Conflicting Values, Priority Disputes, and Scientific Discovery: A Cognitive-Historical Approach.

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Abstract

In the history of science, there have been heated controversies about which scientist was the first to discover a natural law, formulate a particular theory or observe a natural phenomenon, a state of affairs clearly indicative of the penumbra of conflicting emotions and values inherent in science as a social institution. Before the historical turn in the philosophy of science, philosophers tended to believe that, because discovery is an act it is always possible to identify with exactitude the scientist who first made a discovery and the particular time the feat was accomplished, if all the relevant facts were available. Logical positivists and falsificationists (the rationalists) maintain that analysis of scientific discovery is outside the purview of the philosophy of science. According to them, the cognitive processes involved in episodes of discovery are too mysterious and, therefore, not amenable to formal ratiocinative scrutiny. This view appears to be supported by different accounts of scientific discovery made through flashes of insight or intuition embodied in Archimedean eureka-experiences. The central theories of the positivists and falsificationists on discovery (and other aspects of science) were later challenged by philosophers who felt that the two mainstream traditions, in their excessive preoccupation with technical problems of "the logic of science," have lost contact with real science. The present study reconsiders some of the themes hitherto excluded by the rationalists from the philosophy of science, considering the fact that developments in the subject have demonstrated the relevance of these topics to philosophical understanding of science as a knowledge-producing endeavour undertaken by fallible human beings. It traces the origins of the rebellion against rationalism, and identifies some directions it has taken over the years. Some conflicting values or norms shared by members of scientific communities are critically discussed. A novel analysis, within the framework of cognitive-historical method, of Ernest Rutherford's discovery of the large mass of atomic nucleus which revolutionised pre-twentieth century concept of matter and set in motion the nuclear age is presented. Finally, the paper argues that knowledge of the complex motivations and cognitive processes which lead to priority disputes and scientific discoveries provides valuable insights on how to place actual discovery processes within a philosophical framework of human representational and problem-solving capabilities.

Keywords: priority dispute, conflicting norms, scientific discovery, alpha particles, cognitive-historical analysis.

1.1 Background Analysis: Challenges to the Rationalist Tradition in the Philosophy of Science

One of the leitmotifs in scientific literature, especially standard textbooks recommended for the teaching and learning of science, is the claim that a law of nature or natural phenomenon was discovered by so-and-so scientist at a particular point in time. Logical positivists and falsificationist philosophers of science (the self-styled rationalists) in general, distanced themselves from analysing the "context of discovery" to determine whether epistemologically germane insights or conclusions can be drawn from actual discovery accounts in the history of science. Indeed, the rationalists insist that the process of scientific discovery does not fall within the domain of the philosophy of science proper, on the ground that it involves intuition or insight which is not amenable to logical analysis. Now, although logical positivism and falsificationism disagree on key methodological and foundational issues such as demarcation between science and non-science, the meaning of scientific terms, induction and probability, their leading proponents tended to see the philosophy of science as a discipline in which the quest to elucidate the relatively stable and formalisable logical cum epistemological principles of scientific research is preeminent. (Ayer, 1959; Popper, 1959; Shapere, 1992) However, after the historical turn in the philosophy of science had gained traction from the 1960s onwards, critics of rationalism trenchantly challenged not only the specific theories of the two schools of thought in the philosophy of science concerning demarcation, scientific explanation, the nature of scientific revolution, objectivity and theory choice etc, but also faulted the strategy of approaching the issues and problems of the subject as if they were problems of logic simpliciter. This development was inspired by the works of R. Palter, (1956), M. Polanyi (1958), N. R. Hanson (1958), S. Toulmin (1961), P. Feyerabend (1962), and most importantly T. S. Kuhn (1962, enlarged ed, 1970). Over the years, positivists and falsificationists have considerably elaborated some of their doctrines to meet these criticisms. But many philosophers felt, correctly in our view, that a different approach to the problems of the philosophy of science is a desideratum.

Aside from these developments in philosophy, research findings generated by the history of science revealed important facts which were incompatible with the rationalist conception of science and its historical development. (Wightman, 1950; Butterfield, 1957; Kuhn, 1957; Gillispie, 1960; Dijksterhuis, 1961) For example, as Kuhn realised while grappling with Aristotle's physics in the late 1940s, available historical data refuted the positivist belief that the history of science is mainly a record of systematic abandonment of ignorance, superstition, prejudice and other obstacles to science on the basis of increasing accumulation and expansion of verifiable knowledge – a belief described by Kuhn as "the concept of development-by-accumulation." (Kuhn, 1970:2) Consequently, several older theories that were supposedly overthrown and superseded – Aristotelian and medieval mechanics, geocentric, phlogiston and caloric theories, and so on – were discovered to contain far more than the simple-minded error and superstition usually attributed to them by earlier, less scholarly and more positivistic historians of science. (Shapere, 1992: 33) In a paper entitled "The History and the Philosophy of Science," Kuhn recommended active discourse between historians and philosophers of science. His fundamental work, *The Structure of Scientific Revolutions*, is an important contribution to the increasing tendency towards historical approach in the philosophical investigation of science, an orientation that has yielded fascinating results which further exposed the loopholes in rationalist research programmes. It also justifies extension of the boundaries of the philosophy of science to accommodate some topics occluded from it in the heydays of positivism. (See Kuhn, 1977; Neimeyer & Shadish, 1987; Giere, 1989; Nickles, 1989 & 1998, Hoyningen-Huene, 1993; Tiles & Tiles, 1993; Mayo, 1996 for relevant discussion and bibliography)

With the benefit of hindsight, exponents of logic-dominated research programmes in the philosophy of science emasculated the scientific enterprise by excluding important features of scientific research which conflicted with logic-informed, epistemologically rigid foundational paradigms. (Kuhn, 1970, Feyerabend, 1977; Margolis, 1987; Nercessian: 1992). Again, they erroneously downplayed the fact that science is ineradicably a social institution sustained by values and norms which the philosopher of science ought to dissect. But, probably misled by what Richard Bernstein called "Cartesian Anxiety" (the alleged epistemological disjunction between complete objectivity and complete relativism), positivists and falsificationists thought that any approach in philosophical analysis of science which legitimises principles that lie outside the purview of "logic of science" will jeopardise the rationality of science – rationality defined from the perspective of formal logic. (Ayer, 3-28; Popper, 1959; Carnap, 1962; Bernstein, 1983: 16-25)

A relatively recent post-positivist school of thought in science studies called social constructivism has emerged whose proponents engage in "boundry work" aimed at mapping the fuzzy boundaries between science and other disciplines. Naturally, social constructivists display strong sociological orientations in their work, and adopt a more holistic approach in handling specific problems relevant to the philosophy of science. (Barnes, 1982; Bohme &

Stehr, eds., 1986; Fuller, 1988; Pickering, ed., 1992; Shapin, 1992; Gieryn, 1997) Thomas Nickles, for instance, has called for integrated accounts of the sciences which are naturalised, historicised, and socialised. (Nickles, 1989: 242) *The Structure of Scientific Revolutions* by Kuhn is a good example of the strengths and weaknesses of the type of integrative research programme envisaged by Nickles.

The main threads that emerge from social constructivism are: (a) that scientific research is a complex activity whose outcome is determined by the dynamic interaction of reason and emotion within congeries of specialists; (b) that conflicting norms and values inherent in the social institution or organisation of science generate ambivalent attitudes in scientists with respect to how they understand their work in relation to the work of their colleagues.

From a different perspective, M. DeMey (1982), Margolis, (1987), R. Giere (1988), and N. Nercessian (2003) have produced research findings which demonstrate how cognitive-historical analysis or C-HA (which consists of a blend of cognitive science with the history of science) can serve as an empirically corroborable basis for rigorous philosophical reconstruction of science and provides a more balanced and realistic account of science which brings together within a single philosophical framework the “internal” and “external” aspects of science. (Kuhn, 1977: 105-120; Shapin, 333-369) It is not surprising that although Kuhn did not refer explicitly to the methodology in his major writings, rudiments of the approach can easily be found there. (Kuhn, 1970 & 1977) Principles of cognitive science (a loose confederation of cognitive psychology, artificial intelligence, cognitive neurology, linguistics and philosophy) are integral to C-HA. (Margolis, 1987; Nickles, 1989; Nercessian, 1992) The discipline offers analytic techniques which, if used appropriately with a keen eye on their scope and limitations, can help philosophers develop and test models of how new knowledge in a scientific specialty is created and consolidated through painstaking research.

The question may be asked: what relevant epistemological insights(s) can be arrived at by investigating the set of shared conflicting values and actual processes of scientific discovery which form the focus of the present study? Karl Popper, a staunch falsificationist, maintains that the philosopher has little to learn from the history or sociology of science (and cognitive science) because these disciplines are spurious sciences without developed, generally accepted, standards for problem-selection and problem-solutions. (Popper, 1970:57-58) But Popper's view is obviously untenable, for the disciplines he was referring to have developed high standards of scholarship and professionalism that compare favourably with what obtains in the natural and biological sciences. In cognitive psychology, for instance, experts in the field have formulated powerful falsifiable theories and precise experimental protocols which meet the standards of rigour and objectivity required in science. (Fuller *et al*; 1989; Sternberg, 2003; Gleitman *et al*, 2004) Therefore, it is not surprising that interesting ideas on the general nature of concept formation and learning, representation, perception, and conceptual change among others have emerged from the so-called "spurious" sciences. (Margolis, 1987; Fetzer, 1991; Chen et al 1998; Nercessian, 2003) For example, Kuhn, in a paper entitled "Second Thoughts on Paradigms" argues that pattern-recognition or perception of similarity/dissimilarity relationships between new problems and exemplars (that is, research puzzles already encountered and solved by members of a scientific community), rather than application of correspondence rules and algorithms as suggested by Carnap, Popper and others, plays a fundamental role in the acquisition of scientific knowledge. (Kuhn, 1977: 293-319)

1.2 Conflicting Norms in the Social Organisation of Science

As already indicated, mainstream logistic traditions in the philosophy of science did not investigate critically the conflicting values that shape science as a knowledge-producing enterprise undertaken by communities of scientists. Consequently, they contributed very little to elucidation of the epistemological significance of conflicting values shared by scientists. One of the main tasks of philosophy since its inception in antiquity is the critical evaluation of norms or values of all kinds. (Bonevac, 2006; Soccio, 2007) It follows that the values shared by members of scientific communities are legitimate topics for philosophical analysis. R. K. Merton has carried out detailed, philosophically relevant, research on how conflicting values or norms affect the conduct of scientists, especially with regard to priority disputes. (Merton, 1973 & 1976. See also Mullins, 1972; Woolgar, 1976; Mulkay, 1980) These norms can be formulated and juxtaposed as pairs of contradictions or sub-contraries. One of the pairs stipulate that a scientist must be ready to make her new found knowledge available to her colleagues as soon as possible, but must avoid undue tendency to publish her work too soon; another pair enjoin her to esteem new knowledge, but should work without regard for the recognition of others. A checklist of some conflicting pairs of norms or values which generate ambivalent attitudes in scientists is tabulated below (Merton, 1976: 434-435):

Conflicting Pairs of Norms in Science as a Social Institution	
A1. The scientist should be willing to make his new-found knowledge available to his colleagues promptly.	A2. The scientist should not be in a haste to publish his research result.
B1. The scientist should not be too eager to abandon an old theory, or adopt intellectual fads which come and go.	B2. The scientist should be flexible and receptive to friutful new ideas, and avoid being fixated on maintaining established traditions.
C1. The scientist should pay serious attention to details.	C2. The scientist must avoid fastidiousness with minutiae usually associated with pedants.
D1. Scientific knowledge is universal and trans-national.	D2. Scientific discoveries bring prestige to the countries that promoted them.
E1. It is recommended that young scientists should learn from more experienced eminent colleagues.	E2. It is important for young scienists to be independent-minded and endeavour to stand on their own feet.
F1. Scientists should recognize the critical importance of producing new generation of scientists.	F2. A scientist must not allow teaching to take up all her energy at the expense of research.
G1. A scientist should endeavour to know the work of her predecessors and contemporaries in her specialty.	G2. A scientist should avoid omnivorous reading which will hamper her creativity.
H1. Scientists should put premium on new discoveries and inventions.	H2. The scientist should concentrate on research without craving for respect or priority from colleagues.
I1. The scientist should not claim any new knowledge until it is well coroborated.	I2. A scientist should defend his new theories and findings, even in the face of criticisms from others

The list is not exhaustive, but is a fair sample of norms culled from the literature of science. Clearly, tension-generating potentials of these norms which give rise to divergent attitudes and decisions by scientists are demonstrated in the careers of great scientists like Isaac Newton, Joseph Priestley, Charles Darwin Michael Faraday, and Albert Einstein among others. (Mason, 1962; Fine, 1976: 145-158; Cohen, 1985) In the case of Priestley (1733-1804) for instance, aside from the priority dispute about whether he or the French chemist, Antoine Lavoisier, was the first to discover oxygen, Priestley till he died espoused the phlogiston theory of combustion, despite contributing significantly to researches that eventually led to its eventual overthrow. (McKenzie, 1973: 98-104; Kuhn, 1977: 167-171) Therefore, by fixating on the phlogiston paradigm, Priestley failed to reconcile appropriately in practice norms B1 and B2 which, respectively, prescribe that the scientist should not to be too eager to abandon an old theory but at the same time should be receptive to new and fruitful ideas.

Again, consider the pairs of values H1 and H2, which respectively recommend that a scientist should work towards making original contribution to knowledge, but should not be preoccupied with claiming priority for a discovery or with earning respect from colleagues. Generally speaking, scientists respond negatively and defensively to accusations of plagiary; also they are usually disappointed when presented with evidence that their research findings had been anticipated by, or is a replication of, earlier work by another scientist, especially if the latter is a relatively unknown researcher. (Merton, 1976; Boorstin, 1983, Hawking, 1988) Studies of priority disputes in science establish the "competitive dimension' of science" within the social context of research in which each scientist works feverishly to be first to discover an unknown phenomenon or postulate a revolutionary theory that resolves a stubborn anomaly and opens up a novel fruitful line of investigation. In extreme cases, some go to extreme lengths to prove their priority and bully rivals, as it happened in the late 17^{th} century when Newton misused his prestige and domination of the Royal Society of London to humiliate Gottfried Leibniz in a row over the discovery of calculus. (Boorstin, 413-419; Hawking, 181-182)

Scientists want their priority in discovery or invention to be acknowledged for several reasons. (Reif, 1961: 1957-1962; Watson, 1980) Number one, standard textbooks for pedagogy in science, as we already indicated, usually attribute discoveries to particular scientists that made them. This helps prospective scientists to be grounded in the historical evolution of science and, perhaps, inspires them to emulate the great personages in their various specialties. Thus, to be recognised as the discoverer of a new and important phenomenon or inventor of a revolutionary theory is almost like a property right for the scientist so recognised. Two, innovators in science are respected both within the scientific community and in the wider society, which is directly proportional to the impact the discovery makes in terms of explanatory power and technological application. Finally, especially in highly industrialised societies, substantial financial rewards and prestigious awards (Nobel Prize etc.) are often closely tied to fundamental scientific discoveries. Such awards increase the prestige of the awardees, and motivate others to work harder to enhance their chances of being rewarded in due course.

R. K. Merton identifies recurrent features of priority disputes, such as the essential tension between co-operation and competition in the production of scientific knowledge which is necessary as one of the strongest motivations for research. (Merton, 1976: 437-446) Sometimes, according to Merton, scientists are reluctant to engage in priority claims, an attitude which suggests awareness of the impropriety of such disputations. Yet, on several occasions, they are genuinely interested in establishing priority for a particular discovery. Typical examples highlighted by Merton include Newton, John Flamsteed, Priestley, Darwin, and Sigmund Freud. Scholars usually feel uneasy that dispassionate methodic investigation of priority disputes will sully the otherwise heroic, awe-inspiring, image associated with great scientists. (Merton, 1963; 272-282) Such uneasiness, though understandable, is uncalled for and unnecessary. Upon close analysis, it would be discovered that responses of scientists to the imperatives of their profession, like other human behaviours, are not actuated by a single motive which can be unequivocally pigeonholed with positive or negative ethical categories, as the case may be. Rather, desire to be the first to make a discovery and to be recognised as such is the surface presentation, like the part of an iceberg above water, of the deep-seated need for assurance among peers that one has made a significant contribution to the corpus of established knowledge, or produced a result which will be of immense benefit to humanity. Partly on the strength of such considerations, the scientist can pursue his research diligently, encouraged by the belief that his work will be justified by the quality of his contribution to knowledge. To bolster scientific productivity, science as a knowledge-producing social institution must be managed in such a way to promote symbiotic relationship between the cognitive imperatives of research and the self-interests of individual scientists. Creating and sustaining such relationship is always a work-in-progress, not the least because of the difficult challenge of reconciling diverse and sometimes conflicting personalities and motivations of scientists. The problem is exacerbated when, for a particular scientist, desire for honour and reward displaces concern for advancing knowledge, which might negatively affect his work. It must be observed that an institutionalised reward system which lays emphasis on mere number of publications promotes mediocrity in the quality of scientific output. (Popper, 1992: 96) In the universities and research institutes, therefore, it is important to set benchmarks for identifying and rewarding researchers who make profound contribution to the growth of scientific knowledge. That said, since every scientist cannot be a Newton, Lavoisier, Darwin or an Einstein, the work of less talented scientists should be recognised also - for example, by including their contributions in prestigious journals and other publication outlets. (Garfield, 1964; Price, 1965)

In contemporary times, the social character of scientific knowledge has come to the forefront with the increasing tendency towards collaborative research. (Hagstrom, 1965; Price & Beaver, 1966: 1011-1018; Crane, 1969: 335-352; Mullins, 1972: 51-82) In many areas of scientific research specialists are actively encouraged to collaborate due to the growing sophistication of research problems and methods which in many cases necessitates teams of researchers from different scientific and engineering disciplines working together using very expensive equipment. (Weinberg, (1961: 161-164; Rescher, 1978: 175-178) In collaborative work, it is sometimes difficult to determine the input of each scientist, as more and more scientific publications have several authors rather than only one and there is simply no mechanical way of deciding precisely the part played by each contributor in solving a particular difficult equation or conducting a complex experiment. (Price, 1963: 87-90; Merton, 1976: 451-452) Hence, although collaborative research does not completely eliminate priority disputes by shifting the focus of scientists away from establishing priority to concern about how their contributions can be identified after collaborating, it has reduced the frequency of such disputations. The revolution in Information and Communication Technology (ICT) has the same effect as well. Secrecy in research has reduced considerably: scientists are now more willing to brainstorm concerning their researches with colleagues especially through the Internet. And with rapid increase in the number of specialist conferences and publications, the tempo of interaction among scientists is higher than ever before, thereby encouraging collaboration which, as we claimed a moment ago, tends to bring down the frequency of priority disputes.

The question that rears up at this point is: what is the epistemological significance of conflicting norms or values in science? It can be rephrased more usefully as: how will a specific set of values shared by members of a scientific community affect their group behaviour as researchers? In handling that question, rationalists postulate rules or algorithms of choice that would ensure complete objectivity particularly when scientists have to choose among competing theories in the same domain. (Carnap, 1962; Popper, 1959) Now, if there is an exhaustive list of methodological rules or algorithms that can be applied in the same way by each and every scientist to arrive at the same decision, then science will not only lose much of its empirical character and become more like mathematics, it might stagnate or even retard. (Musgrave, 1976: 480) Kuhn argues that no such rules exist. That explains why scientists differ in their understanding and interpretation of the norms of research. To die-hard positivists and falsificationists, this variability is a weakness, but to Kuhn it is a sign of strength, an index of the social nature of scientific knowledge. According to Kuhn:

> If decisions must be made under circumstances in which even the most deliberate and considered judgment may be wrong, it may be vitally important that different [scientists] decide in different ways. How else will the group hedge its bets? (Kuhn, 1970a: 241. See also 238)

To sum up: conflicting norms in science serve as the essential flexible platform or matrix of factors which prevents wholesale persistence with a degenerating tradition of normal science or premature mass-conversion to a fledgling one, both of which would have to be accounted for on purely social-psychological grounds. (Musgrave, 480) This implies that since scientific research is conducted within specialist groups, practices that would indeed be irrational if adopted by those groups as a whole or by a majority of them need not necessarily be irrational when geniuses like Priestley or Einstein engages in them.

1.3 Rutherford's Revolutionary Discovery of the Atomic Nucleus: A Cognitive-Historical Approach

In subsection 1.2, we underscored some conflicting norms which determine the pattern of behaviour of scientists as they carry on the difficult and challenging task of acquiring, consolidating, and improving the corpus of established knowledge. In doing so we highlighted one of the recurrent issues in the historical development of science which orthodox philosophers of science simply refused to countenance as a fit subject for philosophical inquiry – priority disputes. Now, although the question of priority is not identical with the problem of determining the date when a particular discovery was made, both are related in the sense that establishing when a given discovery was accomplished for the first time can help settle any priority dispute that may arise from it. Even so, all scientific discoveries are accomplished within an epistemological ecology partly constituted by the conflicting sets of shared norms or values within scientific communities which we discussed earlier. This explains why whenever heated debate about priority ensues, it always occurs in the backdrop of conflicting values probably because the precise date when the discovery was completed is uncertain. At all events, many interesting and important scientific discoveries are not easily amenable to the questions of "Where?", "When?", and "Who did it first?" (Kuhn, 1977: 171) Such questions recur because both scientists and nonscientists erroneously believe that discovery is a singular, almost instantaneous event which, although it may have preconditions and definitely has consequences, is very similar to perceiving something at a specifiable time. A discovery, according to this conception, is a mysterious, philosophically opaque, event that happens when a particular scientist encounters a hitherto unknown phenomenon or object at a particular time and place. The major source of this inaccurate view of scientific discovery is the nature of the scientific community and the kind of education engendered by it. As already noted, recommended science textbooks report that so-and-so scientist was the first to discover a particular phenomenon or relation between apparently unconnected phenomena which may be named after him or her (for example, Boyle's law, Planck's Constant, Curie point, Zeeman Effect, Compton Effect, etc). Given the premium which the social institution of science, sometimes obsessively, places on originality and contribution to knowledge through genuine discovery, many scientists focus their minds on it. Prestige within and outside the professional circle of science accrues to scientists that make important discoveries, as the case of Nobel Prize winners in the sciences, particularly physics, amply demonstrates. (Watson, 1980)

Generally, scientific discoveries can be classified into two broad types, namely (a) discoveries that were predicted from accepted theory in advance and (b) discoveries which were not, and could not have been, predicted beforehand from current theory. A typical example of type-a discovery is the discovery of gallium, a soft silvery metallic element by Paul Lecoq de Boisbaudran in 1875, four years after the Russian chemist, Dimitri Mendeleev, had predicted the existence of an unknown element (subsequently named gallium) and specified some of its properties. (Chang, 2003) Because the existence of gallium was already predicted from the Periodic Table of elements, its discoverer knew from the start what to look for. This does not necessarily imply that the discovery was neither demanding nor interesting. Rather, prediction of the existence of gallium before it was confirmed provided scientists with parameters for determining when the discovery had been accomplished. Hence, on a general note, it is relatively easy to ascertain the time and date a specific type-a discovery was made and the scientist(s) who did so. Naturally, there have been relatively few priority disputes pertaining to discoveries already anticipated by existing theory, which suggests that only paucity of information can prevent identification of the scientists involved and the date and place such discoveries were made. (Kuhn, 1977: 166-167)

But consider the discovery of radioactivity, electron, cosmic rays, the atomic nucleus and others which were not predicted from accepted theory beforehand and which, therefore, were not anticipated by scientists. They belong to the class of troublesome discoveries for which there were no unambiguous benchmarks to inform scientists when the task of discovery has been accomplished. It should be observed that not all discoveries can be easily classified into any of the two categories identified above: for example, although Paul Dirac's equations entailed the existence of "positive electrons" or positrons, no physicist really understood what the particles were until their existence was confirmed experimentally by C. D. Anderson. (Hey & Walters, 2003: 228-233; Crease & Mann, 1991: 89-90)

We have already made the point that the positivists and falsificationists were mistaken in thinking that "the context of discovery" is not a legitimate subject-matter for philosophical investigation of science. But although Kuhn improved upon entrenched positivist ideas about discovery by acknowledging that it is an extended creative process, and as a historian of science has provided detailed analyses of selected revolutionary discoveries which elucidate different structural elements of the process, he still erroneously overemphasised the gestalt character of discovery – that is, by construing it as the last act when "the pieces fall together." (Kuhn, 1987) This widespread misconception can be avoided by using a cognitive-historical methodology in studying discovery episodes. Cognitive–historical analysis (C-HA) is a fruitful approach for exploring scientific discoveries to yield philosophically germane insights into cognitive mechanisms at work when scientists engage in the creative processes of inventing new concepts and representations of already known or novel phenomena. Fundamentally, it is an interdisciplinary approach forged from the nexus between several disciplines, as indicated earlier. The particular mix of insights from among the disciplines required for analysing any topic depends crucially on the nature of the topic itself. For example, adequate analysis of the relation between scientific theories and the evidence in their support will depend largely on concepts that feature prominently in philosophical theories of induction and probability, whereas theories of perception and information storage and retrieval distilled mostly from cognitive psychology and artificial intelligence will be needed for a realistic philosophical account of knowledge encoded in perception and representation in science. As a research tool in the philosophy of science, C-HA uses information culled from the history of science to identify, dissect and evaluate generalisable representational and problem-solving activities through which scientists create new theories. (Woolgar, 1976; Margolis, 1987; Giere, 1988 & 1989; Nercessian, 1992) Thus, C-HA also enriches what historians of science do in their fine-structure investigations of science by interpreting results of their work in the context of corroborated conclusions drawn from studies of ordinary human representational and problem-solving activities. Moreover C-HA, as a tool for empirically-grounded philosophy of science, enhances fruitful "active discourse" that Kuhn recommended for historians and philosophers of science.

The underlying assumption of the method is the "continuum hypothesis," which asserts that the problem-solving strategies invented by scientists and the representational practices they have developed over time are sophisticated and refined extensions or variants of the cognitive practices used by human beings in solving the problems they encounter in their life-world. (Kuhn, 1977; Margolis, 1987; Hoyningen-Huene, 1993; Nercessian, 2003) The cognitive dimension of C-HA is anchored on the idea that a robust philosophical understanding of scientific knowledge should be psychologically realistic, in that it must reckon with the fact that scientists are not merely human beings who systematically apply rational thought to discover the laws of nature; they are also trained researchers that share with the rest of humanity some biological and social attributes. Thus, C-HA allows the philosopher to properly identify and explain adequately how scientific research is constrained by human cognitive capabilities and limitations, because science is a fascinating product of the interaction of human minds with one another and with the world. This point is not totally alien to philosophy, for it fits into what might be called a tradition of psychological epistemology adumbrated in the works of the British empiricists, notably John Locke, David Hume and J. S. Mill. (Mill, 1843; Grayling, 2000: 484-544) More recently, attempts have been made to formulate what V. W. Quine described as "naturalized epistemology." (Quine, 1969; see also Harmon, 1973; Goldman, 1986; Bird, 2005) The main thrust of contemporary naturalised epistemology is that the best theories of scientific knowledge, that is, theories which provide the most detailed general account of how and why scientists combine their human cognitive abilities with the conceptual resources available to them as members of scientific communities and broader social contexts to construct and communicate new scientific representations in a research field, must be based on a systematic analysis of the language-mediated cognitive mechanisms deployed by scientists in their research. These mechanisms, obviously, can only be known a posteriori, although rigorous analysis can distill some quasi-logical or methodological canons that justify them. In spite of differences in the articulation of their views, proponents of naturalised epistemology assign a highly specialized but relatively minor role to the construction of axiomatic systems by scientists. Additionally, they emphasize the representational character of theories which encode the world in sets of "schemata" or "paradigms". Margolis, 63-187; Fuller *et al*, 1989; Fetzer, 1991)

A major weakness of a priorist or logistic stance in the philosophy of science is that it conceives scientific discovery and conceptual change as static and ahistorical, whereas both are creative dynamic processes extended in time. C-HA avoids the error by drawing attention to scientists in terms of their representations of nature and the research activities that generate those representations. Hence, contrary to Imre Lakatos' recommendation that the philosopher should ignore or distort historical facts to fit methodological theory developed a priori (Lakatos, 1970: 138), philosophical analysis of any component of scientific knowledge must be informed by the actual representational practices scientists employ in articulating and changing conceptual frameworks. To reiterate again, C-HA is the appropriate method in this regard, because it provides a realistic descriptive and explanatory account of scientific practice.

In order to illustrate how C-HA illuminates the interconnected cognitive mechanisms involved in scientific discovery, we shall analyse Ernest Rutherford's discovery of the nucleus of atom along the lines suggested by Nercessian's cognitive-historical discussion of Clerk Maxwell's efforts to construct the theory of electromagnetic field. (Nercessian, 1992) Naturally, like all scientists, Rutherford was working within the theoretical and experimental traditions of his research field - radioactivity. Right from ancient times, the nature of the ultimate constitutive element or substratum of the universe had elicited the thoughtful attention of philosophers and scientists. According to the ancient Greek philosopher, Democritus, atoms were the smallest indivisible components of matter. In 1803, John Dalton, a British chemist, enlarged on the idea of atoms in his theory of chemical combination, but he lacked well-founded experimental findings to make solid progress. Real advance in scientific understanding of atomic structure began in 1897 when J. J. Thomson, in a paper on cathode rays, announced the existence of extremely small, negatively charged, electrons (or corpuscles, as such tiny particles were called then). (Shamos, 1987: 219) Around the same period, Wilhelm Konrad Roentgen, Henri Becquerel and Marie and Pierre Curie were exploring the newly discovered phenomenon of radioactivity, for which satisfactory explanation was unavailable at the time. Thomson proposed a new model of the atom which depicted it as a positively charged sphere on which electrons were scattered around (the plum pudding model). As a unit, the atom, he argued, is stable and electrically neutral, because the negatively charged electrons were counterbalanced and held in place by positively charged electrostatic forces. Thomson attempted to account for the observed spectra of elements with his model by proposing that the electron, being a charged particle, would radiate energy in accordance with the classical theory of electromagnetism if it vibrates in its equilibrium state, and the frequency of radiation would be the same as that of the electron. For instance, hydrogen, having only one electron, should give a single spectral line. But experiments showed that hydrogen spectrum consists of a number of series, each having many lines. Consequently, Thomson's model could not explain the spectrum of even the simplest atom, hydrogen. (Lal & Ahmad, 1997: 135)

Meanwhile, Rutherford (who later became one of Thomson's protégés at Cavendish laboratory) was researching alpha particles at McGill University, Montreal, Canada. He measured the charge-to-mass ratio of alpha particles, just as Thomson had done for electrons. After four years of research on the particles which began in 1898, Rutherford was convinced that they were positively charged helium atoms, that is, helium atoms without their two electrons. (McKenzie, 292-293; Crease & Mann, 16) The series of experiments that led to Rutherford's discovery of atomic nucleus were launched by his research assistants, Hans Geiger and Ernest Marsden, after he had moved to Manchester University, England, in 1907. Geiger and Marsden tried to measure precisely the paths of alpha rays. They noticed that as the particles from a radioactive source sped outwards, a significant number of them were deflected by the air and the walls of the tube such that it was difficult to determine precisely the actual path of each particle. (Rutherford, 1913: 537; Shamos, 253-264) However, using the newly developed Geiger counter, they discovered that most of the alpha particles hardly changed direction, or were deflected a little. But occasionally some were scattered through very large angles, that is, angles greater than 90°. In fact, one in about eight thousand alpha particles would actually hit a thin sheet of gold leaf and rebound backwards. (Beiser, 1995: 119-120) These large scatterings surprised Rutherford, because if Thomson's atomic theory was correct, the alpha particles passing through a metal foil should experience only weak electric forces: they should be able to penetrate the foil with their initial momenta undergoing only a negligible deflection from their original paths. Rutherford described his consternation about the scattering effects he observed with the following words: "It was quite the most incredible event that ever happened to me in my life. It was as incredible as if you fired a 15-inch shell at a piece of tissue paper and it came back and hit you!" (Quoted in Hey & Walters, 48) The following diagram is a simplified illustration of Rutherford's experiment.

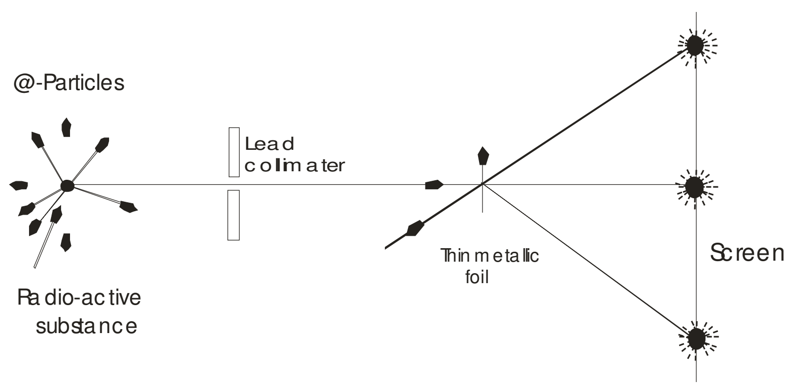

Fig 1. A schematic representation of Rutherford's scattering experiment.
(Culled from Lal & Ahmad (1997), *A Textbook of Modern Physics)*

Rutherford explained the unexpected large scattering of the alpha rays with analogical and imagistic reasoning by transforming the problem of tracing the paths of individual alpha particles into that of analysing the very nature of the electrical forces that determine their spin and momenta after collision with metallic foils still within the schemata provided by classical electrodynamics. The role of analogy in cognition has been explored by M. Hesse (1966), W. Sellars (1985), and H. Brown (1986), Margolis, 1987). In general, analogical reasoning links existing conceptual framework to a new one through the mapping of relational structures from the old to the new. (Nercessian, 1992: 13) As an aid to his imagination, Rutherford was fond of constructing a clear pictorial image in his mind of what was going on when he performed an experiment. On this occasion, unable to break away completely from the "plum pudding" model invented by Thomson, his theoretical calculations nevertheless pointed in a different direction to a new representation of the atom. Rutherford imagined an alpha particle ricocheting off an atom. He conjectured that if almost all the atomic mass was tightly packed into an ultra-tiny charged node in the centre, the latter could possibly deflect an alpha particle. In Rutherford's own words:

> On consideration I soon realized that this scattering backwards must be the result of a single collision, and when I made the calculations I saw it was impossible to get anything of that order of magnitude unless you took a system in which the greater part of the mass of the atom was concentrated in a minute nucleus. It was then that I had the idea of an atom with a minute massive centre carrying a charge. I worked out mathematically what laws the scattering should obey, and I found that the number of particles scattered through a given angle should be proportional to the thickness of the foil, the square of the nuclear charge, and inversely proportional to the fourth power of the velocity. These deductions were later verified by Geiger and Marsden in a series of beautiful experiments. (Quoted in Chandrasehkar, 1990: 3)

The image of the atom as a miniature solar system started forming in his mind as he grappled with the question whether the charged centre was positively or negatively charged – that is, whether it kicked away the alpha particles or whipped them round and round in circles like a swirling object tied to a string. The opposite charge to the nucleus, he thought, had to be some sort of thin gaseous sphere surrounding the middle of the atom. On the basis of the large scattering of alpha particles by very thin metallic foils, Rutherford concluded that the atom is virtually empty. Rutherford first presented his discovery on March 7, 1912, at a session of Manchester Literary and Philosophical Society. Initially, it elicited little attention from his colleagues, but he continued to work to perfect the theory. The unenthusiastic reception was because the theory clashed with one of the most enduring concepts in science and philosophy – the idea of substance. Furthermore, although the nuclear atom model explained the large deflections observed in experiments with alpha particles, it still needed to be interpreted within the paradigm of quantum theory to make it work. This was accomplished by Niels Bohr, one of the earliest physicists to take Rutherford's theory of the atom seriously. Bohr introduced Planck's constant or elementary quantum of action into the model and, thus, was able to explain the spectral lines of hydrogen which, as we observed earlier, could not be explained by Thomson's theory. (Lal & Ahmad, 147-162)

The quest to consolidate his ideas led Rutherford to articulate further his image of the atom. His researches received a boost from the work of H. G. Moseley, who studied x-ray spectra of as many elements as possible. (McKenzie, 294) Moseley showed that, beginning with hydrogen, each element can be assigned a unique number, 1, 2, 3 and so on, which is proportional to the square root of the frequency of the spectral lines. This number is called the atomic number, and it is equal to the positive charge in the atomic nucleus and also to the number of electrons in the atom. In his Bakerian lecture entitled "Nuclear Constitution of Atoms" (1920), eight years after he had announced his discovery, Rutherford suggested that the positively charged nucleus is surrounded by a distribution of negative electrons equal in number to the resultant positive charge on the nucleus. He worked out the relations linking the number of alpha particles scattered through any angle to both the charge on atomic nuclei and energy of the alpha particle. Meanwhile, C. G. Darwin had calculated the deflection of alpha particles by the nucleus, taking into account the mass of the latter. He demonstrated that under the influence of electrostatic forces, an alpha particle describes a hyperbolic orbit round the nucleus, and the magnitude of the deflection is a function of its distance to the nucleus. (McKenzie, 511)

Rutherford's picture of an atom as a miniature solar system functioned as a modifiable representation or mental model which enabled him to draw inferences relevant to solving the problem at hand – an explanation of the observed large scattering of alpha particles by thin metal foils. In other words, the model functioned heuristically as a physical analogy through which Rutherford exploited the powerful representational capabilities implicit in his theoretical constructions. This is at odds with the positivist overemphasis on knowledge encoded in propositions and rules. Therefore, the creative heuristic power of analogical reasoning and patter-recognition mechanisms that feature so prominently whenever scientists are constructing new representations of reality is a modelling process through which relational structures are abstracted from existing paradigm and fitted to the constraints of a new problem situation and schemata. In line with this, Rutherford invented, for the new model of the atom he was proposing, equivalents of gravitational forces that, according to Newton's theory, keep the planets in orbits around the sun. Accordingly, he posited that electrons associated with an atom must continuously rotate around the nucleus, because an attractive force exists between the positively charged nucleus and the negatively charged electrons – if the electrons were stationary, they would plunge into the nucleus. The charge on the nucleus was calculated to be about $^{1/2}Ae$, where A is the atomic weight and e the fundamental unit of charge. Since the atom is electrically neutral, the positive electric charge of the nucleus must be exactly balanced by the negative charge of the electrons orbiting it. (Hey & Walters, 49) C. G Barkla's work on the scattering of *x*-rays by light elements convinced Rutherford that the number of electrons was equal to about half the atomic weight. He also assumed an inverse square law between the nucleus and an alpha particle, by which the probability of scattering through various angles could be calculated mathematically. Rutherford then predicted that eight times as many particles should be scattered between 60^o and 120^o as between 120^o and 180^o. (McKenzie, 293) His predictions were confirmed experimentally by Geiger and Marsden. Keep in mind that it was the ratio of alpha particles that rebounded or were deflected through wide angles to those that were deflected slightly or not at all which finally convinced Rutherford that the atom was virtually empty.

Fig. 2 A diagramatic comparison of Thomson's and Rutherford's models of the atom. Notice that Rutherford's model (B) is closer to the contemporary model than Thomson's

1.4 Comments

Debates about what to include or exclude from the philosophy of science, and about the the proper relation between the philosophy of science and other science studies disciplines have not been completely settled. (Giere, 1973; Burian, 1977; Caldin, 2000, Pinnick & Gale 2000) Logical positivists and falsificationists focused exclusively on the features of science that are amenable to analysis with the tools of formal logic. Philosophers with a more inter-disciplinary research orientation argue that although logical analysis of science has yielded important insights into the nature of scientific knowledge, it has unduly neglected significant features of research which make science a living enterprise undertaken by human beings.

Now, historicised philosophy of science connects to the historico-empirical world in a way that other sub-disciplines in philosophy typically do not imitate, because irrespective of her methodological orientation and preferences the philosopher of science ought to articulate a theory of science realistic enough to explain recurrent features of scientific practice. Since the historical turn, logical coherence and formalisability alone are no longer considered the sole, or even the most important, criteria for assessing philosophical interpretations of science. (Polanyi, 1964; Feyerabend, 1975; Kuhn, 1977; Gieryn, 1997, Nickles, 2003) This attitude reflects dissatisfaction by post-positivist philosophers of science towards orthodox conceptions of what an acceptable philosophical interpretation of science should be and what it should accomplish. There is, therefore, a paradigm-shift from the static a priorist models of logistic philosophies of science to more dynamic, empirically grounded, representations of science which are in tune with actual scientific practice and still retain some degree of generality usually expected in philosophical analyses. On this basis, one can understand why philosophers of science, such as Kuhn, Margolis, and Nancy Nercessian recommended that their theories of science should be read as descriptive and explanatory accounts of science all at once. The recommendation seems paradoxical, but a close study of their works amply demonstrates its suitability and fruitfulness. (Kuhn, 1970b; Margolis, 1987; Nercessian, 2003)

The analytic possibility and desirability of using knowledge practices such as the ones disclosed in discovery accounts to construct a naturalised epistemology is central to cognititive-historical research programme. C-HA provides techniques for interpreting different phases in discovery processes within a realistic epistemological template as changeable and changing forms of problem-solving abilities. In addition, it distils from case studies of actual scientific practices a comprehensive theory of how and why conceptual structures are constructed and changed in science, and increases understanding of how scientists build on existing paradigms while creating genuine novelty. The outcome of such analysis, as contained in the works of Kuhn and others, is a nuanced, defeasible, and epistemologically informative account of scientific discovery that undermines the view that discovery processes are too mysterious to be rationally accounted for. Creativity in science, as in other human endeavors, is a goal-directed process consisting of several interconnected stages. Again, cognitive processes which culminate in scientific discovery are extensions of processes observed in simpler problem-solving situations. From our analysis of Rutherford's discovery of the atomic nucleus, one can identify epistemologically relevant modelling activities in the form of patter-recognition, imagistic and analogical reasoning which are crucial in scientific discovery. The road to that discovery began with the perception of a problem – the unexpected large deflections of alpha particles. The analogies, images and mathematical equations he constructed were integral to the cognitive processes that led to the solution he proposed eventually. Of particular interest is the fact that Rutherford did not take an existing atomic structure and plug the parameters of his solar system model to arrive at a solution. Instead, he applied the idea that large deflections of alpha particles must be due to "a central electric charge concentrated at a point" to create a model which explained better than Thomson's model the phenomenon under investigation.

The solar system image allowed Rutherford to visualize certain structural relationships contained in his mathematical calculations. By clustering together in perceptual space and making visual a nexus of interconnected inferences, the imagistic representations he created were compatible with a variety of perceptual inferences. To pick out the most appropriate one by articulating further the (mathematical) representations and physical analogies he formulated on paper, he had to present them in a form that already focuses on, and abstracts specific aspects of, the phenomenon being investigated. Nercessian (1992: 24-26) provides an account of the cognitive functions which physical analogies like the ones Rutherford employed in interpreting the result of his alpha ray experiments serve in discovery. Briefly stated, the mathematical information implicit in such representations embody structural relations that determine the outcome of experiments and facilitates access to quantifiable aspects of phenomena. As a result, physical analogies provide an intermediate level of abstraction between phenomena and mathematical forms of representation (formulae and equations). They provide also a stable image for the researcher and make various versions of it accessible for direct inspection, thereby assisting the memory in problem-solving. Furthermore, by making available a stable objective embodiment, imagistic representations make it easier for scientists to grasp other relevant aspects of a new representation than text and formulae alone. Applying Nercessian's ideas to our case study, it is evident that at first Rutherford did not understand fully all the subtleties of the solar system model of the atom he was proposing; but he grasped the spatial configurations of the powerful electric charge at the centre which deflected alpha particles. With further research, he was able to link his experimental results with equations embodying abstract structural features of the alpha ray scattering phenomenon. Hence, he succeeded in working out mathematically some testable consequences of the interplay of forces he considered necessary for explaining the observed behavior of scattered alpha particles. Finally, in order to communicate the new atomic representation he invented to his colleagues, Rutherford had to present it in the form of a physical (visually concrete) analogy along with the theoretical calculations that led him to it.

1.5 Conclusion

It must be pointed out that although the historical and cognitive turns in the philosophy of science exposed the weaknesses of positivism, it is important that the philosopher of science should seek out components in historical case studies which can be used to formulate a robust epistemology of science capable of withstanding those weaknesses. CHA, by focusing as it does on scientists and on the historically changing practices through which they create and change representations in a scientific specialty, rather than on static linguistic representations that change radically from time to time, enhances the articulation of empirically-grounded theoretical models of how science actually works and why it works the way it does. Thus although episodes of discovery, from the perspectives of positivism and falsificationism, seem irrelevant to the prescriptive concerns of the philosopher, C-HA enables realistic and normative conclusions about scientific practice to be drawn from such events. One such conclusion is that scientists, in order to ground their theoretical constructions in empirically testable form, must think in terms of concrete images embedded in conceptual frameworks or schemata. More to the point, cognitive analysis allows the philosopher of science to make some sense out of certain perplexing ideas proposed by Kuhn – for instance, the notion that scientists before and after a scientific revolution invariably operate in different worlds. As Nercessian intimates, "if [scientists] do negotiate the world by constructing mental models, prerevolutionary and postrevolutionary scientists would construct different mental models and would, thus, truly have different experiences of the world." (Nercessian, 1992:36)

The central motivation of this paper is to support the growing tendency in philosophical interpretation of science that employs concepts and ideas from other science studies disciplines to analyse selected aspects of science which traditional logistic philosophers of science rejected as irrelevant to the subject. Fixation with the technical problems encapsulated in the epithet "logic of scientific discovery" prevents philosophers from articulating theories that capture the richness and dynamism of science as a living enterprise undertaken by fallible communities of researchers. To avoid the excesses of logical positivism and kindred philosophies, the philosopher of science should go beyond arbitrary logistic borders by construing the history of science as a legitimate source of problems and of data that can be used to assess philosophical reconstructions of science. Needless to say, while still keeping the normative concerns of philosophy in the foreground, she can extrapolate from theories and insights developed from other science studies disciplines to enrich her work. The interdisciplinary approach here recommended stimulates systematic investigation and normative evaluation of the intellectual processes (and their emotional corollaries) involved in rigorous scientific research. More specifically, it provides avenue for philosophically fruitful investigation of scientific discoveries. Cognitive-historical methodology offers a conceptual tool-kit which can be effectively used to reconstruct episodes of discovery in science within the context of the norms implicit in the social organization of research.

In this paper, we have raised doubts about the appropriateness of construing scientific discovery as something that happens to a scientist at an instant in time, an event without a structure. The outcome of rigorous researches in the history of science has demonstrated clearly the inadequacies in earlier positivistic accounts of scientific development, of which discoveries constitute an important part. Therefore, there is no overarching reason for philosophers to tie themselves down intellectually with the apron-strings of positivism, or maintain that the "context of discovery" cannot be handled in a philosophically intelligible manner. As Kuhn rightly pointed out, "considerations relevant to the context of discovery are...relevant to justification as well...," which means that both are legitimate topics for philosophical inquiry. (Kuhn, 1977: 328) (Merton, 1963; Reif, 1961; Kuhn, 1977, 165-177; Boorstin, 413-417; Cohen, 1985; Nercessian, 1992) After all, the main tasks of the philosophy of science, we must add, are *to enhance comprehensive understanding of science* and *improve scientific practice*, not to *logicise* it.

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