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ON THE ESSENTIAL INSTABILITY THESIS OF SCIENCE

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ABSTRACT

The hitherto philosophers of science assumed that justification had a thoroughgoing logic and therefore a philosophical account of discovery must be in terms of a logic of discovery. However the recognition that it is impossible to provide an algorithmic or semi-algorithmic scheme and the recognition that justification itself if viewed more in terms of historical authenticity is un-amenable to a logical articulation and more than that, the realization that discovery has a philosophical relevance independent of its relation to justification have replaced the "logic of discovery" by the "methodology of discovery". This paper begins with the category shift from the idea of an unchanging set of principles that is supposed to underlie and persist through the changing content of science to the idea of radical instability of science, according to which nothing remains stable in the shifts that scientific theorizing undergoes. Thus, it is a categorical shift from the idea of an essentially stable core of science to the idea of an essential instability of science. This replacement of stability by instability is one of the hallmarks of post-Positivist philosophy of science. An attempt is made in this paper to discuss the views of Dudley Shapere who very effectively enunciates the thesis of essential instability of science.

Keywords: *Scientific Discovery; Categorial Shift; Essential Instability; Logic of Justification and Incommensurability;*

INTRODUCTION

Contemporary debates have greatly effected a categorial re-orientation to the discourse on discovery by means of 'categorial shift' that consists in the replacement of older categories by newer ones in understanding the phenomenon of discovery and consequently the nature of scientific activity as a whole. It is to this that the following discussion is devoted. The first shift is apparent in the very character of a philosophical account of discovery. Since the time of Bacon and Newton, all those who took discovery seriously as an object of philosophical study, attempted to give a logic of discovery. In their eyes an adequate philosophical account of discovery must be provided in algorithmic or at least semi-algorithmic scheme. Be it an inductive logic of discovery as in the case of Bacon, Newton and Mill or retroductive logic in the case of Peirce and Hanson, the aspiration was the same. If for the inductivists the inductive logic of discovery was simultaneously an inductive logic of justification, for a retroductivist like Peirce and Hanson the abductive' logic was very close to the hypothetico-deductive logic of justification. Though this may be taken with a pinch of salt in the case of Peirce, this is absolutely true in the case of Hanson, who maintained that the logic of justification was hypothetico- deductive and that that logic was structurally the same as the retroductive logic.

In other words, it was assumed that justification had a thoroughgoing logic and therefore a philosophical account of discovery must be in terms of a logic of discovery. However the recognition that it is impossible to provide an algorithmic or semi-algorithmic scheme and the recognition that justification itself if viewed more in terms of historical authenticity is unamenable to a logical articulation and more than that, the realization that discovery has a philosophical relevance independent of its relation to justification have replaced the "logic of discovery" by the "methodology of discovery", thereby upsetting the traditional equation between logic and methodology. After all the fact that even if the process of discovery or justification conforms to logic, it in no way establishes the existence of *a logic of discovery or justification*. Further as Nickles points out, "whatever conformity to logic may contribute to the rationality of an enterprise, logic alone does not give it epistemic import. For the logic may take one from unwarranted premises to unwarranted conclusions"(Nickles (1985,p.181). The categorial shift from logic to methodology can be supplemented by a variety of similar categorial replacements.

1. BEYOND THE PRESUPPOSITIONISM AND RELATIVISM

Dudley Shapere begins by observing that the traditional approach to knowledge seeking enterprise assumed that "there is something which is presupposed by the knowledge acquiring enterprise, but which is itself immune to revision or rejection in the light of any new knowledge or beliefs acquired"(Shapere,1980 p.61). In explaining this Sudhakar Venukapalli (1989,p33) says,

"Such immunized or unrevisable epistemic entities may be certain general claims about the world or a method or certain rules of inference or certain concepts. Different versions of the traditional approach differ with regard to the nature of what is considered to be unrevisable and immune to change. But what is common to all of them is the view that without presupposing something which is essentially stable, our knowledge-acquiring enterprise cannot proceed at all"

Shapere calls this view presuppositionist view of science. This view has as its adherents a galaxy of philosophers ranging from Plato to logical empiricists. It has, no doubt, encountered serious challenges in the recent times, the most important being the one from historians of science and historically-minded philosophers of science whose claim that scientific change involves not only a shift in the substantive beliefs of science but also alterations in world-view, method, rules of reasoning and concepts, such that scientific change is total, has led to the emergence of relativism. We have now been caught in a dilemma between presuppositionism and relativism. Is there a way out? Such a way out is necessary, according to Shapere, if at all we should tackle the fundamental "issue of how standards of rationality could be held to undergo rational change"(Shapere, 1980,p.64).

The pre-suppositionist view questionably denies the very fact of our standards of rationality themselves undergoing change while relativism, while acknowledging the change, negates the rationality of such a change. Hence, to the extent that this issue is important both pre-suppositionism and relativism are to be eschewed precisely because neither of the two views can even allow the issue to take off. Apart from the common failure to come to grip with this problem, as Shapere later shows, there is something fundamental, which these apparently antagonistic points of view share.

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The presuppositionist view is absolutely questionable because the history of science abounds with examples of scientific change wherein a change in the substantive belief about the world that is a theory replacement is accompanied by a change in the criteria concerning what is scientific and what is not, what is a legitimate problem and what is not, what is a scientific explanation and what is not, and even what is observable and what is not. Shapere substantiates this point with a host of illustrations, the most important being the change in our conception of matter from Aristotelian to classical physics to quantum theory - a process marked by not only a shift in our conception of the constituents of matter but also concerning the questions and possible answers concerning the nature of material substances.

Under the shadow of the Aristotelian theory of matter the nature of material substances was sought to be understood in terms of the perfect or harmonious form exemplified by one earthly substance. In contrast, "chemical analysis as the segregation of substances and their breakdown into their constituents" (Shapere 1980,p 69) became the central task of post-Aristotelian science. Thus, for Shapere (1980, p. 69)

"the central problem in the attempt to deal with matter shifted from, how can earth be brought to perfection? to, what are the constituents of material substances? A reform of the nomenclature of material substances was inaugurated also: the language of chemistry was reformed so as to make the name of the substances correspond to their composition and structure".

However, the compositional conception received fundamental modification with quantum theory and quantum field theoretic approaches. The unstable nature of our concepts of matter has rendered the very idea of 'compositional' and 'constituent' unstable, in the sense the relationship between constituency and separability, which was a straight forward 'if and only if' in classical physics has become vastly more complicated. So it is clear that the problem concerning the nature of different material substances are construed in different ways and what constitutes a legitimate solution changes as we shift our conception of matter.

In fact our very idea of 'observational' is subject to change in accordance with the existing character of scientific knowledge. Shapere, in this connection gives the example of the way in which contemporary Astrophysicists speak of the direct observation of the center of the sun. What is involved in such a talk is knowledge of the behavior of the neutrino the process of their emission, their weak inter-action, etc. This makes it clear how the specification of what counts as observed or observable is the function of the current state of the physical knowledge, and can change with the changes in that knowledge (Shapere,1980,p.71). What is observable is specified by current physical knowledge by specifying what counts as an 'appropriate receptor', the ways in which information of various types is transmitted and received, the character of interference, and the circumstances under which and the statistical frequency with which it occurs (Shapere,1980, p.71) Apart from the talk of appropriate receptor the concept of observation is also specified in terms of our knowledge of environmental conditions. Unless we forget that in science by observation we do not mean what meets the eye-ball, it is not difficult to accept Shapere's contention that observation and observability are specific to a given stage of physical knowledge. Not only is there no way of defining what is observable independent of the available physical knowledge of the time, but also the question whether observability is important or primary does not have 'a once and for all' answer.

In other words, whether observability is an essential criterion for deciding the acceptance or otherwise of a theory has to be decided by the available physical knowledge. For example, the existence of Quarks was accepted even though the attempts to observe them

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failed, and the unobservability was explained away in terms of the color, introduced originally to make Quark theory consistent with Exclusion principle (Shapere,1980,p.72) In short, even 'observability' does not have an axiomatic character in the scheme of science. This is also the case with other considerations like verifiability, conformability, falsifiability, etc. Undoubtedly in science there are a great variety of such general types of considerations (Shapere,1980,p.75). A single sort of considerations does not govern Science. Those considerations interplay and constitute a network. At any given time some considerations become more important than others. It may even be that a consideration, which might be stripped of its primacy, may regain it. What is important to note is that there is no fixed hierarchy among these considerations that stands above the flux that characterizes the substantive beliefs exemplified by theories.

In short, the distinction between scientific and meta-scientific, the latter allegedly enjoying unlike the former, stability, and thus constituting, philosophically speaking, the essence of scientific knowledge is absolutely unwarranted. The distinction between them is possible only in the abstract and in the actual practice they are inseparable. Those who accept the distinction have an essentialist understanding of meaning that fails to appreciate the relation between meaning and use so tellingly pointed out by Wittgenstein.

Words like 'criteria', 'observation', 'observability', 'rules', 'evidence', etc., have meaning only in so far as and is determined by the contexts of their use and these contexts are characterized by and in fact constituted by the character of the substantive beliefs entertained by science at a time. The context-boundless of the content of the so called meta-scientific concepts sets at naught the distinction between the so called first level concepts of science and second level concepts about science.

The rationality of scientific enterprise consists, therefore, not in its success in meeting changeless and abiding criteria that follow from a conception of knowledge or science established a priori but because the products of science, the substantive claims about the world, shape as they develop the so called meta-scientific views, criteria, rules and concepts which decide the issues concerning legitimacy of a problem, canons of acceptability of solutions, genuineness of explanation and scientificity of a point of view. For Sudhakar (1989), “it is this generation and shaping of the meta-scientific considerations by reforming the prevailing network of considerations that a set of substantive beliefs win credence of rationality”. It may be objected that if the standards of the rational acceptance are shaped by the very beliefs the rationality of whose acceptance is rooted in the former, than the concept of scientific rationality involves circularity. But this presupposes that a scientific theory and the standards of rationality it sets have nothing to do with the previous theory and the canons of rationality it had shaped. But this need not be so.

Denial of continuity via time-less and abiding standards does not mean denial of any relation. In fact, the traditional construal of rationality that is parasitic upon an abiding and unchanging set of standards fails to do justice to the fact that science creates itself anew with every radical shift. It may be interesting to note that both traditional presuppositionism and contemporary relativism equate the possibility of rationality with the possibility of prior and timeless stands of rationality. Presuppositionism affirms the latter and accepts the former. Relativism denies the latter and therefore, denies the former. But the third alternative is the one, which refuses to relate the former with the latter.

2. SCIENTIFIC PRACTICE: THEORY TO DOMAIN

Apart from the categorial shift from 'stability' to 'instability', Shapere has introduced another categorial shift from 'theory' to 'domain' as a unit of scientific practice and as the corner stone of an adequate philosophical construal of science. It may be noted that the traditional empiricist philosophy looked upon theory as the ultimate unit of scientific practice. Given the construal of theory as observationally determined and observation-pervaded a theory-centered construal of science very much suited the empiricist plans, programmes and procedures in philosophy of science and was very much consistent with the epistemological commitments of empiricism. Similarly, the theory-centered construal of science, albeit with a non-empiricist construal of theory, very much suited the purposes of the Hypothetico-deductivist school of Popperians. Looking at both of them from hindsight, one wonders whether they did not have something fundamentally common as evidenced by the fact that both of them shared a theory-centered construal of science according to which the ultimate unit of scientific practice is a theory and science is fundamentally nothing more than a collection of perhaps inter-related theories however much the Empiricists and Popperians may differ from each other with regard to the nature of scientific theories and their relation to observations. This example is sufficient to show that the question, "what is the unit of scientific practice"?, is determined by and in turn_ determines our whole .approach to science as an epistemic activity. This is as much true of post-Popperian philosophy of science as Popperian and pre-Popperian philosophy of science. This is obvious from the fact that Kuhn adopted paradigms as units of scientific practice in accordance with his general conception of scientific knowledge. As Newton-Smith points out "The positive and salutory virtue of Kuhn's use of his notion of a paradigm is to remind us that in looking at the scientific enterprise it is important to focus on more than the theories (in the narrow sense of the term) advocated within a given community". The same is the case with Lakatos who replaces 'theory' by 'research programmes' as the unit for methodological appraisal, and Shapere who replaces 'theory' by 'domain'.

The intention of above argument is not to say that these philosophers of science would refuse to talk about theories or in terms of theories. When they replace 'theory' by something else what they mean is that to consider theories as fundamental constituents of science is to miss something very significant about science. That something is the sociological dimension of science according to Kuhn and a methodological point inaccessible to any school of methodology including hypothetico-deductivism at least as commonly understood, according to Lakatos. In short, the dissatisfaction with a theory-centered construal of science and the felt need for a shift from theory to something else as the fundamental unit of scientific practice were engendered by the perception. of something that was deemed to be an essential aspect of science but was missed by a theory-centered view of science. It is this perception, which were both the cause and the effect of the post-positivist developments in philosophy of science. Shapere's attempt to replace theory by domain must be viewed in this context.

According to Matti Sintonen and Mika Kiikeri (2004,p.228), "Dudley Shapere in turn proposed that the organizing principles in what he called scientific domains enable and suggest certain styles of questions and provide, at the same time, constraints on admissible of intelligible answer. In fact, he maintained, theories can be regarded as answers to questions arising from such domains".

What is it which Shapere finds to be an essential feature of science as a knowledge-acquiring enterprise and which a theory-centered construal of science fails to highlight and account for? It is that the views of nature, which constitute the body of science "have, over

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the course of the history of science, become increasingly coherent, in the sense both of constituting a more and more unified perspective on a larger and larger body of detailed beliefs, and of providing an intelligible picture of the world we experience"(1980, p. 61). At any given stage of the development of a relatively sophisticated area of science, say electricity, magnetism, light, chemistry, etc., one finds invariably a certain body of information which is taken to be at that stage an object of investigation. "Further, those general subjects are in many cases considered to be related in certain ways"(Shapere, 1972,p.407). Shapere here has in mind the fact that in the 19th century it was believed for a variety of reasons, "that electricity, magnetism, chemistry and light were related in a manner it was thought reasonable to search for a common account of all these subjects" (Shapere, 1972, p 407). Shapere (1974) refers to such bodies of related items as 'domains'. A body of information that is investigated into constitutes a domain. Further, a domain is not a discipline - specific but somewhat inter-disciplinary or it is characterized by 'inter-field-connections'.

The progress of science is always marked by the widening of a domain in such a way as to make an existing domain part of a wider domain. It is this domain-widening which Shapere considers to be the essential feature of science as a cognitive enterprise and consequently the rationale behind a discovery. In this connection Shapere takes the example of the periodic table of chemical elements (Shapere, 1972,p.407), which was developed in the last third of the 19th century. By early 1870's it became clear that if chemical elements were arranged in terms of atomic weights in a table and if some space was left for undiscovered elements, then certain periodicities in the properties of the elements (Shapere, 1984, p.288) would come to light. Most of the working scientists especially chemists for whom atoms of the physicists were a matter of speculation did not feel the need for any explanatory theory for the extensive, detailed and precise relationships revealed by the periodic table. However, slowly the conviction grew that a deeper explanation was to be given for this periodic table and "in particular, since the fundamental ordering factor, the atomic weight, increased by discrete 'jumps' rather than by continuous or irregular gradations, that deeper explanation was expected to be in terms of discrete components. Thus that composition was to be understood in terms of constituent massive particles"(1974, p.408). This expectation became a demand due to three important reasons, namely,

1. more and more areas began to appear as domain in which an atomistic explanation was expected (for example, the case of chemical spectroscopy)
2. atomic explanations became more successful in other domains like statistical mechanics and,
3. reasons accumulated for believing that the domain under consideration (the Chemical elements related through periodic tables) to be itself related as part of a larger domain to others including ones which were lending themselves to atomistic explanation (Shapere,1972, pp.407-408).

The general import of this episode, for Shapere, is the fact that a scientific explanation for a domain, whose realization constitutes a discovery, is partly a reason for and partly a consequence of a sustained effort at increasing the domain of investigation by making the domain under consideration part of a wider domain. So the idea of scientific progress and consequently the idea of discovery should be treated as inseparable from the idea of domain and domain-expansion. The traditional empiricist philosophers of science like Nagel soft-pedaled the notion of domain and domain-expansion by relating scientific progress to the idea of reducing one theory to another theory. The construal of scientific change in terms of

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reduction only highlights the logical aspect of theoretical progress and blinds us to the fact that what is changed is the very idea of the object of investigation. Further, it misleads us into believing that the theory shift was for a logical purpose and was guided by logical considerations. But in actual practice the attempt at a wider theoretical explanation is hardly if ever motivated by logical considerations. Nor are there at any time logically conclusive reasons for the wider theoretical explanation. Going back to the case of the periodic table Shapere points out that the three reasons given in favor of an atomistic explanation could be countered without being unfair to facts or canons of logic (Shapere, 1974, p.408-409).

Hence theory change is not simply a change in theory. Giving wider scientific explanations is not simply mapping one theory into another. After all, theory change and widening the explanatory mechanism occurs over a domain apart from which they stand without flesh and blood. Hence a theory change must be understood only in association with the change in domain. As Nickles points out “Shapere's 'domain' of information and Darden and Maull's technical concept of a scientific 'field', ...(permit) them to argue that many issues formerly discussed under the rubric of 'reduction' are better treated in terms of inter field relations"(Nickles,1980,p.44).

3. KUHN AND SHAPERE ON SCIENTIFIC CHANGE

Thomas Kuhn already anticipated many of the central ideas of Shapere, especially those concerning his category shifts from the essential stability to instability as well as from theory to domain, in his magnum opus, ***The Structure of Scientific Revolutions***. Two ideas of Kuhn are of particular relevance here. His idea of a 'paradigm' is very much close to what Shapere calls 'domain'. In fact one cannot be understood or at least explicated without the other. A paradigm is what covers a domain and is recognizable, among other things, in terms of the domain it covers. The scientific activity which Kuhn calls 'Normal science' and which is carried on under the umbrella of the paradigm purports to bring about a quantitative change by means of an extension of the domain that falls under the concerned paradigm. This is because Normal science aims to increase the precision and extend the scope of the existing theory by attempting *"to adjust existing theory or existing observation in order to bring the two into closer and closer agreement, ... (to extend the) existing theory to areas that it is expected to cover but in which it has never before been tried....(and) to collect the concrete data (e.g., atomic weights, nuclear moments),required for the application and extension of existing theory”*(Kuhn,1977,p.233).

On the other hand, a change in the paradigm brings about a qualitative change in the domain by means of a radical readjustment and re-description of the domain thus engendering a domain-upheaval. In short, both the paradigm stabilization and paradigm shift are to be understood in terms of their impact upon the domain that is covered by a certain paradigm. Secondly, Shapere's contention that the so called meta-scientific considerations which were supposed traditionally to remain constant and fixed amidst the flux that characterizes substantive beliefs of science (i.e. theories) are shaped by and hence contingent upon the latter such that a scientific change is a total change is very much anticipated by Kuhn. In fact this is the crux of his central thesis of 'incommensurability'.

According to Kuhn any two successive paradigms are incommensurable with each other in the sense that the criteria on the basis of which we 'prove' the superiority of new paradigm over the old one are too complex and too unique to the situation at hand to be expressed in the neutral idiom of logic and laboratory operations. When a paradigm changes everything changes including the world, the methods of understanding it, the criteria of legitimacy of problems and solutions, and more particularly the meanings of the central terms of the paradigm. All this does not mean that the views of Shapere and Kuhn are identical or that they are sympathetic with each other. What is pointed out here is that there views against theory-centered construal of science and what Shapere calls pre-suppositionist or transcendentalist view of science is sufficiently similar to group them together in some ways.

CONCLUSIONS

The most important light that Shapere has thrown on discovery through his categorial shift from the essential stability to essential instability of science consists in laying bare the truth that the more fundamental a discovery, the more total it is, in the sense that it not only recreates the world but also recreates our ways of looking at it. It gives a new answer to the question what the world is like and also to the question how to understand it. (Shapere,1972, p.418)This Shapere does by considering standards of rationality as immanent to the changing substantive beliefs, which science holds in the form of its theories and consequently by denying the transcendental character of the methodological canons. In doing so he joins Kuhn, Wartofsky, Toulmin and others in setting in motion the process of what Rorty (1978) calls 'de- transcendentalization'.

BIBLIOGRAPHY:

[1] Abimbola, Kola. *"Rationality And Methodological Change: Dudley Shapere's Conception Of Scientific Development".* In Principia, **10**(1) Brazil: Federal University of Santa Catarina (UFSC).2006, pp. 39–65.
[2] Kuhn, Thomas S. The Essential Tension: Selected Studies in Scientific Tradition and Change, Chicago: University of Chicago Press. 1977, p 233.
[3] Newton-Smith, W.H. The Rationality of Science, London: Routledge and Kegan Paul.1981, p 106.
[4] Nickles, Thomas. *'Beyond Divorce: Current Status of the Discovery Debate'*, Philosophy of Science, Vol.52**,**1985, pp. 117-206.
[5] Rorty, R. *"Epistemological Behaviorism and the De-Transcendentalization of Analytic Philosophy".* Hermeneutics and Praxis. 1978, pp. 95-96.
[6] Shapere, Dudley. *"Notes Toward a Post-Positivistic Interpretation of Science".* In P. Achinstein and S. Barker (eds.), The Legacy of Logical Positivism. Baltimore: Johns Hopkins University Press), 1969, pp. 115–160.
[7] Shapere, Dudley. *"Discovery, Rationality, and Progress in Science".* In K. Schaffner and R.S. Cohen (eds.)., PSA 1972, Dordrecht, Holland: D. Reidel, 1974, p 407-419.
[8] Shapere, Dudley. *"Scientific Theories and Their Domains".* In F. Suppe (ed.), The Structure of Scientific Theories, 2nd Edition, University of Illinois Press, Urbana, Chicago, London. 1977.
[9] Shapere, Dudley. *"The Character of Scientific Change".* In T. Nickles (ed.), Scientific Discovery, Logic and Rationality**,** Dordrecht, Holland: D. Reidel. 1980.
[10] Shapere, Dudley. *"The Concept of Observation in Science and Philosophy".* Philosophy of Science 49, 1982, 485-525.
[11] Shapere, Dudley. Reason and the search for Knowledge, Boston: D Reidal., 1984.
[12] Shapere, Dudley. *"External and Internal Factors in the Development of Science".* Science & Technology Studies 4, 1986, pp.1-9.
[13] Sintonen, Matti and Mika Kiikeri: *"Scientific Discovery".* In Ilkka Niiniluoto, Matti Sintonen and Jan Wolenski (ed.), Handbook of Epistemology, NewYork: Springer,2004.
[14] Sudhakar V. *"Are Scientific Theories Incommensurable?",* Social Scientist,Vol.17, No.3/4, 1989, PP.31-50.