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SCIENTIFIC REVOLUTION, INCOMMENSURABILITY AND TRUTH IN THEORIES: OBJECTION TO KUHN'S PERSPECTIVE

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ABSTRACT

This is to revaluate the truth-content of scientific theory in the event of scientific revolution. In this paper, we argued that the radical views on incommensurability offered, especially, by Thomas S. Kuhn (shared by Paul Feyerabend, and others) do not actually undermine either the realist or cumulative stature of science. The core of our discussion is, ultimately, to provide a clearer and broader picture of the general characteristics of scientific revolution or theory change. In doing this, the paper pinpoints the audacity behind this change and the nature of truth undergirding any emergent or overthrown scientific theory. The paper has some rebounding echoes of the realist and cumulative features of science while addressing the issue of the real character of theory change. Following some unique interpretations of scientific revolution, truth in science could still be redeemed in the face of theory change and Kuhnian Incommensurability Thesis. To this effect, the hermeneutical approach is considered most suitable in this paper.

Key Words: Scientific-Revolution; Incommensurability; cumulative structure; Truth; Realism

INTRODUCTION

Thomas S. Kuhn tries to lay credence to his own position on scientific revolution by telling us from the start to look back at the history of science in order to get a better picture of how science works and how theories change. Kuhn did maintain, in the opening statement of his book, *The Structure of Scientific Revolutions*, that if history is properly viewed as something more than a mere repository for anecdotes or chronology, then it could produce a "decisive transformation" in the Received Image of science by which "we are now possessed". There are a number of items making up the image Kuhn wants to transform. They include: Demarcation criterion between science and non-science, Unity of Science, Cumulative stature of science, Realism, Observation-Theory distinction, Precision of concepts in science, Observation and experiment as foundations of science, Context of Discovery-Context of Justification dichotomy and the question of tight deductive structure of science. Kuhn presents his own picture with ideas like paradigm, normal science, incommensurability, crisis, conversion and paradigm switch. Now, Martin Carrier makes us understand that, it was actually in London (1965) when Kuhn's *The Structure of Scientific Revolutions* (1962) was "broadly discussed as a challenge to Karl Popper's falsificationist methodology at a conference" that the issue of theory change was first set to boil, i.e., became a heated and urgent matter. In the words of Carrier: "The ensuing controversy between Kuhn and Popper and the later contributions from Imre Lakatos, Paul Feyerabend and others did much to fuel the debate and lifted theory change to the top of the agenda of philosophy of science" (Brown 132). In the thick of this unending debate "Kuhn, Feyerabend, and others" agreed and "graphically demonstrated" that the old theory or "deteriorating framework will not be given up until and unless there is a genuine rival on the scene" (Van Fraassen *The Empirical Stance*, 93). Of course, any of the rivals that shows its mettle and appears most robust and smooth is appreciated and accepted. But, then, our key question in this paper is: Does scientific revolution or theory change actually undermine the truth of a theory that has been overthrown? The answer provided in this paper is that truth in science could still be retained in the face of theory change that gives rise to the Kuhnian Incommensurability Thesis. The thrust of our argument is that Kuhn's Thesis of incommensurability in theory change does no actual damage to the cumulative or realist image of science. This, we suppose, is what makes our paper different from the contributions of other scholars who have been engaged in this time-honored debate. In other words, this paper serves as a modest contribution to the discussions of scientific revolution in the history and philosophy of science.

In point of fact, we cannot get away from theory change even though we have some level of relative stability in science. In Gaston Bachelard's view, science develops "through a series of discontinuous changes (epistemological breaks)" (Audi 67). As it stands, great scientific theories that aided some technological achievements have yielded grounds in the past to more comprehensive and sophisticated theories. This explains why Paul Dicken (2016) poses the following question: "…how can we be sure that our own scientific theories will not suffer a similar fate in the future?" (2). Bearing this supposedly Pessimistic Meta-Induction in mind, Popper once said, in the London School of Economics Lectures, that science could aptly be described as "revolution in permanence" (qtd. in Baigrie and Hattiangadi 436). But this seeming Pessimistic Meta-Induction, coming from Popper, does not in any way make him an antirealist. Popper, in his doctrine of *verisimilitude*, has demonstrated to us that he is at least a fiduciary realist who holds a strong faith that we are gradually getting closer and closer to the truth in the several twists and turns of scientific revolutions. In the midst of this, we claim that any scientific theory with a fair measure of veracity, regardless of the tsunami of revolution, retains its pragmatic applicability.

It stands to reason that we cannot go the whole breadth in resolving the entire difficulties involved in the issue of theory change, as intricate as they may be. The paper does not, in the strict sense, provide any template for theory change. In strict terms methodological terms, our focus is mainly on Kuhn's Incommensurability Thesis, which is an outcome of scientific revolution, and seems to puncture the cumulative stature of science. If the cumulative is so undercut, then scientific realism is also affected in a remarkable way. This becomes clearer with Ian Hacking's assertion that, "Kuhn's historico-philosophical work has been a major element in the re-discussion of realism" (66). In other words, the paper is a concourse of discourse on theory change, incommensurability, cumulative stature and realism (as it appertains to the truth of scientific theories).

The foregoing gives us, then, the warrant to exfoliate the concept of scientific revolution; examine the cumulative stature and the Kuhnian Theisis of incommensurability, pointing out, as it were, the very spot, if any, where the cumulative stature of science has been seriously punctured in the event of any scientific revolution; and lastly proceed to our unique account of theory change which attempts to keep the cumulative and realistic posture of science intact. We then conclude with a summary overview.

EXFOLIATING THE CONCEPT OF *SCIENTIFIC REVOLUTION*

The two concepts, *scientific revolution* and *theory change*, are used interchangeably in this paper. Even though both terms are run together for the purposes of this paper, it remains a clear fact that, not all changes in theory inspire the elaborate Kuhnian kind of revolution in science. In the labyrinthine world of Kuhn, *revolution* has something to do with *paradigm shift* in a science that has crossed the pre-paradigmatic stage. For Kuhn, scientific revolution is a great fire ignited by steady accumulation of anomalies in the day-to-day practice and application of theories within Normal Science. These anomalies arise as a result of failure on the part of theories to conform to experimental tests and other applications. The piling of anomalies, according to Kuhn, gradually leads to a *crisis* moment, which eventually precipitates into a full-blown revolution. Now, articulating more precisely the Kuhnian notion of revolution, K. Brad Wray maintains that, "Scientific revolutions are those changes in science that (1) involve taxonomic changes, (2) are precipitated by disappointment with existing practices, and (3) cannot be resolved by appealing to shared standards" (61). It is pretty obvious that, there is more to scientific revolutions than the limits seemingly given to us by Kuhn. Of course, Kuhn understands there are other dimensions in the discourse of scientific revolution.

Meanwhile, one should not be oblivious of the idea of scientific revolution commonly shared by Pierre-Maurice-Marie Duhem, Alexander Koyré, and John A. Schuster, among others. Duhem, in *The Aim and Structure of Physical Theory* (1903), did make a seemingly paradoxical comment, namely that, "the history of science can record no revolution" (qtd. in Baigrie and Hattiangadi 443). Duhem did not say this because he never read from the history of science that theories replace one another as science progressed through time. His major aim was simply to defend the cumulative image of science, especially from the view-point of mathematical conception of scientific theories. However, Duhem's contention was not brooked or countenanced by many scholars, especially Koyré, who derided his naïve understanding of the early modern applications of mathematics in science.

There is need to put Duhem's statement in proper perspective, which time and space may not allow us. But for the records: the main sense of *scientific revolution* Koyré had in mind (following Duhem's denial) was that of intensification of scientific activities in the early modern period – i.e., between the 16th and 17th centuries. For John A. Schuster also, "…

Scientific Revolution is commonly taken to denote the period between 1500 and 1700, during which time the conceptual and institutional foundations of modern science were erected upon the discredited ruins of medieval world-view…" (217). Duhem was opposed to this kind of idea, maintaining what he called ***continuity thesis***, which strictly claims that there was proliferation of scientific activities even in the middle ages. To be sure, Duhem demonstrated his thesis by offering ample examples of intense experimental and theoretical studies on statics and dynamics to the point that the modern period can no longer be seen as the pinnacle of "intensified" scientific activities.

Pretty clearly, the foregoing Koyréan or Schusterian sense of scientific revolution (which Duhem strongly discountenanced) differs from the Kuhnian sense of *Scientific Revolution*, which simply amounts to *theory change*. Meanwhile, we are very much attached to the Kuhnian dimension of *revolution*, which in no way can be taken as a direct rebuttal of the Duhem-Koyré-Schusterian notion of *revolution*. Our key interest in Kuhn's idea is incidentally where it hinges on his notion of incommensurability, to which we must now turn.

CUMULATIVE STATURE AND THE RADICAL QUESTION OF INCOMMENSURABILITY

Over the years, philosophers of science have dwelt on the issue of theory change to the extent that it has become well-worn or shop-soiled. Ernest Nagel, in *The Structure of Science* (1961), holds the view that scientific knowledge is cumulative, such that any given successor theory must necessarily take the preceding theory under its wings – i.e., the old theory is subsumed or is somewhat ***reducible*** to the new theory. The Logical Positivists coined a Received View, dubbed "Thesis of Development by Reduction" – largely sponsored by Rudolf Carnap and Carl Hempel. What reduction of the old theory entails is that, a new theory must be able to solve all the problems its predecessor could solve, as well as the ones that appeared quite insoluble for the old theory. In other words, the new theory must be able to take care of all the predictions of the old theory, describe all the associated phenomena and eliminate all sources of error and failure in the old theory. Within this Received or Time-honored View of science, **T**2 (i.e., the new or successor theory) could be seen as the more robust form of **T**1 (i.e., the old or superseded theory). In line with this, Moritz Schlick (leader of the Vienna Circle) once argued that no theory which has at any point in time undergone verification may be completely dispensed with (Brown 133). The conventionalist Henri Poincaré corroborates this positivistic orientation when, in *Science and Hypothesis*, he maintains that, something must forever remain of an old theory (say, the structural relation of terms in a given theory or the very *theoretical entity* being investigated). Yet another conventionalist Duhem, who had a strong affiliation with the positivist tradition, argues, in *The Aim and Structure of Physical Theory*, to the effect that scientific knowledge is cumulative up to the extent that any scientific theory can, after all, be saved by making some major or minor adjustments in the relevant hypotheses. In the logical positivist/empiricist worldview, therefore, scientific revolution means nothing but the upgrading of the dignified realistic and rational outlook of science. Now, articulating in a summary way, the picture of science as cumulative, Frederick Suppe observes succinctly that, science as a cumulative enterprise extends and augments old success with new success. The upshot is that, "old theories are not rejected or abandoned once they have been accepted; they are just superseded by more comprehensive theories to which they are reduced" (Suppe 56).

Meanwhile, in his wake, Kuhn tried to upturn this somewhat classical image of scientific revolution or theory change, calling in question so many ideas that go with the cumulative image of science. Kuhn sees it somewhat as "an image of a national culture drawn from a tourist brochure or a language text" (1). In alliance with Kuhn, Paul Hoyningen-Huene maintains that only a "… presentist sort of historiography allows the history of science to appear as a cumulative growth of knowledge…" (488-489). In other words, the cumulative portrait of science is a distortion of real history of science. Of course, Kuhn challenged the entire Received Image of science on many fronts, especially wounding the rational disposition of science. But at the moment our interest is not directly on redeeming the rationality of science. The inkling is more on salvaging the *cumulative* and the *realist* postures of science in the midst of relentless and countless revolutions in science. But before anyone can achieve this one must needs look at the various dimensions or varieties of incommensurability ferreted by Kuhn and his allies in the process of discussing the question of scientific revolution.

Now, R.I. Griffiths affords us a perfect Kuhnian picture of what happens in the event of scientific revolution as he writes: "…scientific change takes place by a process of revolution in which the ontology, epistemology, and methodology, provided by one paradigm, is replaced by a new one. The old paradigm is called into question when a sufficient number of sufficiently severe experimental failures (anomalies) throw the paradigm into a crisis state" (Griffiths 127). It is noteworthy then that, for Kuhn, in the event of any scientific revolution, **T**2 (successor theory) is not commensurable with **T**1 (preceding theory). The fact is that the successor theory addresses different problems and also uses different theoretical terms or concepts. The implication is that **T**1 can never be subsumed under **T**2. This is because there is no common measure or yardstick with which the two theories can be compared. This further entails that there is no room for crucial experiment in science – i.e., the experiment that helps us decide or choose between two rival (or competing) theories. Following Kuhn, the original idea of incommensurability reads: "In applying the term 'incommensurability' to theories, I had intended only to insist that there was no common language within which both could be fully expressed and which could therefore be used in a point-by-point comparison between them" (qtd. in Sankey 769). Nonetheless, Sankey mutes that in 1983 "…Kuhn outlines a notion of 'local incommensurability' which he claims to have been his original idea. Local incommensurability consists in failure to translate between localized clusters of inter-defined terms" (770-771).

In the thick of the debate on incommensurability, W.H. Newton-Smith, in *The Rationality of Science* (1981), did trace some sources of incommensurability. He mentions the one that arises "due to value invariance" (149). He also enumerates the one brought about by putative "radical standard variance" (150). He, finally, shovels-in the one that emerges "due… to radical meaning variance" (150). But most fittingly, Hacking identifies three different types of incommensurability advanced by both Kuhn and his worthy ally, Paul K. Feyerabend (65-74). The three kinds are ***topic*** incommensurability, ***meaning*** incommensurability and ***dissociation***. Dissociation has to do with the cultural time-lag that separates the thoughts of writers of a more distant past from the people of the present, making it a lot more difficult for current Western students to understand hermetic writers like Paracelsus and John Dee, among others. The only way to get over this kind of difficulty, according to Hoyningen-Huene, is to apply "stubborn *hermeneutics*" to such antiquarian texts (489). For its own part, topic incommensurability has to do with the idea that a new theory has a different fish to fry, i.e., it treats a topic that is clearly different from the topic that the old theory focused on. Lastly, meaning incommensurability occurs with the fact that often times a succeeding theory gives different understanding(s) to some key theoretical term(s) or concept(s). In other words, there is a change in the meaning of those key theoretical terms. Following the doctrine of meaning-incommensurability, the term "planet" means or represents different things in Ptolemaic and in Copernican astronomy; "mass" represents different things in classical Newtonian physics and in Einsteinian relativity physics; the “atom” of John Dalton is different from that of J.J. Thomson, Ernest Rutherford, and *that* talked about in quantum or wave mechanics; and so on.

Within the discourse domain of meaning incommensurability, Kuhn’s ally, Feyerabend, swiftly object to Nagel’s position on reduction of old to new theories. Feyerabend begins by insisting that, “…a change to new meanings and new quantitative assertions is a natural occurrence which is also desirable for methodological reasons ….” Feyerabend further enunciates that, “For Nagel such a change is an indication that reduction has not been achieved, for reduction in Nagel’s sense is supposed to leave untouched the meanings of the main descriptive terms… (81). In this case, Feyerabend sees the Nagelian inclination to “meaning invariance” as totally unacceptable and unreasonable. But this is an intricate matter, for we do know that Nagel’s understanding of “invariance” is different from Feyerabend’s putative notion of it. Nagel (sharing a frame of mind with a figure like Hacking) knows that in the event of any scientific revolution certain aspects and functionality of theoretical term/entity must be tampered with. The truth of the matter is that, whereas Nagel, Saul Kripke, Hilary Putnam, Hacking and such kindred minds can condone such tampering with the functionality or stereotype of any theoretical/unobservable entity, Feyerabend, Kuhn, Bas C. van Fraassen and their likes will not tolerate it. They would rather feel that there is a complete change in the entity involved. Taking meaning incommensurability to observational report and theoretical language distinction, Suppe insists that, “Feyerabend’s view here comes as…a complete reversal of the Received View’s picture of a one-way flow of meanings from the observation language to the theoretical language” (176).

Henceforth, Gürol Irzik and Teo Grünberg, in “Carnap and Kuhn: Arch Enemies or Close Allies?” did trace the source of what the latter day Kuhn calls “local meaning incommensurability”, which is simply “…the claim that there is no language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be translated without residue or loss” (291). This theme of untranslatability (a post-positivistic orientation in the philosophy of science), is seen clearly in what Rudolf Carnap regards as semantic incommensurability. Carnap’s espousal of this semantic incommensurability runs thus:

> In translating one language into another the factual content of an empirical statement cannot always be preserved unchanged. Such changes are inevitable if the structures of the two languages differ in essential respects. For example: while many statements of modern physics are completely translatable into statements of classical physics, this is not so or incompletely so with other statements. The latter situation arises when the statement in question contains concepts (like, e.g., 'wave-function' or 'quantization') which simply do not occur in classical physics; the essential point being that these concepts cannot be subsequently included since they presuppose a different form of language (qtd. in Irzik and Grünberg 291).

As it stands, the Kuhnian meaning incommensurability in all its shades of interpretation is often called Kuhn’s *latest* conception of incommensurability by some scholars. Xiang Chen, in “Thomas Kuhn's Latest Notion of Incommensurability,” offers the fundamental reason why Kuhn had to latch unto this notion of incommensurability. Kuhn did so precisely because he wanted to avoid being continuously labeled as a relativist. He also abandoned the hackneyed Gestalt-switch analogy and expunged the “implied perceptual interpretation of the thesis”. In the process of this remediation:

> Kuhn then develops a metaphor based on language: during scientific revolutions, scientists experience translation difficulties when they discuss concepts from a different paradigm, as if they were dealing with a foreign language.

> Incommensurability thus is confined to meaning change of concepts, and becomes a sort of untranslatability (Chen 258).

This untranslatability thesis of Kuhn's (including, of course, that of Carnap) synchronizes with Quine's doctrine of indeterminacy of radical translation. Sankey corroborates this, while knowing that Kuhn bluntly refused to accept Quine's idea of inscrutability of reference. But, then, Kuhn's version of indeterminacy of translation is readily branded *taxonomic* incommensurability in recent times and is considered to have links with van Fraassen's constructive empiricism, which, *eo ipso*, "leads to taxonomic incommensurability" (Bird 693). At the moment, we may need to explore more on this taxonomic notion of incommensurability. From Alexander Bird it is made evident that, Kuhn applied this sort of incommensurability in his later work. As it now stands with Kuhn:

> Two theories are taxonomically incommensurable when there is no straightforward translation between taxonomies of the two theories. This is exhibited, for example, by chemical theories before and after Lavoisier. Eighteenth century chemists talked of 'principles,' a term that has no translation in the language of nineteenth century chemistry, while the latter referred to 'elements,' meaning by that term something quite different from the same word as used by Priestley and others (Bird 693-694).

For his part, Chen explains that taxonomy has to do strictly with ***kind*** terms, so much so that when Kuhn talks about taxonomic terms he is simply referring to kind terms. In fact, in his 1991 essay, "The Road Since *Structure*", Kuhn clearly states: "To the extent that I'm concerned with language and with meanings at all, ...it is with the meanings of a restricted class of terms. Roughly speaking, they are taxonomic terms or kind terms, a widespread category that includes natural kinds, artificial kinds, social kinds, and probably others" (qtd. in Chen 258-259). Beyond Chen's review, Putnam and Hacking did argue extensively against the issue of natural kinds and other kind terms. Here, *natural*, *artificial* and *social kind* terms are of little significance when one is considering the practical or pragmatic dimension of science as we are doing in this paper. Yet our interest is simply to lay bare a clearer understanding of taxonomic incommensurability from the Kuhnian point of view. In which regard we now invoke a suitable passage from Chen thus:

> A scientific revolution produces a new lexical taxonomy, in which some kind terms refer to new referents that overlap with those denoted by some old kind terms. Therefore, incommensurability does not result merely from translation failures of individual concepts. The prerequisite for full translatability between two taxonomies is not shared features of individual concepts, but a shared lexical structure.... Scientists from rival paradigms face incommensurability because they construct different lexical taxonomies and thereby classify the world in different ways (260).

Be that as it may, one very interesting thing Bird (692) said about the Kuhnian idea of incommensurability is that it does not in any way support antirealism, though he still pinned Kuhn down as an epistemological antirealist by other means. In "Once More on Kuhn," John S. Nelson clearly notes that, "Kuhn is at one point troubled by whether one should say that a scientific revolution is a change in human apprehension of reality, a change in the reality itself, or both" (81). According to Nelson, "Kuhn, however, goes one step further. He claims that it is not at all clear what it would mean to say that one theory is 'more like' reality than another" (82). This Kuhnian confusion should sound like sweet music to some ears in the midst of howling winds of scientific revolutions. Are scientists engaged in two different paradigms actually leaving in two different worlds? From where do they select their facts?

These questions are left for Kuhn who certainly accepts inter-paradigmatic autonomy of facts just like Feyerabend.

Henceforth, A. Polikarov writes on the issue of validation of Incommensurability Thesis (IT) and insists that the general notion of incommensurability (Ic) is derivable "from the concepts of scientific paradigm (P) and scientific revolution (R)". He maintains that, "There are several concepts of P, as well as various conceptions of R. The Ic concept also has more than one meaning". As such, "The validity of the IT is restricted to a subset of P, R, and Ic" (Polikarov 127). Unfortunately, this is a fact some other critics of Kuhn fail to appreciate. Yet, Giovanni Boniolo warns that the idea of incommensurability introduced by the "Post-empiricist philosopher of science, e.g. Hanson [1958], Kuhn [1970] and Feyerabend [1975]" must be taken with a little pinch or grain of salt. Boniolo reminds us that, "A scientific theory is not something which grows independently of history and, therefore, is not something which has nothing to do with the other theories" (469).

In all fairness, what Kuhn said concerning revolution and incommensurability could undoubtedly be applicable to some instances in the history of science. But the degree of incommensurability that could grant such an impetus to scientific revolution in order to completely override the cumulative stature of science is a pivotal issue that Kuhn neglected to bring to bear in his analysis. Since Kuhn maintains silence, it shows that he somewhat realizes the fact that the "Thesis of Development by Reduction" so preached by Nagel, Carnap and Hempel, among others, can also be supported by some historical evidence in science. What makes a new theory completely estranged from the old theory if they are still tackling the same problem, though from different dimensions? Most times, the same entity is still being investigated by rival or competing theories. The stereotypes or features of the said entity might change from one theory to another, but that does not warrant a change in taxonomy. Carl Linnaeus, the famous taxonomist, will never classify an albino tiger, for instance, as a different species of animal. Kuhn's general semantical (meaning) notion of incommensurability seems very appealing on the surface. But, then, when we go beyond the surface, i.e., beyond the logico-mathematical or linguistic models of theories on paper, and begin to link them to some empirical phenomena or reality, Kuhn's incommensurability thesis loses appeal.

To a large extent, science exists beyond linguistic razzmatazz. Science deals with real entities and issues about the world, language is only secondary. It is the duty of the scientific community to teach both the "theorize-first" and "look-first" scientists to associate their cognitive capacities with facts in the real world. At least one philosopher of science, Alexander Rosenberg, was honest enough to admit, willy-nilly, that one science (say, biology) suffers some serious limitations which other core sciences, namely physics and chemistry, do not suffer from. According to him, "…the limits reflect a combination of facts about the world and facts about the cognitive and computational limitations of the scientists…" Following from this, Rosenberg reveals that the key problems biology "faces are those of explanatory and predictive power" (58). The truth of the matter remains that, besides the capacity to unify mental constructs and physical facts, the ability to gather some seemingly disparate facts and form them into a theoretical nexus (decked out in a logico-mathematical language) is one of the most brilliant values in scientific practice and heightens the predictive capacity of any scientific enterprise. Little wonder biology has such a degraded dignity in the district of the natural sciences, since it falls short of some such brilliant skills.

In order to adequately uncover why Kuhn's arguments on incommensurability should really be taken with a grain of salt, we must veer my way to some unique account of theory change. This will clear the air on how it really stands with revolutions in science.

A UNIQUE ACCOUNT OF THEORY CHANGE

The neo-positivists (Rudolf Carnap, Ernest Nagel, Hans Reichenbach), the radicals (R. N. Hanson, T. S. Kuhn, Paul Feyerabend, Imre Lakatos), the convensionalists (Henri Poincaré, Pierré Duhem, Edouard Le Roy), and the falsificationist Popperians or critical rationalists, are some of the groups that did explore the facticity of *scientific revolution*. But our aim at this point is not so much to present their various views here as to point out that most of those views are lopsided. This lopsidedness arises from the tendency to universalize their doctrines as exhaustive principles of scientific revolution. Perhaps some of their statements concerning the character of science and its revolutionary attribute contain some elements of truth. Sometimes in the event of scientific revolution one theory is subsumed or absorbed into another (as the neo-positivist Nagel says). At other times, the revolutionary structure appears non-cumulative (as the radical Kuhn and Feyerabend maintain). Further still, revolution may be a matter of *problemshift* (as Lakatos holds). Most times, too, what scholars call revolution may simply be a matter of adjustment in the auxiliary hypotheses (as the conventionalist Duhem and instrumentalist Quine's Thesis posits). Yet, again, sometimes once theories are falsified they are thrown out without any major adjustments (as Popper principally conjectures). In an eclectic spirit, we do take them all as likely possibilities in the event of scientific revolution. Each of such positions has some degree of plausibility.

Henceforth, we wish to combine some sterling statements made by Kuhn, Popper and Lakatos in order to provide some *raison d'être* for the slim measure of instability witnessed in science. Kuhn made a claim that "science does not deal in all possible laboratory manipulations. Instead, it selects those relevant to the juxtaposition of a paradigm with the immediate experience that paradigm has partially determined" (Kuhn 87). Popper made a declaration that "…progress in science, may be regarded as a means used by the human species to adapt itself to the environment, to invade new environmental niches, and even to invent new environmental niches" (Popper 81). And lastly, Lakatos talked about *problemshifts* (*degenerating* and *progressive*). To be sure, according to Lakatos, the "demarcation between progressive and degenerating problemshifts sheds new light on the appraisal of scientific …explanations" ("Methodology of Scientific Research Programmes" 118). Painting further a broad picture of his methodology of research programmes, Lakatos explains:

> A research programme is a rather special kind of 'problemshift'… It has a tenacious *hardcore*, like the three laws of motion and the law of gravitation in Newton's research programme, and it has a *heuristic*, which includes a set of problem-solving techniques ("Why did Copernicus's Research Programme Supersede Ptolemy's? 179).

Here, then, we try to stitch these disparate views into a seamless whole or radiant mosaic, as it were. We invoked Kuhn mainly to support the conviction that scientific experiment deals with many test combinations or test situations that cannot be said to be exhaustive at any given moment in a theory's history. Popper himself is ineluctably invoked to advance or support our view that science is evolutionary and keeps penetrating deeper realms of reality, making new findings, and helping us dominate "new environmental niches". Ultimately, we deployed Lakatos mainly to state our belief that sometimes theories are abandoned owing to shift of interest, or to the fact that other aspects of some given phenomena have arrested the interest of experimenters or theoreticians within a given field. With all these convictions in place the seeming strong jaws of scientific revolutions loose grips on the fact that some theoretical explanations remain *true* even at the point they are rejected and abandoned or merely forgotten for want of relevance.

To be sure, we do not accept Robert F. Almeder's Pessimistic Meta-Inductivist stance which holds that, "…people were simply mistaken in thinking that there was a relationship of correspondence between extra-linguistic fact and the proposition originally assigned 'is true'… For we know that a significant proposition of statements presently assigned 'is true' will be re-assigned 'is false' in future" (58). This sort of thing does not happen in science: Once any existential or empirical proposition or hypothesis has been accurately verified, it can hardly be falsified again if all things are left equal. If experimental test conditions change, then there is this likelihood that the result might change. Consequently, I think that the following question raised by Almeder is *sine cum fundamento in rem*: "How does it come to pass that one and the same statement can be assigned 'is true' at one time and 'is false' at a later date?" (59).

As matter of fact, scientists have not all the time and funds on earth to do a combination of all possible test situations. But a time will come in the history of science when luck, interest, need and finance might push scientists to do some novel combinations of experimental tests which may yield a different result. Contrary to the assumptions of many, Nature itself, like man, is dynamic. In point of fact, Aristotle, Friedrich Nietzsche, along with the early 20th century metaphysicians or process philosophers like Henri Bergson and Alfred North Whitehead, etc., have all a very firm grasp on the idea of Nature as dynamic or evolutionary. In other words, Nature obeys the Heraclitian principle of flux, *à la* "*Pantha rei kai ouden menei*" (Everything changes, nothing remains constant). Radioactivity of substances and quantum physics, for instance, afford some wonderful insights into this very Heraclitian picture of reality. The truth is that, Nature will not behave the same way under different catalytic impulses. Incidentally, as Nature keeps unfolding itself under different promptings and perturbations of the experimental art, we keep discovering that the law-like order we experience at the macro-physical level does not actually obtain at the micro-physical level. Hence, Ian Stewart is right in saying that, "…the decays of a radioactive atom, are held to be determined by chance, not law" (2).

The scenario of unpredictability (chance) and discontinuity of forms gave rise to what Popper appropriately calls the dualistic picture of the world: "indeterministic at the small, owing to quantum-mechanical indeterminism, and deterministic in the large, owing to macro-physical determinism" (92). The contemporary import and intricacies of chance or indeterminism were also well-expounded by David Bohm in *Causality and Chance in Modern Physics*. Bohm rightly observes that, "…indeterministic mechanists … regard determinate law as reducible completely and perfectly to an approximate and purely passive reflection of the probabilistic relationships associated with the laws of chance" (64).

As it stands, we must stress that the change in experimental study of any aspect of nature or any phenomenon leads ultimately to a change in theory (or explanatory perception). In 1784, for instance, Martinus van Marum used the large electrostatic generator with its 24-inch discharge screen to experimentally demonstrate or confirm Franklin's electric *fluid theory* or the *current theory* of electricity. But recent conception of electricity as comprising particles or electrons in motion tends to refute the fluid theory. The only legacy of the fluid theory to the recent conception of electricity is the word "current". In any case, it needs to be radically made clear that the fluid/current conception is not false, nor any technological instrument(s) the fluid theory brought about a mere mirage. The controversy could be likened to that of wave and particle theory of light. At least in quantum optics both aspects of light have substantial experimental supports. So, it is a question of which ***aspect*** one is studying. Louis de Broglie gives us a way to think deeper in this direction with respect to *matter* as such. He was influenced by the fact that radiation (light energy) has a dual nature (corpuscular/particle and wave/fluid). As a matter of fact, "De Broglie came to the important conclusion that a kind of wave is associated with material particles," which led to the "experimental confirmation of matter waves" – a feat achieved by Otto Stern in 1923 after inventing the molecular beam apparatus with his associate, H. Gerlach (Pandey and Tripathi 118-120). The moral of this tale is that Franklin's fluid theory of electricity can be sustained at all costs. This is precisely the case because, among other features, the wave aspect of matter presents a substantive impression of fluidity for particles in motion. Following de Broglie's achievements in the 20th century, scientists must be forced to re-accept the results of van Marum's experiment on the current theory of electricity without any tincture of doubt.

Thus far, we need to cite yet another tractable scientific theory. We need to answer an important worry bugging the mind of many a scholar as regards why the Ptolemaic theory had strong predictive powers, whereas the Copernican theory takes it to be false. Say what one may, it is historically evident that apart from handling the anomaly of the equinoxes, the Copernican theory is no more accurate than the Ptolemaic theory. The answer truly lies in the word "aspect". The Ptolemaic astronomical calculations were done from the point-of-view of the earth, which remains the Earth-bound astronomer's vantage point, or more accurately, his ***reference frame***. Even though it is considered complex by virtue of the fact that it contains more epicycles as auxiliary explanations, it worked perfectly within the limits of the then known planetary system. Given the facts on ground, the theory *was* true (and is *still* being used today in some engineering calculations). In fact, the Popperian *verisimilitude* (truth-likeness or nearness to truth) bears this out.

What, then, is truth? Truth in the traditional sense is *adequatio rei ad intellectum.* Simply put, this predominantly medieval conception of truth stands for "correspondence of theory to facts" – some form of logical isomorphism between propositions and facts. But considering the fate Ptolemaic geocentricism suffered, the undiluted Copernican heliocentric theory cannot as well be said to be the ultimate truth. As is evident, the sun is not the "fixed" real centre of our planetary system or the universe, since it moves round a central void just as the early Pythagoreans, Philolaus and Aristarchus, submitted. We keep on discovering more planets and even more galaxies. Therefore, one might clearly state that both geocentricism and heliocentricism are true by approximation. Nevertheless, more forays into this argument might lead to an elaborate or extravagant semantic excursion for which we are currently ill-prepared. In short, given that the universe is infinite, we as human beings possess the right to determine or fix the radius of the centre of our own planetary system or the universe at large. And the Earth might fall within that radius, hence the relative predictive excellence of the Ptolemaic/Aristotelian astronomy. This is not to say that all the auxiliary hypotheses or embellishments of geocentricism are all true or unquestionable. Dicken, for instance, writes:

> Aristotle believed that the outer planets were affixed to great crystalline spheres revolving slowly around the Earth. Roughly 2,000 years later, they were liberated by Newton and placed orbiting the Sun, held in place by a mysterious gravitational attraction like some kind of cosmic centrifuge. Neither account, however, is currently held to be true. According to Einstein, the planets are merely constrained by the deformation of space-time itself, and would wander off into the blackness of space if it wasn't for the vast astronomical trough at the centre of our solar system (2).

Be that as it may, we need to make a categorical statement that, scientific *truth* is not the God's-Eye-View stuff. It falls short of the *absolute*, *global*, *omniscient* or *heroic model* of truth. Each time anyone mentions truth in science, it is always in reference to some sort of pragmatic and humano-ontic correspondence. With this disposition, we think that what could be tagged as "immodest Leibnizian" hope of one *divine best* description of our universe and its components has been somewhat curbed. The fact remains that science is evolutionary, and we keep updating our knowledge by standing on different aspects of reality. In fine, this very stance could be rendered in the following Latin expression: *Scientia mundum perspectivas navigat* (i.e., Science navigates the world in perspectives).

Admittedly, this view smacks of implicit relativism. But, then, our own version of relativism differs from that of Kuhn and the social constructivist Bruno Latour. It is not antithetical to a realistic view of the world. It does not synchronize, for instance, with Kuhn's idea that two scientists using different paradigms see different things when they view a thing from the same angle. There is always an objective inter-paradigmatic situation whereby essential facts are properly demarcated, isolated, and tabulated by two different scientists belonging to Kuhn's so-called different paradigms. The world does not always posit itself in the rabbit-and-duck fashion Kuhn used in his Gestalt-Switch example. In fact, we see in Bird a more compatible ally because in scientific investigations, "…seeing the answer is not typically just like recognizing a face or suddenly seeing the duck-rabbit as a duck. On the contrary, solving such (scientific) puzzles involves cogitation, the extended process of thinking and reasoning" (106). As such, one should object to whatever picture Kuhn must have painted with the Gestalt-Switch brush. Thus, we could glean from Panos Theodorou a complete psychologised rendering of paradigm switch:

> We jump into some paradigmatic experience not by means of an interpretation (in 'some ordinary sense of the term' ...) …, but by a non following-a-rule-like process, which transforms or organizes the stimuli into appearing beings which are simply seen. Such a process is presented as the result of a kind of a non-algorithmisable programming and re-programming of our neural constitution. The whole process is a one-step move from non-seen stimuli to seen beings constituted and allowed in the one or the other paradigm (190-191).

Moreover, our view differs from the social constructivist (ethno- methodological) inkling of Latour's, wherein culture and social frame of mind determine our perception of objects. we must then borrow from Putnam (in his dialogue with Bryan Magee) who says that given the fact that human conception determines knowledge does not affect the fact that a policeman, for instance, is objectively standing by the corner, if he is actually standing there. To perceive him as standing there *waiting for a friend*, or standing there *to track down criminals*, or standing there *to help and direct* is only a matter of interest as to what *aspect* of **standing** he is doing. That we are investigating any of these aspects does not expunge the reality of the standing. So, different aspects and behaviour of nature, entities and phenomena can be studied in science, and complex combinations of experimental manipulations may yield novel behavioural manifestations of matter and force(s). Even though we do not share the same vision with Patrick Heelan's peculiar conception of the "prescientific" (1983), but fortunately we see that he has some grasp of our idea of scientific investigations when he proposed "horizontal realism", which "…interpreted successful scientific theories as introducing new perceptual discrimination against the background of prescientific perception, and new horizons for revealing aspects of reality not available prescientifically" (Rouse 78). So, most times, scientific revolution is like the revelation or manifestation of more hidden treasures in nature. In the process of unfolding these treasures, it may not be completely reasonable to for one to use the term "incommensurable", as Kuhn has done in his picture of theory change. If indeed two theories are considered incommensurable, then there is no basis for making any choice between. A common ground must be shared by two competing theories before any scientific community begins to make any choice at all or en-thrown a new paradigm. We can only allow the use of such term only within the context of points of difference or contrast between two competing theories. As such, Kuhn's so-called Incommensurability Thesis does not affect the cumulative stature and successes gained in scientific theorization and investigation of nature.

CONCLUSION

Following all we have said so far, it is so glaring that quite a dozen numbers of things could be said in the realm of scientific theory and scientific revolution. Yet, the corrective light we have shed so far does not permit us to make any generalization or universal statement about scientific theories and attendant revolutions in science. Any attempt to make such a universal statement prevents one from giving exhaustive account of the nature of science, or rather the practices within the scientific enterprise. So many philosophers of science have fallen prey to presenting some such universal phantasmagoric or surrealistic pictures of science that cannot take-off from any existing grounds of science. To be sure, mixing the *ought* with the *is* can only drown one in the deep pond of Naturalistic Fallacy. The intricate and pragmatic nature of science take its far away from any insinuations of what science *ought* to be.

Science will not eventually cease to be science if we stopped believing, for instance, that the universe is run by a single phenomenological law. Science will continue to be an intricate epistemic phenomenon that transcends language, logic, mathematical generalization, assignation of God's-Eye-View of reality, and so forth. It remains a *humanly* rational enterprise of a pragmatic sort that attempts to give us the correct picture of the universe within our given human and historic frame of understanding, representation, experimental manipulation and ability to transform the world, technologically. This can only be achieved if there is a level of consistency. As such, the cumulative structure of science, long advanced by Aristotle and others, cannot be denigrated or overridden in any form by perennial revolutions in science. *Pace* Kuhn, a studied attention to the history of science from antiquity affords a clear demonstration of the cumulative or continuity character of science. With this said, we believe that we have achieved an unsung task of this paper, which is to loosen the grips the fear of perennial scientific revolution has on most scientific realists. Of course, this particular fear made some scholars who would have ordinarily stayed in the realist camp to flee to the seemingly protective zone of antirealism or non-realism and, by so doing, passing the buck or casting the burden of proof on the realists.

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