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REVIEW

Understanding scientific rationality through scientific discovery

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ABSTRACT

Philosophy of science and history of science were not so closely related in the past precisely because the problem of discovery was either thrown away or made an appendage to the problem of justification. This paper makes an attempt to explicate the complex interaction between the problem of discovery and history and philosophy of science, including its bearing on scientific practice. Post-positivist and anti-positivist critical discourses in philosophy of science totally disturbed the orthodox and established disciplines of knowledge and radically transformed our understanding about science, scientific knowledge and the context of scientific discovery. Understanding the significance of the problem of discovery to history and philosophy of science enables us to arrive at an adequate theory of science as a cognitive inquiry and creative human endeavor. The central argument in this paper is to drive home the point that the contemporary debates on problem of discovery can shed new light on the central issues of history and philosophy of science, and most importantly on the nature of scientific rationality.

KEYWORDS: Scientific discovery, Scientific rationality, Context of discovery, Context of justification, Scientific explanation

The re-orientation which philosophy of science underwent in the beginning of the second half of twentieth century was both cause and effect of a renewed interest in the problem of discovery (Schickore J and F Steinle, 2006). As Thomas Nickles (1980, 2017) points out,

"The most important, general reason for the revival of interest in discovery surely has been philosophers' increased concern in the past two decades with real science and historical cases. Critical reaction to Arthur Koestler's The Sleepwalkers, published in 1959, a year after the appearance of Norwood Russell Hanson's Patterns of Discovery, helped to pique philosophers' interest both in the history of science and in the discovery process."

The subsequent works of Thomas Kuhn (1970, 1979) and Paul Feyerabend (1987, 1988, 1991) further promoted the interest in discovery. However it should be noted that the interest in discovery was not simply the result of the historic-sociological orientation of philosophy of science, but also the cause of the latter.

The interest in the problem of discovery is replacing the very central terms in which philosophy of science has been seeking to provide the account of science and its rationale. Wartofsky's (1980) words in this connection are only an index of the direction in which such a change is taking place. According to him the philosophical consideration of discovery has necessitated

"a fundamental category shift. Because discovery in science is an epistemological question, i.e., one concerning scientific knowledge, and indeed, the genesis of such knowledge, the shift that is needed is one in the epistemological categories in terms of which philosophy of science studies or understands scientific thought" (Wartofsky,1980).

According to Wartofsky, in dealing with the question of scientific creativity which is central to the problem of discovery, we must invoke a neglected epistemological category, namely 'Scientific judgment' which must replace the dominant epistemological category in the contemporary philosophy of science, namely, *scientific explanation*. In the same vein, Finocchiaro (1980) attempts to replace knowledge by understanding as the central category in understanding science. In this connection he says*:*

"It (our understanding of scientific discovery) is one which would replace knowledge and search for truth as the key

> *explanatory concepts by those of understanding and problem-solving"* (Finocchiaro,1980).

The most important point about the contentions of Wartofsky and Finocchiaro is the contrast between explanation and judgment in the case of the former and knowledge and understanding in case of the latter. This crucial distinction is reminiscent of Von Wright's (1971) contrast between explanation and understanding. Such a category-shift will immediately distance our construal of scientific thought from not only the customary view of science as made up of deductively structured theories with their progressive approximation to truth but also the time-honored view of knowledge-claims including scientific knowledge-claims as true justified beliefs.

The problem of scientific rationality has been the central issue in philosophy of science. The positivists attempted to construe and defend the idea of scientific rationality in terms of their notions of pure observation and quasi-deductive ways in which the theoretical superstructure of science was supposed to be related to the indubitable observational basis. Popper gave a better theory of scientific rationality in terms of his ideas of criticism and falsification. It must be noted that these ideas of scientific rationality failed to take into account many factors that play an active role in actual scientific practice. The reasons for this poverty were the rejection of the phenomenon of discovery as a philosophically irrelevant aspect of scientific thought. The most important of such factors are the supposedly extra-scientific considerations that pertain to certain metaphysical ideas.

It may be noted that such a notion of rationality is basically ahistorical. This is obvious since both positivists and Popper construed a philosophical account of science in terms of a set of logically articulable logical rules. Anything that is not available or amenable to such a logistics articulation, was considered to be non-rational and hence irrelevant to a philosophical understanding of science. They were considered to be at best external factors as distinct from internal ones, which are intrinsically tailored to the terms of representation allowed by the program of logical reconstruction. As Stefan Amsterdamski (1975) so aptly puts it,

"if, however, we treat science as a part of the intellectual culture of a given historical period, then the division of history into 'internal' and 'external' appears totally artificial and arbitrary. The essence of the development of science is then determined not only by its logic, but also by elements and factors, which lie outside the sphere of logic. What…. appears as pathological, unnatural and irrational from such a perspective appears as natural. Metaphysics, myths or superstitions are in some manner as immanently a part of science as the facts which we attempt to include into the rational reconstruction."

In this context, it is imperative to recall Isaac Newton, whose commitment to the Cartesian metaphysics compelled him to reject the idea of gravity as an ultimate principle not needing further explanation. It may also be noted that Newton opposed Huygen's wave theory of light and insisted upon the acceptance of corpuscular theory because of his metaphysical commitment to atomism. It was not, nor did Newton believe, that the corpuscular theory had better empirical support. After all, when Robert Young in 1801 successfully replaced Newton's theory by a somewhat altered version of Huygen's theory he confessed that he did not possess any more experimental evidence than the ones Newton himself worked out. The fact that it is the same empirical basis which was invoked to support the two divergent theses shows that Newton's espousal of his theory was rooted in something more than empirical i.e., evidential matters.

Some more examples can further strengthen Amsterdamski's view. In his remarkable essay, 'The Legitimation of Scientific belief: Theory Justification by Copernicus', Bruce Wrightsman convincingly shows the role of Copernicus' theological commitments in the formulation of his ideas and the basis for its acceptance, at least in the eyes of Copernicus. Commenting on Copernicus' idea of the Sun, the lamp of the universe, as the fittest entity to occupy the center of 'this most beautiful temple', an idea that has close association with the idea of God in Judaic-Christian tradition, Wrightsman (1980) writes:

"In the course of his argumentation, a definite pattern of reasoning emerges which renders such epitaphs for discovery as 'irrational' and 'subjective' patently

> *inappropriate. One may not like Copernicus's reasons for coming to believe in and justifying his system but that is not a rational ground for refusing to accept them as reasons. We must therefore remind ourselves that scientific investigation had much broader implications for Copernicus than it has for many today and included those purposes which we classify as religious and extra-scientific. Such considerations, however, were crucial for Copernicus and were demonstrably instrumental for his achievement."*

In a similar way, Michael Ruse explains the content of Darwinian theory in terms of Darwin's acceptance of the Newtonian paradigm as the methodological ideal and teleology as a basic fact of the life-world. The former gets expressed in the way in which Darwin casts his arguments in support of Natural Selection as well as the very structure of the *Origin of Species* which has *"a fan like form, with core of selection explaining in so many different areas: biogeography, paleontology, embryology, and so forth"* (Ruse,1980). All this is in consonance with the 'consilience of inductions' so well elaborated by Herschel and Whewell as an admirable quality of Newton's theory. Darwin's belief in the teleological world-view was undoubtedly a metaphysical belief since the belief in teleology however widely shared was not an altogether obvious and self-evident empirical fact. In this context, Michael Ruse (1980) says,

> *"The concern with ends was no less an obsession for Darwin than it was for his opponents…. Both in his Newtonianism and his Teleology, Darwin let his way of discovery influence the theory he produced….In these two modes of Newtonianism and Teleology…Darwin incorporated subjective elements into his theory of organic origins, and ….these elements were a direct function of his route to discovery."*

Discovery appears non-rational only if we overlook the complex pattern of thought that precedes justification. What precedes justification is not simply hitting upon an idea. It involves confrontation of a problem, creation of an explanatory

hypothesis and hitting upon auxiliary assumptions in conjunction with which it yields test-implications (Krajewski, 1981). The confrontation of a problem is usually in the form of identifying an anomaly and this itself is a rational act. Further, the confronted problem needs to be put into a new light whenever the old attempts made to solve it prove inadequate. This again is a rational act. Creation of an explanatory hypothesis is not simply the result of an instantaneous flash. The scientific practitioner must make sure whether such an idea is in harmony with other theories, which he doesn't find fault with. If he finds fault with those theories also, the problem becomes deeper in its significance and wider in its ramifications. Further, he must make sure that the proposed theory is internally consistent. These considerations are more palpably present in fundamental i.e., theoretical sciences. Lastly, it needs a protracted struggle to identify the auxiliary hypothesis without which the proposed theory does not yield test implications. If all these steps do not call for the role of rational faculty one wonders what rationality and rational faculty mean.

It is true that Karl Popper, who takes an anti-discovery stance, very much recognizes the importance of 'problem' in the gamut of scientific theorizing. In fact, unlike Positivists, he considers it to be the starting point of scientific reflection. He very rightly recognizes science as an essentially problem-solving activity. However, such recognition does not have a functional role in his scheme of science. Science, according to him, resides in the attempt of falsifying or critically evaluating the conjectures. It is this that compels him to relegate all that precedes evaluation to the domain of the non-rational. Thus the phenomenon of problem-solving, characteristic of science, remains in spite of Popper's formal recognition a neglected area of philosophy of science. It is to the credit of the contemporary discussion on the problem of scientific discovery that this neglected topic is given its due share of attention.

By pushing the idea of problem-solving to the center of the stage, the problem of discovery can bring about a fundamental change in the very orientation of philosophy of science, where, by and large, ideas of theory and explanation are treated in isolation from the problem-context in which the theories and

explanations occur. It is such an isolation and abstraction that has given rise to the attempts of reducing, philosophically, certain concepts to other concepts and certain theories to other theories. Though such reductions do occur in the scientific practice itself in connection with certain problems, such reductions are misleadingly interpreted by philosophers of science in epistemological terms thereby rendering their account of actual scientific practice not only half-true but even misleading (Hesse,1959).

This discussion points to the need for working out a very broad theory of scientific rationality and the central role of the problem of discovery in working out such a theory. Such a theory of scientific rationality must replace the existing one which fails to do justice to the different facets of the thought-process in science and also is incompatible with the fact that most of the times there is a conspicuous absence of consensus in the reception of a theory. This should not have been a fact if the norms of rationality in science were as cut and dry as believed to be by the logical empiricist as well as Popperians who made justification the essential stage of scientific thought. Even Thomas Kuhn seems to push under the carpet such a recalcitrant fact of the absence of consensus when he gives the impression that the consensus will ultimately prevail due to the institutional framework of science wherein the peers will manage. Such a position leaves unanswered many questions. Joseph Agassi pinpoints such questions and difficulties, the most important being the vicious circle brought about by the situation: How do we bring about the consensus? Through leaders. Who is a leader? One who is accepted by consensus?

An adequate theory of rationality which will make room for the wider considerations on the one hand and the frequent absence of consensus on the other will be able to locate and characterize the creative dimensions of scientific activity without compromising time-honored logical values like consistency. In the words of I.B. Cohen (1956), *"the logic of discovery converges on the logic of the discovered".* In her celebrated paper 'Should Philosophers of Science Consider Scientific Discovery?' Elzbieta Pietruska-Madej puts her finger right on the pulse of the issues when she explains the contemporary

construal of scientific change as irrational and the failure of philosophers like Popper in establishing the rationality of scientific change. She traces both of them to the neglect of the problem of discovery. The surrender of the study of the process by which new scientific knowledge emerges coupled with an interest in the dynamics of science has resulted in a paradox. Further, Pietruska-Madej says,

> *"The growth of scientific knowledge must appear to philosophers to be a discontinuous process: the stages about which they (as philosophers) have nothing to say alternate with those that are the main and proper subject of their interest!"*

In other words, the predicament of irrationality emanates from the apparent phenomenon of discontinuity – the inevitable result of the Nihilistic attitude towards scientific discovery. For the continuities in science can be established in the domain of discovery where new knowledge and old knowledge somehow are related in certain ways, which may not be specifiable in formal ways. These relations, though not formally articulable, are strong enough to establish new knowledge as indispensible i.e., as somehow being determined to occur. That is the reason why scientists and historians of science speak of certain discoveries as historical necessities. That is why it makes sense when Medawar (1963) says that if Crick and Watson had not discovered the structure of DNA someone else would certainly have done so. It is also the case with the eminent historian de Solla Price's (1986) conviction about the discoveries of Boyle and Planck. Pietruska-Madej very rightly points out that it is this inevitability of the new scientific discovery that can explain simultaneous discoveries. The awareness of the relations is part of the reflection that constitutes discovery process. Hence a study of discovery will go a long way in extricating us from the predicament if irrationalism.

The whole of the preceding discussion was intended to drive home the point that the problem of discovery can shed new light on the central issue of philosophy of science, namely, the nature of scientific rationality, the Nihilistic view towards discovery has a naïve and wrong conception of not only the process of

discovery but also of justification as well. In the zeal to present justification in cut and dry terms, which are amenable to a logistic formulation, it fails to make certain crucial distinctions between various types of scientific theories. It therefore fails to recognize the fact that the various types of scientific theories might call for various types of evaluation. For example, the phenomenological theories and deep-structure theories might be subject to qualitatively different types of evaluation. Similarly, some theories might make no reference to ideal conditions/ entities whereas others make no sense without postulating them. The evaluation process is definitely not the same in the case of these fundamentally distinct types of entities. In other words, the existence of the degree of complexity resulting from the variety of the ontologies of those theories has been totally missed by those who construe justification in terms of a logical monolith.

According to the writings of Worral (1985), Nickles (1985), Krajeswski (1981), Leplin (1980), etc., certain types of theories whose peculiarities are of paramount theoretical significance involve a type of justification in which figure the factors which concern discovery and thus obliterating the line between discovery and justification. Thus this discussion is sufficient to warrant the contention that the picture of science presented by the Nihilistic construal of discovery is not just incomplete but even distorted. That is to say, the anti-discovery view is guilty not only of omissions but also of commissions.

There is an increasing need felt by philosophers of science to bring into relevance history of science in order that their professional concern must be related to the real science rather than the 'ideal' science that exists in the minds of tough-minded logicians of science. But it is not at all clear what type of history and what amount of history should make entry into the philosophical discussion about science. Even Thomas Kuhn (1979) who by percept and practice is more than most of other philosophers' enthusiast of history of science has not been able to specify the modalities that should govern the relationship between history of science and philosophy of science. He almost sounds pessimistic when he says that since philosophically useful historical accounts of science are too scarce and far between it is necessary that philosophers of science themselves

should become historians of science. Thus Kuhn has left to philosophers of science the difficult task of formulating the precise ways in which history of science and philosophy of science can be brought together, yet keeping the autonomy and integrity of these disciplines intact. This is more easily said than done.

The problem of discovery can successfully work as a point of mediation in making historical accounts of science philosophically charged and philosophical accounts of science historically informed. Larry Laudan (1977), though skeptical of the clarity of the demand for bringing history and philosophy of science together, agreed that the best possible way of doing so is by focusing our attention on the history of methodology of science. One can say, whether Laudan agrees or not, that methodology mostly concerns not so much about justification in the narrow sense of testing but the norms that decide, in however loose a way, the terms of acceptance and these in turn have a lot to do with discovery. It may be worth mentioning here that philosophy of science and history of science were not so closely related in the past precisely because the problem of discovery was either thrown away or made an appendage to the problem of justification. This reveals the significance of the problem of discovery for history of science.

It is often emphasized that science education must not present science as a repository of formulae but as a human activity with an essential historical dimension. That is to say, science education must present science in a dynamic rather than a static mold. But obviously mere presentation of historical facts in a chronological order does not in itself constitute a historical perspective. The question, then, is what would be a fertile history of science that can equip a student with a historical perspective? By and large, the available histories of science, whenever they are accessible to a student of science, would be found lacking in terms of providing a perspective.

A serious study of the phenomenon of discovery can contribute to finding terms for historical narrative that can possess both philosophical richness as well as pedagogical utility. It must be admitted that, unlike in general history, history of science could have had simple patterns of explanation since

> *"scientists appear to be engaged in a more orderly and rational activity than those involved in politics, waging wars, or molding social institutions. However, there is one crucial aspect of science, which seems particularly recalcitrant to analysis, namely, the process of discovery"* (Koertge, 1980).

In what follows, a very brief discussion on how the problem of discovery can bring out the significance of history of science to science-studies and how it can make history of science capable of being very relevant is discussed.

The problem of discovery enables the historians of science to identify and explain certain important phenomena in the history of science. These concerns are what are called simultaneous discoveries and also aborted discoveries. Historical accounts of science that only chronicle successful discoveries and concentrate only on empirical ratification of theories cannot even recognize these phenomena, let alone explain them. It is in this context one needs to make a reference to simultaneous discoveries and also how certain discoveries have a determinate character.

The simultaneous discoveries in science are too large in number to be dismissed as accidental. This is because as de Solla Price (1986) points out, one can get the impression that every fact and every theory were lying waiting to be discovered and when their time came these ideas got discovered by several competing people (Koertge, 1980). The occurrence of simultaneous discoveries, in other words, is made possible by the fact that the seeds of those discoveries pre-exist in a manner that the occurrences are determined. These seeds exist in the previously existing knowledge. Hence a discovery that is simultaneously made must be understood in terms of its relations with the old knowledge. The understanding of those relations is made possible by taking up the problem of discovery. For it is in the context of discovery that those relations reside. To be more precise, they exist in the relations that concern the structure of the problems and the character of the background knowledge. As Burian (1980) points out,

> *"It is the definiteness of problem structure and of the shifts in that structure when the background knowledge changes which account for many of the notorious cases of simultaneous discovery."*

Though history of science as a discipline has not overlooked the discovery problem, the problem has not been handled with the analytical tools that are indispensible for understanding the conceptual layers of the problematic. Mere presentation of facts, figures, data and biographical information are not sufficient for such understanding. The second phenomenon of aborted discovery occurs quite often. Note that these are not falsified theories but those that are made too soon. As Pietruska Madej (1985) puts it,

> *"These are discoveries which are made, but which are then completely disregarded by science. What is mysterious here is the fact that science often fails to adopt an idea of genius, but then rediscovers it years later. How then do we explain the failure of such 'discoveries'? If we consider the essence of scientific discovery to be a matter if invention alone, we cannot answer this. We answer it when we note that there are some objective reasons in science that make the idea in question superfluous or unnecessary, reasons either built into the body of science or disconnected from it."*

A consideration of the problem of discovery facilitates a proper understanding of what an explanation ought to be in history of science. It is very difficult to find adequate explanations in most of the historical accounts and it is undoubtedly true that the objects of historians' inquiry are too complex to be woven into a neat pattern. The advice of some philosophers like Carl G. Hempel (1966) to historians to adopt the Deductive-Nomological Model of explanation is absolutely wrong-headed. Not only is it doubtful whether the D-N Model fits explanation in Natural science, but also, even if it does, there no reason why it should be extended to history.

It may be, in fact it is, the case that historical understanding has an integrity and uniqueness, which makes it *sui generis.* The absence of covering laws in historical narrative makes the proposal all the more precarious. Since the available models of historical explanation are unacceptable it is necessary for the historians to go in search of their own alternative. Philosophers of history, like William Dray (1957), have gone a long way in doing so. Their work underpins the need to explain human

actions in terms of the norms and reasons as they make sense to the agents. The historian of science must seek similar entities in his explanation of the historical events – achievements and failure.

Given the obvious fact that the empirical justifications in terms of verification or falsification are straightforward acts the complexity that characterizes scientific events must be traced to the context of discovery (Howard 2006; Richardson 2006). Protracted interest in the texture of discoveries constituted by the problem-situation and the constraints and options opened will facilitate the stabilization of the conception of rational explanation that fits the professional concerns of the historian of science. It is only by developing such a theory of explanation that the historian of science can steer clear of descriptivist barrenness and deductivist dogma.

The question of wholesomeness of historical explanation so far as science of history is concerned has often been a bone of contention. This question concerns historiography of science. The purist historiography exclusively concentrates on the so-called internal factors. These internal factors are supposed to cover those considerations, which make science a cognitive system. As against this position it is more and more emphasized today that an adequate historical account of science must recognize the primacy of external factors, which include the social, the political, the economic and the cultural dimensions of the historical context in which science as an institution exists and develops. However, it is obvious that an adequate historiography should not only take into account both the factors but also must seek to work out an analytical model that specifies the relative importance of these factors.

If one looks at the use of the word 'internal factor' by the internalists, one sees that the word is used in too narrow a sense. It may be noted that most of the internalists are of positivist or quasi-positivist persuasion. Being Nihilists in connection with the problem of discovery they exhaust their story of science in terms of empirical justification and it is this empirical justification that is considered to be the central internal factor. As a description of historical facts about science this picture is absolutely wrong. Innumerable examples can be given where

scientific theories are accepted without adequate empirical justification and rejected in spite of pretty strong empirical support. The example of Joanne's falsifying Aristotle's Law of falling bodies nearly one thousand years before Galileo and Simon Stevin's successful attempt to disprove the same law one hundred years before Galileo, are sufficient to support the latter point. So far as the former point is concerned, it may be noted that the Copernican view did not outmatch the old view in terms of its empirical support. In fact the conclusively superior empirical support, the Copernican view got nearly two hundred years after its acceptance in the form of the pendulum experiment.

The above-mentioned considerations have led some philosophers to distinguish between justifications on the one hand and validation or acceptance on the other. The latter is supposed to be broad in the sense that it includes apart from empirical success matters like conformity with the metaphysical commitments, methodological stands and value commitments of the society. It may be noted that these factors are germane to the process of discovery. From this it does not follow that every discoverer is aware of them and sticks to them during the process of discovery. But from this it does follow that what goes on before justification has nothing to do with these factors.

From the above discussion one can infer that an adequate historiography must have a wider construal of internality and such a construal should be related to the phenomenon of discovery. It is only then that we will be able to bring out the complexity of the internal factors. Such a broad construal might even obliterate the line between the external factors and the internal factors and thus facilitate a unitary historiographical framework, which brings home the point that when we investigate discovery we are investigating

> *"not a timeless structure like deduction but rather an event subject to the historian's skills, one about which the first and most appropriate questions to be asked are historian's questions"(McMullin, 1980*).

Such a history of science can be creative by being re-creative. In this context Agassi (1981) says,

> *"The restriction on our conception of science imposes a severe restriction on our internal historiography of science, which consequently prevents us from recreating the past of science as freely as we recreate the past of literature."*

Thus the addition of the discovery-dimension to history of science is nothing more than the addition of historical dimension to discovery. It is this, which can make history of science relevant to philosophy of science and also can provide modalities and guidelines to a creative relation between them.

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