THEORY LADENNESS AND SCIENTIFIC CHANGE. THE IMPLICATIONS OF POPPER–KUHN DEBATE

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Abstract. The thesis of the theoretical ladenness of observation is equally accepted by Popper and Kuhn, but the two argue different consequences. If Popper believes that the theoretical ladenness does not prevent the application of the falsificationist methodology, Kuhn, although he also accepts a similar mechanism of scientific change based on reduction ad absurdum, proposes a semantic approach and claims that the impossibility of a neutral language inevitably leads to incommensurability. My goal in this paper is to follow this argumentative path and to better fix the epistemological positions of the two regarding the possibility of comparing the theories with each other and with nature.

Keywords: Karl Popper, Thomas Kuhn, scientific change, theory ladenness, falsifiability, incommensurability.

A SHORT INTRODUCTION: RESURRECTING AN OLD SOLVED PROBLEM

In his *Logic of Scientific Discovery*, at the end of the chapter about the empirical basis of science, after supporting the thesis the thesis about the relativity of basic statements, Popper concludes that "the empirical basis of objective science has thus nothing 'absolute' about it"[1]. Later, in his "Addendum" to the German edition from 1968 and English edition from 1972, Popper made some additions by which he emphasized that the basic empirical (observational) statements are not merely fallible, but also theory laden. Moreover, Popper mentions that his epistemological project is to replace *perception* as "basis" by *critical testing*. He warned that his term "basis" has ironical overtones, suggesting that the basis is not firm, and that any observational statement, like all languages, "are impregnated

[1] Karl R. Popper, *The Logic of Scientific Discovery*, Routledge, London and New York, 2002, pp. 93–94.

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with theories"[2]. Even a so-called phenomenal language, let's suppose, one about colours, is impregnated by theories about space, time and colours. Therefore, although Popper had identified the problem of the theoretical loading of observation since the first edition of the *Logic of Scientific Discovery*, he felt the need to return to it in the late additions to the 1968 and 1972 editions. Assuming that these were not simple gratuities, but had a reason, we may wonder which one it was?

We may suppose that some of these last short additions were the result of Popper's need to place himself more explicitly in his debate with Thomas Kuhn, which was in full swing at that time. Thomas Kuhn had already mentioned certain points of agreement between them, including the fact that both emphasize "the intimate and inevitable entanglement of scientific observations with scientific theories"[3]. Although we agree that the core of the debate between the two lies in the way we understand the concept of "normal science", the debate also updated other collateral issues, such as that of the theoretical leanness and its relation with scientific change.

It is obvious that if we take into account the relation between theory and observation and the way in which theories are subjected to empirical testing, then we will be able to distinguish between two meanings of the theory-ladenness, one general, which Popper has in mind[4] and another narrow and radical, supported by Kuhn. Generally speaking, theory-ladenness is the view that in the empirical testing process observation cannot be free from prejudices or unbiased or "pure" because any judgment or statement or sentence is contaminated, affected or loaded by the theoretical presuppositions, expectations and beliefs of the observer. This line of reasoning was reloaded by N. R. Hanson who pointed out that what we know influences what we observe and remarked that "there is more to seeing than meets the eyeball"[5]. The radical form of theory-ladenness was developed as incommensurability by Kuhn (and Feyerabend) who argues that two different paradigms cannot be compared because the is no neutral language and the meanings of observational terms from different paradigms are different even when their uses are the same.

The problem of theory-ladenness may be conceived as a component of a general framework accepted at least implicitly by Popper and Kuhn for a debate

[2] *Ibidem*, p. 94.

[3] Thomas S. Kuhn, "Logic of Discovery or Psychology of Research?", in (eds.) I. Lakatos, A. Musgrave, *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge, 1970, p. 2.

[4] For example, Popper mentions in his "Preface" to the first edition of *LSD* that a scientist works in a research framework understood as an organized structure. (See Popper, *The Logic of Scientific Discovery*, p. XV, and his comments in "Normal Science and hid Dangers", in (eds.) I. Lakatos, A. Musgrave, *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge, 1970, p. 51).

[5] N. R. Hanson, *Patterns of Discovery*, Cambridge University Press, Cambridge, 1958, p. 7.

between realism, objectivism and convergence to truth, on the one hand, and relativism, subjectivism and under-determination, on the other hand.

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"[6].

Taken together, all these implies that the observation cannot be a neutral arbitrator in theory choice. In this research I consider the fourth point of the confrontation, the references to the others being only collateral.

THEORY LADENNESS AS A KUHNNIAN OBJECTION AGAINST POPPERIAN IDEA OF FALSIFIABILITY

As we know, Popper rejected inductive logic and he had developed the hypothetical deductive model of the theory and proposed a falsificationist methodology according to which theories are hypotheses that are tested through experience and, if they are not rejected, they are provisionally accepted, that is, they are considered corroborated. Popper argues that inductive logic "does not provide a suitable criterion of demarcation"[7], and this criterion is nothing but Hume's old problem, taken over by Kant and transformed into the main problem of the theory of knowledge, i.e., "the problem of finding a criterion which would enable us to distinguish between the empirical sciences on the one hand and mathematics and logic as well as metaphysical systems on the other"[8]. The traditional positivist answer has been that the statements of the natural sciences are testable on an empirical basis or empirically verifiable. Popper supposes that this mechanism of empirical testing assumes an inductive logic and proposes an alternative demarcation criterion, that of falsifiability:

> „I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that it can be singled out, by means of empirical tests, in a negative sense: it must be possible for an empirical scientific system to be refuted by experience"[9].

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[6] Chhanda Gupta, "Putnam's Resolution of the Popper-Kuhn Controversy", in *The Philosophical Quarterly*, Vol. 43, No. 172, 1993, p. 322.

[7] Karl R. Popper, *The Logic of Scientific Discovery*, p. 11.

[8] *Ibidem.*

[9] Karl R. Popper, *The Logic of Scientific Discovery*, p. 18.

Popper develops the logic of falsifiability and explains that a theory is empirical and falsifiable if and only if it divides the class of all possible basic statements into two non-empty sub-classes, namely, the class of the potential falsifier of the theory (all the basic statements with which it is inconsistent), and the class of basic statements which it does not contradict. Briefly speaking, "a theory is falsifiable if the class of its potential falsifiers is not empty"[10].

Although Popper replaces verifiability with falsifiability, the problem of basic statements remains, and the only consistent solution available in such an empiricist framework is to admit both the relativity of the empirical basis and that, logically speaking, we have no other solution to the problem of justification. Popper mention Fries's trilemma, the choice between dogmatism, infinite regress and psychilogism, and claims that "the basic statements at which we stop, which we decide to accept as satisfactory, and as sufficeint tested, have admittedly the character of dogmas, but only in so far as we may desist form justifying them by further arguments (or by further tests)"[11].

Therefore, Popper admits that in the epistemological interplay between theory and experience, the latter can be considered a criterion for accepting the theory, another possibility being excluded because it leads to unacceptable logical consequences. But Kuhn asserts that different paradigms transform observation and experience[12] and that "paradigms determine large areas of experience at the same time"[13]. He 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"[14]. 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 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"[15].

However, Kuhn finds certain similarities between these two images of scientific change:

> "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

[10] *Ibidem*, p. 66.
[11] *Ibidem*, p. 87.
[12] See Thomas S. Kuhn, *The Structure of Scientific Revolutions. Second Edition Enlarged*, The University of Chicago Press, Chicago, 1970, p. 7, pp. 111–112.
[13] *Ibidem*, p. 129.
[14] *Ibidem*, p. 7.
[15] *Ibidem*.

> the role played in this process by the older theory's occasional failure to meet challenges posed by logic experiment, or observation"[16].

This description is, indeed, a very Popperian one.

But the mechanisms of the scientific progress are differently conceived by the two, as they themselves specified, accepting the controversy surrounding the difference between a logic of discovery and a psychology of research. The argument that we can build starting from Kuhn's discussion about the transformation of experience by a paradigm and the falsificationist thesis proposed by Popper according to which a basic empirical statement in the form of an existentially quantified sentence is sufficient to reject a hypothesis, is that if the observations are theoretically loaded, then could we still accept that the propositions of experience have the solidity of an epistemic basis that allows such decisions to be made? In short, does the theoretical loading of the observation affect the process of empirical testing?

AN EPISTEMOLOGICAL APPROACH OF THEORY LADENNESS

Popper and Kunh agree with the necessity of a distinction between the logic of discovery and the psychology of research, but each of them interprets differently the relevance of this distinction for an analysis of knowledge development. If Popper approaches both theories on science as logical theses, Kuhn, on the contrary, believes that the authenticity of his perspective lies precisely in the naturalization of traditional epistemological concepts, giving them historical, psychological and sociological dimensions. Hence the different ways in which the two philosophers describe normal science and take into account the problem of the framework.

Theory ladenness problem may be seen form the same perspectives, as a logical or semantical one, on the one hand, and as a psychological one, on the other hand. A logical approach is related with a linguistic vision on the scientific theory and with a semantic interpretation of the theoretical ladenness: the meaning of any observational term is (partially) determined by some theoretical presuppositions embedded in the previous background. The result is a weak form of theory ladenness, as Popper suggests, under the conditions in which we accept that the progress of science is made in the direction of getting closer to the truth, somehow in the mode of a semantic realism.

The psychological approach is based on a perceptual understanding of theory ladenness and is expressed by the principle according to which the theories shared

[16] Thomas S. Kuhn, "Logic of Discovery or Psychology of Research?", in *Criticism and the Growth of Knowledge*, pp. 1–2.

by the epistemic subject have an effect on his perceptions so that any observation contains theoretical ingredients. As a result of this, it is not possible to identify a basic level at which the epistemic subject can produce pure observations. The development of science occurs like *gestalt* changes, and the theoretical (or semantical) relations between different paradigms are described using the notion of incommensurability. The thesis can be extended from perceptual knowledge and judgments about perceptions to the entire cognitive process, from data interpretation and memory to communication[17]. Anyway, the consequence of such a psychological approach is that all appearances are theoretically laden in the sense that they are interpretations in one way or another.

If we leave aside the psychological aspects and consider only the logical consequences, then the debate between Popper and Kuhn regarding the theory ladenness concerns two main aspects, one regarding the empirical testing of theories, the other regarding the so called problem of the consensus of those who share certain common theoretical commitments.

Generally, according to the rules of scientific method, the scientist develops the theory under the control of observational evidence and these are used to test the predictions derived from the theory. But if any observation is theory laden then it means that some theoretical theses are already implicitly assumed from the beginning and, as a consequence, the observations cannot fulfil the task of neutral arbitrator between theories which are related with the presumed thesis. But when the premises of a reasoning assume from the beginning the truth of the conclusion instead to support it, then it is committed the so called fallacy of begging the question. This means that the position in question is at least partly assumed without a proof, in other words, that the argument has the logical form of a vicious circle. Thomas Kuhn and his followers argue that a scientist cannot use observational evidence to test a theory without a previous commitment to that theory. This means that the so called objectivity and neutrality of empirical data or evidence are only an epistemological myth.

One of the most cited case studies in literature is the one proposed by Laymon regarding the Michelson-Morley experiments. Laymon argues that the theory tested by Michelson-Morley experiments is assumed in the experimental design of these experiments. But although this supposition of the theory in its own test is without harmful epistemic effects, the so called Independent Evidence Thesis, namely, that "the appraisal of a theory is based on phenomena which can be detected and measured without using assumptions drawn from the theory under

[17] This suggestion was made by William F. Bretwertl, Bruce L. Lambert, "The Theory-Ladenness of Observation and the Theory-Ladenness of the Rest of the Scientific Process", in *Philosophy of Science*, vol. 68, no. 3, *Supplement: Proceedings of the 2000 Biennial Meeting of the Philosophy of Science Association. Part I: Contributed Papers* (Sep., 2001), pp. S176–S186.

examination"[18] must be subjected to examination. First, Laymont revised the thesis by adding the fact that the development of science is based on competing theories, so that "the appraisal of a theory is based on phenomena which can be detected and measured without using assumptions drawn from the theory under examination *or from competitors to that theory*"[19]. Therefore, the serious epistemological problem, in the form of a vicious circularity, would be that the theory to be tested or a competing one are used to select and describe the evidences used in the testing process. For example, in the case of Michelson-Morley experiments, first intention was to measure the velocity of the Earth with respect to the aether, but the experiments were then regarded as tests of the Fresnel aether theory itself. The results of these experiments were interpreted as a strong evidence against the existence of the aether. Thus, although we have identified a serious epistemological problem in the form of a vicious circularity, we can at the same time note the fact that we have no reason to abandon the testing procedures precisely because the use of the theory to be tested in the construction of the experiment does not logically imply that the results obtained will not be objective, so that we cannot decide if the tested theory deserves to be accepted or should be rejected. In fact, I would say, this always happens in any theory testing if we accept that the observations are theoretically impregnated and we have no other way to approach the facts.

The other side of theory ladenness is the problem of scientific agreement or consensus between the members of a research community and between the members of different research communities. If the background beliefs of two researchers are different then they may think that their observations are different although they observe the same phenomena. As a result, they would disagree not only about the theories, but also about these observations, and the latter would no longer be able to play the role to arbitrate. The scientific consensus at the level of scientific community would become more and more difficult to be achieved and the more the observations would be theoretically loaded, the deeper the gap would become.

Kuhn argues that the project of a neutral observational language or a so called "language of pure percepts"[20] is unfeasible because the theoretical commitments cannot be avoided: a paradigm, "taken either from a current scientific theory or from some fraction of everyday discourse"[21] is supposed from the beginning and any refined scientific language (or a language from which we eliminate all non-logical and non-perceptual terms; as Goodman does in *The Structure of Appearance*),

[18] Ronald Laymont, "The Michelson-Morley Experiment and the Appraisal of Theories", in A. Donovan, L. Laudan, and R. Laudan (eds.), *Scrutinizing Science: Empirical Studies of Scientific Change*, The Johns Hopkins University Press, Baltimore, 1988, p. 245.

[19] *Ibidem*, p. 246.

[20] Thomas S. Kuhn, *The Structure of Scientific Revolutions. Second Edition Enlarged*, p. 127.

[21] *Ibidem*.

"embodies a host of expectations about nature and fails to function the moment these expectations are violated"[22]. Thus, any language is restricted by the knowledge embedded in it and it cannot produce a neutral or objective description of the given. As a result of this paradigm embodied-experience associated with the cultural and professional patterns shared by a community, the world is cut both in concepts and in the things that populate it. For example, a Copernican who learned the new meaning of the world "planet" and how to use it correctly won't see a planet when he will look at the sun and he will work with new distinctions applied to the celestial bodies. Similarly, a chemist will see oxygen instead of dephlogisticated air, or a physicist will talk about a pendulum instead of a constrained fall. These theoretical biases are epistemologically active even when sentences are not used to describe the observations, but just graphics, drawings, audio or video recordings. Our choices, including the setting of the device, are influenced by the theoretical framework in which we find ourselves willy-nilly already from the very beginning[23].

Incommensurability, understood as the incapacity to choice between competing paradigms or theories, is a consequence of this radical interpretation of the theory-ladenness of observation. The argument is that all the observational terms are theory laden and that there can be no neutral observational language so that to compare the experimental results. The observational terms are different meanings in different paradigm even if we use the same word, for example, the word "mass" used in Newtonian mechanics or in Einstein's special theory of relativity. Mass is constant in the first case, but it depends on the velocity of the object in the second case.

But some authors have argued that in spite of the theoretical dependence, there are "circumstances under which the theory-ladenness of an experimental result can be a virtue"[24]. One of the cases that deserve to be mentioned is that different theoretical contexts explain why some experiments have to be considered differently. For example, tests of the velocity addition law at speeds close to or small compared to the speed of light are considered the same in Newtonian mechanics and it isn't necessary to make a distinction between them, while in the

[22] *Ibidem.*

[23] Although Kuhn seems willing to admit that an operational definition would allow a description in pure observational language (Th. Kuhn, *The Structure of Scientific Revolutions*, p. 129), Daston and Galison argued that even in these cases of the so called operational definitions there is a prior epistemological guidance for the choices and interpretations we make on these non-linguistic data (See Lorraine Daston, Peter Galison, *Objectivity*, Brooklyn, Zone Books, 2007, pp. 15–190, 309–361).

[24] A. Franklin, M. Anderson, D. Brock, S. Coleman, J. Downing, A. Gruvander, J.Lilly, J. Neal, D. Peterson, M. Price, R. Rice, L. Smith, S. Speirer and D. Toering, "Can a Theory-Laden Observation Test the Theory?", in *The British Journal for the Philosophy of Science*, Vol. 40, No. 2, 1989, p. 229.

Einsteinian relativity such experiment become different if the velocity is close to the speed of light.

Moreover, these experiments will be laden with the theory used to arrange the experimental apparatus, but there is no problem if the theory of the experimental apparatus and the theory of the tested phenomena are different. For example, in testing whether parity is conserved in the weak interactions, we use an apparatus whose operations depend on the strong and electromagnetic interactions, accepting that parity conservation was already established.

But, really, a more difficult problem arises when the apparatus or a part of it depends on the theory of the phenomena which is to be teste. This is the case of a vicious circularity, for example, if we use a mercury thermometer to measure the temperature as a part of an experiment whose purpose is to measure if the bodies expand when their temperature increases. But the circularity is avoided if the mercury thermometer is calibrated using another thermometer that depends on another theory, for example, a constant volume gas thermometer whose functioning depends on the proportionality of the pressure of a gas to its absolute temperature. Calibration serves to sepparate the theory of the apparatus from the theory under test.

Therefore, all we need to do is to include theoretical beliefs in the observational content, and not to conclude that theory ladenness of observation prevent us to test a theory. It is possible to design theory-neutral experiments and to overcome, even if not completely, the challenges of theory ladenness, as it were the failure to interpret observations, experimenter bias, or difficulties in data acquisition[25]. Anyway, "those who believe such tests are impossible should present a workable example from actual science. At the moment we know of no such cases"[26].

COMPARING THEORIES: A KUHNIAN MODEL OF FALSIFIABILITY?

Therefore, the revision of the distinction between the theoretical and observational levels and the thesis of the theoretical ladenness of observation does not at all endanger the thesis of testability or falsifiability as it is developed by Popper as a logical tool meant to explain the change in science. The claim that observation statements are theoretically impregnated differently doesn't mean that it is impossible to compare two theories if we take into account the relation between those theories and observations: even if the empirical consequences predicted in their own terms by the two theories are different, the observations will be relevant

[25] Allan Franklin, "The Theory-Ladenness of Experiment", in *Journal for General Philosophy of Science* [*Zeitschrift für allgemeine Wissenschaftstheorie*], Vol. 46, No. 1, 2015, p. 155.

[26] A. Franklin, M. Anderson, D. Brock, S. Coleman, J. Downing *et al.*, "Can a Theory-Laden Observation Test the Theory?", p. 231.

for each of the two theories and an epistemic decision shall be justified. In other words, in the end, we will be able to say which of the two theories explains the observations better, even if the conceptual schemes used are different.

If we consider the historical case of the Copernican revolution, extensively analysed by Kuhn in a book in which the theory developed in *Structure of Scientific Revolutions* is foreshadowed[27], then we can say that the price the scientist pays for attachment to an alternative is that he can make mistakes. A single observation inconsistent with his theory proves that he has been using the wrong theory all along. As a result, his conceptual scheme must be abandoned and replaced. Kuhn's problem in *The Copernican Revolution* is to identify and to explain the grounds that underlie the acceptance of a theory at a given time. I think that this historical research allows Kuhn to have some insights into the development of science as a process and to reach a view on the relations between science and society at different stages of science development. Through such historical research, we grasp both the common problematic area and the radical differences between Copernicus' theory and the previous ones, but at the same time, we find that the previous ones were equally credible for those who supported them. However, why were they accepted? The reason for their acceptance was the same for which we later accepted Copernicus' theory: "they provided plausible answers to the questions that seemed important"[28].

Kuhn thinks that a theory has its heyday when it is accepted and taken for granted. He believes that the history of science is an important source to have "a perspective from which to examine the scientific beliefs which it takes so much for granted"[29]. He is surprised by several aspects that were in disagreement with the traditional image of scientific progress:

- scientific theories do not follow each other cumulatively, but replace each other;

- theories in science are not definitive, they are temporary and they can be revised or abandoned;

- but the old theories were believed by the members of scientific community because they fulfilled some explanatory functions specific to science.

Kuhn concludes in an evolutionist vocabulary that anticipates further developments in the "new philosophy of science" and not only that "if we can discover the origins of some modern scientific concepts, and the way in which they supplanted the concepts of an earlier age, we are more likely to evaluate intelligently their chances for survival"[30].

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[27] I have developed this argument in Constantin Stoenescu, "Before *Structure*. The Rise of Kuhn's Conceptual Scheme in *The Copernican Revolution*", in *The Annals of Bucharest University*, vol. LXXII, No. 2, 2023, pp. 69–90.

[28] Thomas S. Kuhn, *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought*, foreword by James B. Conant. Harvard University Press, Cambridge, 1995, p. 3.

[29] *Ibidem*, pp. 3–4.

[30] *Ibidem*, p. 4.

The Copernican revolution was for Kuhn a case study that allowed him to understand the mechanism by which a theory is accepted. Moreover, he realized that astronomical observations and theories have an impact on cosmological thinking and on the set of concepts regarding the structure of the universe. Seen in their historical sequence, the cosmologies went further and further from a scientific, technical and systematic point of view, but each one, at its moment of glory, received the consensus of the intellectual community and society as a whole. This consensus is ensured by the fulfilment by each cosmology of two requirements, namely, that of providing an image of the world that satisfies certain psychological needs and that of giving a coherent explanation of the observed phenomena[31]. Scientifically speaking, the theories invented by the astronomers are tentative solutions based on interpretations of observations that are incorporated into the vocabularies used. From here we can reach the paradoxical situation in which "two astronomers can agree perfectly about the results of observation and yet disagree sharply about question like the reality of the motion of the stars"[32].

On the other hand, Kuhn discovered in the case of the Copernican Revolution a strong resilience of the belief in the central position of the Earth, although the problem of the planets revealed serious inadvertences. Kuhn becomes aware of the fact that a scientific community hardly gives up the conceptual scheme that its members share in common. That is why it is not hazardous to say that perhaps even modern man would believe in the universe of the two spheres if the only celestial bodies visible to the naked eye were the sun and the stars. But the planets were also visible. The logical form of Kuhn's argument is one of a reasoning by *reductio ad absurdum*. Kuhn adopts a methodological strategy in which the observations, as a tribunal of experience, are those from which the interpretations derive: "Once again we consider observations before dealing with interpretive explanations. And once again the discussion of interpretations will confront us with a new and fundamental problem about the anatomy of scientific belief."[33] But he finds that, in fact, already established beliefs were the ones that guided the observations and made them appear as we expected. However, in the case of the problem of the planets, the observations could no longer be adjusted according to our expectations and, therefore, this problem became the source of the Copernican Revolution.

In *SSR* Kuhn will provide several examples and reach a general conclusion about the scientific change that is completely opposite to the picture that Popper had already described. In the chapter "Thee Response to Crisis" Kuhn assumed that crises are a necessary precondition for the scientific change, but the so called anomalies aren't treated as counter-instances by scientists, though in the vocabulary of philosophy of science they are labelled as such. History of science shows us that a paradigm is declared invalid not because of its anomalies, but only if another candidate is ready to take its place. His generalization is unavoidable:

[31] *Ibidem*, p. 7.
[32] *Ibidem*, p. 26.
[33] *Ibidem*, p. 45.

> "No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature."[34]

So, according to Kuhn, doubts about a theory do not appear as a result of some anomalies that should lead to severe tests through which the theory is falsified, but because these difficulties, always present in the paradigm-nature fit, can lead to the emergence of a competitor that does not face the same entanglements, for example, as in the case of Copernican theory, the calendar is better designed. It is interesting that Kuhn reflected at length on this aspect regarding the detachment from Popper's theory during the development of *SSR*, being aware of the consequences of such an epistemological option. Kuhn sent the manuscript of his work to Feyerabend, and the latter, still influenced by the falsificationist methodology proposed by Popper, made two objections, namely, that history is irrelevant to the methodology of science and that the history of science rather supports the falsificationist stereotype. Feyerabend contrasts what science is in a historical and descriptive sense with what science should be in a methodological and normative sense. Moreover, the so-called stereotype of falsification is not based on the history of science, but starts from the comparison between testable theories and untestable theories, i.e. dogmatic, from the fact that we prefer theories that have empirical relevance compared to those that do not can conflict with experience. We prefer theories with a high degree of testability and consider alternatives that provide additional tests. If a theory is not abandoned as soon as it is confronted with a counterexample, this does not mean that the falsificationist stereotype is incorrect, but something else, namely, that a truly critical attitude does not involve abandoning the theory immediately after the first difficulties appear, but systematically testing it. On the other hand, the history of science shows us that theories that were later rejected were initially accepted precisely because they made correct predictions. Falsificationism is compatible with such a historical situation because a theory that successfully passes empirical tests is thereby only provisionally accepted as a useful tool.[35] These objections remain valid from the perspective of a normative methodological approach, even if Feyerabend himself will adopt Kuhn's theory and even radicalize it.

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[34] Thomas S. Kuhn, *The Structure of Scientific Revolutions. Second Edition Enlarged*, p.77.

[35] See Paul Hoyningen-Huene, "Two Letters of Paul Feyerabend to Thomas S. Kuhn on the Draft of *The Structure of Scientific Revolutions*", in *Studies in History and Philosophy of Science*, vol. 26, no. 3, Elsevier, 1995, pp, 353–387. For a detailed analysis of these comments made by Feyerabend to the first version of *SSR*, see Constantin Stoenescu, "Începutul drumului către *Structură* și două scrisori ale lui Feyerabend către Kuhn", in (eds.) M. Dumitru, A-P. Iliescu, V. Mureșan, C. Văduva, *60 de ani în universitate. O carte dedicată profesorului Mircea Flonta*, București, Editura Universității din București, 2015, pp. 317–344.

This demarcation between the normative and descriptive approaches of scientific change and the so-called comparison of theories with nature is clearly depicted by Lakatos:

> "For Popper scientific change is rational or at least rationally reconstructible and falls in the realm of the *logic of discovery*. For Kuhn scientific change – from one paradigm to another – is a mystical conversion which is not and cannot be governed by rules of reason and which falls totally within the realm of the (*social*) *psychology of discovery*"[36].

This distinction between logic of discovery and psychology of research became the public label for the philosophical clash between Popper and Kuhn. Moreover, Lakatos brings another additional distinction that allows an even better understanding of the debate between the two philosopher, that between naïve and sophisticated falsificationism. In Lakatos's view, Kuhn understands only naïve methodological falsificationism according to which "science grows by repeated overthrow of theories with the help of hard facts"[37], but not sophisticated falsificationism, so that his critique touches only the first one. If for the naïve falsifications any experimentally falsifiable theory is acceptable, for the sophisticated falsificationism a theory is acceptable only if it leads to the discovery of new facts. Therefore, the relation between theory and facts is radically revised by the sophisticated version of falsificationism and theory ladenness of observation ceases to be a problem for the process of falsifying a theory based on confronting experience since "contrary to naïve falsificationism, no experiment, experimental report, observation statement or well-corroborated low-level falsifying hypothesis alone can lead to falsification. There is no falsification before the emergence of a better theory."[38]

A FINAL REPLY AS A CONCLUSION

But the idea of rejecting theories and the capacity of a new one to discover new facts is common to both Popper's falsificationist theory and Kuhn's paradigm shift theory, so that we can talk about a kuhnian model of falsifiability[39]. The difference is that the logical procedures are different: Popper's traditional account of falsifiability is an argument by *modus tollens*, while Kuhn's idea of a paradigm shift is an argument by *reductio ad absurdum*.

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[36] Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes", in (eds.) I. Lakatos, A. Musgrave, *Criticism and the Growth of Knowledge*, p. 93.

[37] *Ibidem*.

[38] Imre Lakatos, "Falsification and the Methodology of Scientific Research Programmes", p. 119.

[39] See Mark A. Stone "A Kuhnian Model of Falsifiability", in *The British Journal for the Philosophy of Science*, Vol. 42, No. 2, 1991, pp. 177–185.

As far as the possibility of comparing theories is concerned, Kuhn sticks to a semantic approach: "The point-by-point comparison of two successive theories demands a language into which at least the empirical consequences of both can be translated without loss or change."[40] This kind of evaluative comparison of theories is based on a common or neutral language that would consist of pure sense-datum terms, or, in Delvin's interpretation "a vocabulary that attaches to nature 'in ways that are unproblematic' and 'independent of' any scientific theory about nature"[41]. But, as we already have accepted above, there is no such a theory independent or neutral language. Therefore, the temptation of incommensurability remains for Kuhn.

[40] Thomas S. Kuhn, *The Road since Structure. Philosophical Essays, 1970-1993, with an Autobiographical Interview*, edited by James Conant and John Haugeland, The University of Chicago Press, Chicago and London, 2000, p. 162.

[41] William Devlin, "Kuhn and the Varieties of Incommensurability", in K. Brad Wray (ed.), *Interpreting Kuhn. Critical Essays*, Cambridge, Cambridge University Press, 2021, pp. 109–110.