. If it is so, then the incompatibilities disappear and, more than that, appreciates Wisdom, "Kuhn's theory and (when correctly interpreted) Popper's (when developed) are identical"<sup>55</sup>. Wisdom also remark the similarity between his view and Lakatos attempt to reconcile the two in his theory of scientific research programmes.

### **Popper's last attack: The myth of the framework**

Apparently Popper and Kuhn admit that they agree in some respects, the main point of convergence or similarity being the recognition of the role of the context or framework or problem-situation in any scientific research. Popper described this standpoint even from the "Preface to the first edition, 1934" of *The Logic of Scientific Discovery*, where after two preparatory quotes from Schlick and Kant he states that "A scientist engaged in a piece of research, say in physics, can attack his problem straight away. He can go at once to the heart of the matter to the heart, that is, of an organized structure. For a structure of scientific doctrines is already in existence; and with it a generally accepted problem-situation. This is why he may leave it to others to fit his contribution into the framework of scientific knowledge"<sup>56</sup>. Afterwards, in his commentary on Kuhn's *Structure*, Popper highlights that his point of view that "there is an edifice, an organized structure of science which provides the scientists with a generally accepted problem-situation into which his own work can be fitted" seems to be "very similar" to Kuhn's main point that the so-called normal science "presupposes an organized structure of assumptions, or a theory, or a research programme, needed by the community of scientists in order to discuss their work rationally"<sup>57</sup>.

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<sup>54</sup> Wisdom, "The nature of 'normal science'", 1974, p.825.

<sup>55</sup> *Idem*.

<sup>56</sup> Popper, *The Logic of...*, 2002, p. xv.

<sup>57</sup> Popper, "Normal Science...", 1970, p. 51.

Also, we must not overlook the fact that Popper makes a meta-theoretical consideration regarding his debate with Kuhn, which proves again that he is strongly aware of the role of previous or background knowledge in any theoretical interaction, including a critical debate. Thus, each of them judges the other from the perspective of their own expectations, which is nothing but a confirmation of the thesis "that we approach everything in the light of a preconceived theory"<sup>58</sup>.

But this convergence is only apparent, because the two philosophers understand differently the fixation of a scientist in a theoretical context. The differences between the two are related with their different insights regarding the 'normal science' or 'normal scientists'. In fact, from Popper's perspective, the way Kuhn understands normal science involves the elimination of critical discussion in the case of the foundations of the theory or the research tradition, which leads to a relativistic position, characterized by Popper as an acceptance of the "myth of the framework".

Popper claims that this thesis is mistaken and rejects it. He admits that it is easier to discuss puzzles within a common accepted framework and then to delete them into a new framework than to discuss fundamentals, but the thesis that the framework cannot be critically discussed can be critically discussed and does not stand up to criticism. Later, in his *The Myth of the Framework*, Popper provides a concise formulation of this thesis: "A rational and fruitful discussion is impossible unless the participants share a common framework of basic assumptions or, at least, unless they have agreed on such framework for the purpose of the discussion"<sup>59</sup>.

Popper explains why he isn't a relativist. First of all, he believes in 'absolute' or 'objective' truth in Tarski's sense. Secondly, Popper explains his different view from Kuhn about the idea of a framework: "I do admit that at any moment we are prisoners caught in the framework of our theories; our expectations; our past experiences; our language. But we are prisoners in a Pickwickian sense: if we try, we can break out of our framework, but it will be a better and roomier one; and we can at any moment break out of it again"<sup>60</sup>.

Therefore, according to Popper, it follows that the central point of his view on the debate around the myth of the framework is that "a critical

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<sup>58</sup> *Ibidem*, p. 52.

<sup>59</sup> Popper, *The Myth...*, 1994, pp. 34-35.

<sup>60</sup> Popper, "Normal Science...", 1970, p. 56.

discussion and a comparison of the various frameworks is always possible<sup>61</sup>. Although it is difficult, it is possible to learn totally different languages, like English, Hopi or Chinese. Oh course, it is difficult to have a discussion between people from different frameworks, but it can be done in the end if those who belongs to different frameworks adopt rational strategies.

Taking Kuhn's suggestion of how a new paradigm is adopted through conversion by a researcher, Popper supposes that even if we admit that "an intellectual revolution often looks like a religious conversion (...), this does not mean that we cannot evaluate, critically and rationally, our inner views, in the light of new ones"<sup>62</sup>. Thus, it is false to say that the transition from Newton's theory of gravity to Einstein's is an irrational leap, and that the two aren't rationally comparable. On the contrary, there are a lot of points of comparison between the two. Generally speaking, in science, as a very distinct human activity from theology, a critical comparison of the competing theories or of the competing frameworks, is always possible. The denial of this possibility starting from the idea that the framework in which the scientist is caught is like an "iron cage" is a mistake: "In science (and only in science) can we say that we have made genuine progress: that we know more than we did before"<sup>63</sup>.

Therefore, in Popper's view, the debate between him and Kuhn is a fundamentally logical one. Popper thinks that Kuhn proposed a logical thesis, even if a wrong one, described by Popper as such: "the scientist is *logically forced* to accept a framework, since no rational discussion is possible between frameworks"<sup>64</sup>. Popper adopts an anti-psychological view because he thinks that 'scientific knowledge' may be regarded as subjectless<sup>65</sup>. Scientific knowledge may be regarded "as a system of theories on which we work as do masons on a cathedral. The aim is to find theories which, in the light of critical discussion, get nearer to the truth. Thus the aim is the increase of the truth-content of our theories"<sup>66</sup>.

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<sup>61</sup> *Idem.*

<sup>62</sup> Popper, "Normal Science...", 1970, p. 57.

<sup>63</sup> *Idem.*

<sup>64</sup> *Idem.*

<sup>65</sup> See Popper, "Epistemology without a Knowing Subject", 1968.

<sup>66</sup> Popper, "Normal Science...", 1970, p. 57.



Popper takes up his argument based on the epistemology without a knowing subject in his "Replies to My Critics"<sup>67</sup>, where he accepts the difference drawn by Kuhn between the logic of discovery and the psychology of research and he concludes that science as a theoretical system is independent from the subjectivity of: "Science is part of world 3., and not of world 2; or more precisely, the psychological world 2 of the scientist is almost completely dependent upon the man-made world 3, the world of scientific theories and problems. The world 3 science can be investigated only logically. Thus any good psychology of research will have to depend on, and be guided by, the logic of discovery"<sup>68</sup>.

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<sup>67</sup> See Popper, "Replies to My Critics", 1974, section "39. Kuhn on the Normativity of Normal Science".

<sup>68</sup> Popper, "Replies to My Critics", 1974, p. 1148.

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