AN INVESTIGATION INTO THE NATURE OF THE KUHNIAN PARADIGMATIC ACCOUNT OF HOW SCIENCE PROGRESSES

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ABSTRACT

Many philosophers of science are in agreement that Thomas S. Kuhn, a trained physicist, is in the area of Philosophy of Science and second only to Karl R. Popper in terms of contribution in the said area. His works "rank second to those of Popper in terms of number and importance" (John Shand, 1993, p.359).

In his work, The Structure of Scientific Revolutions (1962), Kuhn attempts at a correct empirical description of science as it is practiced. In the said work, Kuhn argues that science does not progress cumulatively but by what he calls "Scientific revolutions", by which he means, the throwing away of views that fail to work and replacing them with new ideas. On how these new ideas are chosen, he argues that scientific progress is like religious conversion, which is not deliberative.

Many critics and commentators have found Kuhn's account of science implausible. Amongst his critics, we can mention Karl Popper, Allan Musgrave, Larry Laudan and Imre Lakatos. All these critics are in agreement that Kuhn's account of science may not only be relativistic but it may also lead to scientific irrationalism. Kuhn defends his position in his later work, The Essential Tension (1977). In this work, he argues that his critics have misinterpreted him. He is of the opinion that he has produced a true empirical account of science as it is practiced and how it progresses.

This work is aimed at showing that Kuhn defends what cannot be found in his work, The Structure of Scientific Revolutions (1962). Consequently, we show that Kuhn's account of science does not meet the requirements of the scientific method and consequently, the assumption that Kuhn's account of science reflects the actual practice

of science is untenable. In this respect, we show that Kuhn's account of science is scientifically irrational and that, contrary to Kuhn's claim that science progresses by revolutions, science progresses cumulatively. Inspite of the above shortcomings, we argue that, to the extent that Kuhn's account of science is sociological in nature, then to the same extent, Kuhn's account of science captures the actual practice of science.

In chapter one, I introduce the problem of study. I state the objectives of the study and then review related literature to the study. In chapter two, I give a descriptive account of the major tenets in Kuhn's account of science and then proceed to show the logical structure of Kuhn's account, which is a case of a strong inductive argument. In chapter three, I show that Kuhn's paradigmatic account of science is not scientifically rational since it does not meet the requirements of scientific rationality, that is, that all rational results must be universal, necessary, and determined by rules.

In chapter four, I examine some epistemic necessary conditions stipulated by the scientific method on how science is actually practiced. In this chapter, I show that although Kuhn's account of science does not meet the logical conditions of the scientific method, it nevertheless explicitly presents the role played by the scientific community in the activities labelled under "science". In chapter five, I show that science progresses cumulatively as the history of science can testify. Consequently, I argue that any account of scientific progress which contradicts the cumulative element of scientific progress is not reflected by the history of science, hence it is doubtful whether Kuhn's account of science is a true account of how science progresses.

Finally, I recommend that, first, as scientific progress is cumulative, one needs to re-examine one's metaphysical presuppositions before one rejects alternative approaches to scientific inquiry. Secondly, I recommend that if knowledge is to advance, then, conceptual clarity must be acknowledged. This recommendation is as a result of Kuhn's ambiguous and vague usage of "paradigm".

CHAPTER ONE: INTRODUCTION TO THE STUDY

1.0 BACKGROUND TO THE STUDY

The question of the nature of scientific progress is one that has been of particular interest to philosophers of science, especially in the last two decades. Larry Laudan (1977, p.2), for instance, writes:

> *For a long time, many have taken the rationality and progressiveness of science as an obvious fact or a foregone conclusion, and some readers will probably still think it bizarre to believe that there is any important problem to be solved here. Although this confident attitude has been almost inescapable given the cultural biases in favor of science in modern culture, there have been a number of recent developments that bring it into serious question.*

In the early 1960s, attention was drawn to the question of scientific rationality and progress by the book, *The Structure of Scientific Revolutions* (1962) by Thomas Kuhn, hereafter abbreviated as SSR. This book is "a sustained attack on the prevailing image of scientific change as a linear process of ever-changing knowledge, and an attempt to make us understand that process of change in a different and a more enlightening way" (Shapere, 1984, p.27).

Kuhn's SSR is specifically directed against Karl Popper's work, *The Logic of Scientific Discovery* (1934), in which the latter attempts at a distinction between science and non-science. In this book, Popper argues that what is characteristic of scientific method is not a verificational procedure but falsification. He argues that if a hypothesis is proposed, we should, by observation and experiment, seek to discover a counter-instance, which will conclusively falsify the hypothesis. In the SSR, Kuhn attacks Popper's account of science by arguing that most theories are incomplete or inadequate in certain respects: "no theory ever solves all the puzzles with which it is confronted at a given time; nor are the solutions already achieved often perfect" (Kuhn, 1962, pp.38-39).

Theories give rise to what Kuhn calls "anomalous experiences", that is, failures of data to "fit" theories because they do not measure up to the existing paradigms. Popper argues for a systematic subjection of 'bold conjectures' to 'criticism' with a view to their refutation. He sees this as the normal activity of scientists. By contrast, Kuhn thinks of 'normal science' as being at once a more conservative and ideological activity. Scientists work within a community committed to a shared framework of theory, ideas, and presuppositions, that is, 'paradigms.' The above discussion has been referred to as 'the Popper – Kuhn debate" (John Hrrison Barbet 1990,p.252). The Popper-Kuhn debate will not concern us in this work. Although the debate is still on, our work will centre on Kuhn's account of science with a view to examining whether it reflects the actual practice of science.

In the SSR, Kuhn stipulates how science progresses from what he calls "normal science" to "scientific revolution". Central to Kuhn's interpretation of the history of science is his notion of a "paradigm". Paradigms are "universally recognized scientific achievements that, for a time, provide model problems and solutions to a community of practitioners" (Kuhn, 1962, p.x). Kuhn argues that repeated failures of "a normal science" tradition to solve a problem or other anomalies that develop in the course of paradigm articulation produce "the tradition-shattering complements to the tradition-bound activities of normal science"(Ibid., p.6). The most pervasive of such tradition – shattering activities in the history of science are "scientific revolutions". Kuhn writes:

> *Confronted with anomaly or with crisis, scientists take a different attitude toward existing paradigms, and the nature of their research accordingly.... Scientific revolutions are inaugurated by a growing sense that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way (Ibid, pp. 90-91).*

In the above interpretation of scientific development, Kuhn places "paradigms" central in his conception of science. The question is, "How are 'paradigms' chosen?'' In the case of competing paradigms, Kuhn writes: "Evaluative arguments over the merits of alternative paradigms are vastly minimized, such arguments being circular, and the essential factor consisting not in deliberation or interpretation but rather in the gestalt switch" (Ibid.).

From the above quotation, two theses can be attributed to Kuhn: first, the arguments put forward by scientists in disputes about paradigms are of little importance because they are circular and that "the essential factor" in a paradigm change is not any sort of rational deliberative process but rather a gestalt switch; and second, that conflicting paradigms imply conflicting or alternative modes of conventional activity. Clearly the two sets of conventions (paradigms) cannot themselves offer a suitable basis for their own mutual evaluation. But if there were some external factor, "rationality", which could be used to evaluate the alternative conventions, then one would be bound to wonder why it could not enter into Kuhn's account of science. Kuhn is of the opinion that the choice between alternative paradigms is like a "religious conversion" which calls for no rational thought (Ibid., pp. 198-204).

To accept the independent role of "reason" into the SSR would call into question the whole of Kuhn's view of science, for there is no appropriate scale available with which to weigh the merits of alternative paradigms for they are incommensurable (Ibid., p.72). To favor one paradigm over the other is, in the last analysis, to express a preference for one paradigm rather than another – a preference that cannot be rationalized by any non-circular argument (Ibid., p.94). Thus, Kuhn's argument in the SSR implies irrationalistic science.

Many critics have found Kuhn's account of science implausible. In his book, *Objective Knowledge* (1979), Popper accuses Kuhn of being a relativist on the grounds that, according to Kuhn, a paradigm shift involves not only a change in theory so that data will fit but also a change in the actual definitions of such central terms as 'truth' and 'proof', and indeed perhaps 'nature' itself. In his article, "Kuhn's second Thoughts" (1971), Alan Musgrave points out that Kuhn's paradigmatic account of scientific revolutions may lead to irrationalism. Israel Scheffler also, in his *Science and Subjectivity* (1967), points out that Kuhn's account of science may not only be relativistic but it may also lead to scientific irrationalism.

Kuhn defends his position in his article, "Reflections on my critics" (Kuhn, 1970). He denies that he is a relativist in so far as he is of the opinion that "scientific development is, like biological evolution, undirectional and irreversible. One scientific theory is not as good as another for doing what scientists normally do" (Kuhn, 1970, p.23). In his *The Essential Tension* (1977, p.6), Kuhn argues that philosophers of science have formulated the SSR's philosophical claims in terms of categories and distinctions characteristic of positivist philosophy of science, thereby misrepresenting its actual meaning. As result, Kuhn thinks he has been "widely presented and criticized as defending extreme and implausible philosophical theses that infact cannot be found in this book" (Ibid., p.6). The latter view was pointed out by Musgrave when he wrote, "what does emerge clearly from Kuhn's defense of his early views is that it is a conception to which he does not now, and perhaps never did, subscribe" (1971, p.39).

Although Kuhn denies that his account of science leads to irrationalism, a critical examination of the Kuhnian paradigmatic account of science raises questions as to whether this kind of account of science measures upto the scientific method. The scientific method has been summarized by Dudley Shapere in his article, "The structure of Scientific Revolutions" as: "A fundamental feature of science is its ideal of objectivity, an ideal that subjects all scientific statements to the test of independent and impartial criteria, recognizing no authority of persons in the realm of cognition" (1984, p.1).

The above statement presupposes the following dichotomy concerning the authority of science: the authority by which scientific claims are evaluated is either that of impersonal criteria, that is, methodological rules, in which case science is objective and rational; or that of persons, that is, the subjective preferences of individuals, in which case science is subjective and irrational. A close account of the evaluation of paradigms in the SSR reveals that Kuhn's account implies subjectivism in science. Kuhn may have the psychological disposition that his account of science is rational (Kuhn, 1977, p.6), but close examination reveals that, epistemologically, Kuhn's philosophy of science presupposes irrationalism.

In the light of the foregoing discussion, the researcher attempts at a clear exposition of the relativistic charges, which have been put forward against Kuhn. Of particular interest to the researcher is an attempt at an examination whether the Kuhnian paradigmatic account of science reflects the actual practice of science. Kuhn was trained as a physicist and has been influential in the Philosophy of both the physical and social sciences; in this respect he is second only to Popper (John Shand, 1993, P.308).

1.1 STATEMENT OF THE PROBLEM

In the SSR (1962), Kuhn claimed to have captured correctly how science is practiced. However, his critics such as Musgrave (1971) and Shapere (1984) argued that Kuhn's account is far from being a true account of how science is practiced. Consequently, this led to a philosophical dispute on whether or not Kuhn's work was a correct interpretation of how science is practiced.

In the light of the foregoing debate Kuhn published his book *The Essential Tension* (1977) to defend his position in his earlier book-the SSR.

In the context of this debate, this study is a philosophical analysis to determine whether or not Kuhn's SSR is a correct empirical description of how science is practiced. Specifically, the study is an attempt to examine whether or not Kuhn's account reflects how science is practiced in terms of scientific rationality and scientific progress.

1.2 OBJECTIVES OF THE STUDY

This study aims at achieving the following objectives:

(a) To determine whether or not Kuhn's account of science does meet the requirements of the scientific method and whether or not Kuhn's account of science reflects the actual practice of science. In this respect, I specifically want to examine whether or not:

i) Kuhn's account of science is scientifically rational; and,

ii) Science progresses by revolutions as claimed by Kuhn or cumulatively as Popperians have claimed.

(b) To argue that, to the extent that Kuhn's account of science is sociological in nature, to the same extent, it captures the actual practice of science.

1.3 JUSTIFICATION OF THE STUDY

John Shand, in his work, *Philosophy and Philosophers (1993)*, argues that Kuhn can be rated the second most important Philosopher of Science after Popper. His argument is based on the fact that Kuhn has written many works in the said area and that his works rank second to those of Popper in terms of number. Shand further argues that all the other writers in the philosophy of science have either made replies or taken sides on the "Popper-Kuhnian debate". Shand's argument justifies the choice of Kuhn for a case study in the history and nature of science.

One of the fundamental tasks of philosophy is to take the concepts that we daily use in everyday life and science, analyze them, and determine their precise meanings and their mutual relations. Evidently this is an important duty. In the first place, clear and accurate knowledge of anything is an advance on a mere general familiarity with it. Moreover, in the absence of clear knowledge of the meanings of the concepts that we use, we are certain, sooner or later, to apply them wrongly or to meet with exceptional cases where we are puzzled as to how to apply them at all. I, thus, subject the Kuhnian paradigmatic account of science to the same; to clarify its meaning.

The leading intellectual spokesmen of the scientific method are in agreement that "science" is essentially a problem-solving activity (Laudan, 1977; Lakatos, 1978; Nagel, 1961). The major methodological premise of the scientific method is the proposition that science may be viewed extrinsically and that this reveals them as phenomena, which are functionally related to various facets of social reality. The methodological premise recommends itself because it promises to be a useful and timely guide to research. That the scientific method can be used to solve scientific problems raises the methodological problems of standards: What are the criteria for rating the importance of problems in science? When is a "problem" scientific? Now a precise and clear understanding of the actual practice of science can help in identifying the standards that can be employed in evaluating the merits of scientific problems in the changing world.

1.4 LITERATURE REVIEW

In this work, the literature review will be centered on two themes. First, I deal with definitions of key concepts, namely, "Kuhnian paradigms" and the "scientific method". This is for clarification purposes. Secondly, critics of the Kuhnian account of science will then be dealt with.

Kuhn is majorly known through his monumental work, the SSR (1962). He has, however, written many other works but I will be concerned with those in which he attempts at an account of science. It is convenient to divide his publications into three classes. First, there are concrete historical narratives, produced in the 1950s and the early 1960s, and addressed mainly to professional historians of science: particularly noteworthy here are *The Copernican Revolution* (1957), and a series of papers on the history of thermodynamics.

Second, there are publications, beginning around 1960, which represent an attempt to understand science in general terms and to identify its distinctive features. This is the work through which Kuhn is most widely known, and in which most of his philosophical ideas are to be found. It includes his books, the SSR (1962) and *The Essential Tension* (1977) in which he attempts at a defense of his early ideas after a series of criticism. Finally, there is a work, which reflects the detailed attention Kuhn has devoted in recent years to the history of quantum mechanics. *Black Body Theory* (1978) is the major contribution to date. Most of our citations relate to the wide- ranging and comparatively speculative material of what perhaps should be called Kuhn's "second period".

The SSR is about the authority of science. In it, Kuhn (1962) proposes a new interpretation of this authority that most readers have found either plausible or challenging. The proposal is that science's authority ultimately resides not in a rule-governed method of inquiry whereby scientific results are obtained but in the scientific community that obtains the results (Garry Gutting, 1980, p. 1). The scientific community is determined by the paradigm the community members share. Phenomena, which do not fit the expectations generated by the paradigm, become puzzles to be solved by the proper application of the paradigm.

The usage of paradigms in scientific research becomes suspect when we ask about the grounds for accepting one paradigm as better than another. For if "the difference between successive paradigms are both necessary and irreconcilable" (Ibid., p.102), and if those differences consist in the paradigms being "incommensurable" then there is no common battleground for the two paradigms to compete. In other words, why does one win? The logical tendency of Kuhn's position is clearly toward the conclusion that the replacement of one paradigm by another is not cumulative, but is mere change. The researcher is of the opinion that although Kuhn denies that crucial scientific decisions are nonrational, there are textual bases for Kuhn's critics to interpret him as irrationalistic.

Kuhn (1977) tries to defend his paradigmatic account of science as not being irrationalistic. In this work, Kuhn writes:

> *Subtle analytic distinctions that had escaped the historians would often be central when the philosophers reported on their reading. The resulting confrontations were invariably educational for the historians, but the fault was not always theirs. Sometimes the distinctions dwelt upon by the philosophers were not to be found at all in the original text. They were the products of the subsequent development of science or philosophy, and their introduction during the philosopher's processing of signs altered the argument (Ibid., p.6)*

Central to Kuhn's argument in the above passage is the idea that the ideas in the SSR have been disorted by philosophers. Kuhn's argument is that many of his philosophical critics have misread his SSR and that they do not make any specific textual reference from the SSR.

It is, however, questionable as to whether or not Kuhn himself is responsible to some extent for being misunderstood. For example, he compares scientists' changes of paradigm to religious conversions and that those working with different paradigms "live in different worlds". He cites with approval Max Planck's dictum that, new views triumph only by the deaths of their opponents. It is easy to see how philosophers, especially those imbued with positivism, would be likely to read him as presenting an irrationalistic philosophy, and Kuhn does nothing in the SSR to prevent this sort of misunderstanding.

Douglass Lee Eckerg and Lester, Jr. (1980) in their article "The Paradigm Concept and Sociology: A critical Review" share Kuhn's views that a distinction cannot be drawn between science as a cognitive system and science as a social system and thus opening up the possibility of sociological studies of the development and evaluation of specific ideas.

Michael D. King (1980) in his article "Reason, Tradition and Progressiveness of Science" argues that the behavioural or functional approach to sociology of science can be traced back to its roots in R. K. Merton's efforts to weld together an anti-rationalistic sociology and a rationalistic view of science. King's position can be put thus: scientific rationality comes from a particular point of view constituting a self-authenticating tradition of thought. Thus King a Kuhnian in reasoning.

According to Carl Kordig (1971), revolutionary new idea s concerning science have recently been advanced by Feyerabend (1975) Kuhn (1962) ,and others. He argues that the claim that there are pervasive presuppositions fundamental to scientific investigations seems to be essential to the views of the said men. He further argues that transitions from one scientific to another force radical changes in what is observed, in the meanings of the terms employed, and in the metastardndards involved. In other words, Kordig is a Kuhnian sympathizer.

Kuhn has been referred to, as an "irrationalist" philosopher of science because he believes that science cannot be governed by rules as set in the scientific method. Advocates of the scientific method argue that any "science" worthy its name must adhere to the scientific method. Abraham Kaplan in his book, *Sociology learns the Language of Mathematics*, argues that the "scientific method is the same everywhere: it is the method of logical inference from data provided and tested by experience" (Ibid., 1952, p.39). It is questionable whether Kuhn' account of science is based on "logical inference from data provided and tested by experience" given that Kuhn believes and shows, that they do not appear at all in scientific revolutions.

Robert Oppenheimer (1947), in his article "The Scientific Foundations For World Order," observes that, in science itself we have a limited but magnificent example of a real fraternity. He argues that real scientific fraternity is based on the scientific method. Given the evaluation of paradigms, it is questionable whether Kuhn's work can be included in this fraternity called science. In the first place, Kuhn rejects the scientific method. This creates the impression that his scientific work has no membership rights into the scientific fraternity.

According to Ernst Mach, in his article, "The Economy of Science" (1902), any science worthy of its salt must follow the scientific method since this method stipulates rules, which can be used to cross-examine the plausibility of the concerned scientific claim. Consequently, any "science" which follows the scientific method is "communicated by instructions in order that one man may profit by the experience of another and be spared the trouble of accumulating it for himself; and thus, to spare posterity, the experiences of whole generations are stored up in libraries". Given that Kuhn is of the opinion that "science's authority ultimately resides not in a rule-governed method of inquiry whereby scientific results are obtained but in the scientific community that obtains the results," it is doubtful whether his account is not relativistic and hence not objective since it does reflect the actual practice of the scientific method.

Having clarified major concepts in our work, we now wish to review critics who have written on the Kuhnian paradigmatic account of science. The criticism which has been levelled against Kuhn centres on the "Popper – Kuhn debate" on the nature of scientific progress. It is the wish of the researcher to review only the critics who have written on Kuhn's account of science. These include Wolfang Stegmuller (1976), Isreal Scheffler(1967) and Imre Lakatos(1970), among others.

According to Stegmullar (1976), in his book, *Accidental Theory Change and Theory Dislodgment*, Kuhn's account of science is a "bit of musing" by a "philosophical incompetent'', and suggests that "experts" will be able to close the "rationality gaps" which it appears to open. It is noteworthy, however, that Stegmullar himself, a professional epistemologist, is completely unable to do that. He excuses his failure by suggesting that the field in which he is an expert is still in its "infancy" (Ibid.; p.269, p.246). Another way of interpreting his failure may be a sign of the soundness of Kuhn's "bit of musing". If, according to him, only "experts" are able to close the "rationality gaps", it is doubtful whether Kuhn will succeed where Stegmullar has failed, given that the latter is an expert in the above sense.

Israel Scheffler (1967) points out that Kuhn's account of science, when carefully studied, may lead to irrationalism. He does not show how this may be the case. His main concern is the circularity, which he finds in Kuhn's argument. He claims that according to Kuhn, all arguments in paradigm disputes are circular. His pointing to the irrationality of Kuhn's account of science will concern us in our study.

In their article, *Criticism and the Growth of Knowledge* (1970), Imre Lakatos and Allan Musgrave argue, that if there were scientific revolutions, then the growth of knowledge would insufficiently be determined by "rules of reason." It is thus open to "religious maniacs" to justify their irrationalism by pointing to its existence in science itself (Ibid.; p.93). Similarly, if normal science exists, then it is well highly impossible to demarcate scientific from customary activity. They conclude that normal science must not exist. What Kuhn means by "normal science" must then be redefined as unscientific. Gauged by the scientific method, it is questionable whether Kuhn's paradigmatic account of science attains a scientific status.

According to both John Watkins and Karl Popper, Kuhn's account of science is not scientific at all. According to Watkins, in his article, "Against Normal Science" (1970), the greatest critic of the Kuhnian paradigm was Popper. Watkins reports: "the condition which Kuhn regards as the normal and proper condition of science is a condition which, if it actually obtained, Popper would regard as unscientific" (1970, p.8). Popper himself attests to the above claim when he reports, "what Kuhn describes as normal science is a phenomenon which I dislike (because I regard it as a danger to science) while he apparently does not dislike it (because he regards it as "normal") (Popper, 1970, p.52). Central to Popper's charge is that Kuhn's account of science does not pay any respect to the scientific method. I back Popper's charge against Kuhn that the scientific method is essential in science since it does not only set goals to be sought in scientific performance, but it also provides a basis for adjusting results obtained. The researcher is of the opinion that Kuhn's account of science seems not to reflect the scientific method.

In his book, *Personal Rationality* (1995), Jozef Misiek discusses the requirements of "Scientific rationality". In this book, he identifies scientific activity with rational activities, which must be guided by necessary and universal rules. In other words, Misiek is in agreement with the scientific method that, rational decisions must be necessary, universal, and governed by rules. The idea of a mutual cross control promoting truth in science and progress in society is an influential suggestion first made by Michael Polanyi. Truth would be not a property of a belief, but a result of a confrontation leading to consensus between members of a community". "The consensus between members of a community" that Misiek is talking about is universal rules as stipulated by the scientific method. The author maintains, along Misiek's reasoning that scientific rationality must be based on the scientific method. I shall subject Kuhn's work to the same method to determine whether it is scientifically rational.

Harold Brown (1990) stipulates the conditions of scientific rationality. He argues that a belief or decision is rational if it conforms to a set of criteria, and if the same criteria are applicable in every context; rational individuals need not debate over which criteria should be applied. If alternative criteria are admitted, we may find ourselves having to choose between them, and we will need some way to make this choice on a rational basis. Brown continues to argue that even if we attempt to restrict different criteria to different domains, decisions must be reached on a rational basis, that is, through reasoned out rules. He adds, "When we have such principles we know what we are doing, and in their absence we might encounter situations in which we have no coherent basis for making an important decision" (p.13). Brown adds that all "rational thinkers" must arrive at the same conclusion. But it is not enough that all rational thinkers arrive at the same conclusion since this might occur as a result of a massive coincidence, rather than through reasoning.

In reply to the foregoing objection, Brown argues that all rational decisions must be necessarily universal. He writes, "the existence of a necessary tie between the available information and a rationally acceptable result allows us to understand why all rational individuals who start at the same point must arrive at the same conclusion" (p.14). Brown's position can be summarized using Immanuel Kant's position in the *Critique of Pure Reason* (1963,pp.43-43) in the following manner: a rationally acceptable conclusion must follow with necessity. I will subject Kuhn's account of science to the three requirements of rationality to gauge the extent to which it meets the said are requirements.

In his book, *Theory of Knowledge* (1977), Roderick Chisholm argues that, what makes a premise rationally acceptable is the extent to which it has been accepted on the basis of "appropriate rules". He writes, "we consider certain things that we know to be true, or think we know to be true, or certain things which, upon reflection, we would be willing to call evident." He continues to argues that, with respect to the above, we then try to formulate a reasonable answer to the question, "What justification do we have for thinking you know this thing to be true?" or "What justification do you have for counting this thing as something that is evident?" Chisholm's argument can be briefly put thus; "We should only believe propositions that are justified and in the absence of sufficient justification we must suspend belief". I will follow Chrisholm's argument and support our view that scientific propositions must be subjected to justification if they must be believed.

Imre Lakatos in his article, "Falsification and the Methodology of Scientific Research Programme" (1970) has produced a theory, which nears the Kuhnian paradigm. According to Lakatos, a "research program" provides the extra-logical guiding principles that organize scientific research. This is a set of hypotheses that guides research in a discipline much the same way that a Kuhnian paradigm does, but these hypotheses are explicit propositions that are divided into sets that do different jobs. One of these sets embodies what Lakatos calls the "hard core" of the programme. This is a set of claims about nature that form the heart of the programme, and any alternation of this hard core amounts to the abandonment of that programme.

When problems arise, scientists seek to protect the hard core by modifying some of the claims in the protective belt. A research program provides a 'positive heuristic' and a 'negative heuristic.' The former is a set of suggestions as to how to proceed in deploying the program as a means of understanding nature. The negative heuristic is a set of injunctions as to how we should not proceed in developing the program, and seems to amount to the general methodological demand that the hard core shall be protected.

The following example will serve to clarify what Lakatos is driving at. Ancient astronomy may be viewed as a research program for understanding and predicting planetary motions with two theses constituting its hard core: that the earth is stationary, and that all planetary motions are circular. In addition, there were a number of other principles that were added to this hard core at various times, and that were modified or abandoned as astronomers attempted to account for the observed planetary motions. Although he does not call his theory a paradigm, Lakatos' research programs have all the characteristics of the Kuhnian paradigms.

Like Lakatos, Laudan (1977) argues that research is typically guided by a set of principles that transcends the particular theories in a discipline. For Laudan, a research tradition is a set of ontological and methodological 'dos' and 'don'ts' (P.80) that become instantiated in a sequence of theories, and that can undergo change as the discipline develops. Like Lakatos', Laudan's research tradition is more Kuhnian than he may want to admit.

Ernest Nagel (1961) observes that, the desire for explanations, which are systematic and controllable by factual evidence, generates science. He adds that, this is the organisation and classification of knowledge on the basis of explanatory principles, which is the distinctive goal of science. He holds that sciences seek to discover and to formulate, in general terms, the conditions under which events of various sorts occur, the statements of such determining conditions being the explanations of the corresponding happenings. Nagel's position on science is only one aspect of the scientific method – the mechanical principle of the scientific method, which will be dealt with in Chapter Four of the current work.

At the end of an extended attack on the thesis that science is guided by a universal method, Paul Feyerabend (1975, P.302) writes,

> *If science has found a method that turns ideologically contaminated ideas into true and useful theories, then, it is indeed not mere ideology, but an objective measure of all ideologies.... But the fairy tale is false, as we have seen. There is no special method that guarantees success or makes it probable... Basically there is hardly any difference between the process that leads to the announcement of a new scientific law and the process preceding passage of a new law in society....*

From the above passage, it should be noted that Feyeraband was a critic of the scientific method with the understanding that the 'scientific method is a tale' which is non-existent. In later stages of this work, I will explicate the new principles of the scientific method thereby 'proving that the scientific method is existent'.

Focusing on scientific hypotheses, Richard Rudener (1966,P.6) writes,

> *Now, in general, the context of validation (i.e.; justification) is the context of our concern when, regardless of how we have come to discover or entertain a scientific hypothesis or theory, we raise questions about accepting or rejecting it. To the context of discovery, on the other hand, belong such questions as how, in fact, one comes to latch onto good hypotheses, or what social, psychological, thinking up fruitful hypotheses.*

The gist of the above quote is that scientists come up with hypotheses, but the fact that someone has thought of a hypothesis does not provide a reason for accepting it. Scientists decide whether a claim is scientific by subjecting it to empirical tests. The rules of scientific method determine which tests are relevant, and whether a body of empirical results is sufficient for accepting or rejecting a proposal, or whether judgement should be suspended pending further investigation. The current study is conducted along the same reasoning, that is, that "anything scientific" must follow the rules of the scientific method.

In the *Logic of Scientific Discovery* (1934,P.31), Karl Popper tells us:

> *The initial stage, the act of conceiving or inventing a theory seems to me neither to call for logical analysis, nor to be susceptible of it. The question how it happens that a new idea occurs to a man – whether it is a musical theme, a dramatic conflict, or a scientific theory – may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge.... As to the task of the logic of knowledge – in contradistinction to the psychology of knowledge – I shall proceed on the assumption that it consists solely in investigating the methods employed in*

those systematic tests to which every new idea must be subjected if it is to be seriously entertained.

The central argument in the above passage can be put thus: "the process of discovery is not as important as the justification of the discovery through the scientific method". In other words, Popper is saying that it does not matter much how discoveries are made. What matters, according to Popper, is "the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained. In the same book, Popper concludes that the only distinguishing mark between science and non-science is the scientific method.

1.5 THEORETICAL FRAMEWORK:

This study will be guided by the scientific approach. Philosophers of science are generally in agreement that the scientific method is objective in nature (Popper, 1950; Laudan, 1977; Lakatos, 1978). This objectivity is what has been called "the public character" of the scientific method, which is achieved by recognizing experience as the impartial arbiter of controversies in science. An experience is "public" if everybody who takes the trouble can repeat it. In order to avoid speaking at cross-purposes, scientists try to express their theories in such a form that they can be tested, that is, refuted or otherwise confirmed by such experience.

In the scientific method, everyone who has learned the technique of understanding and testing scientific theories can repeat the experiment and judge for himself or herself. Inspite of this, there will always be some who come to judgements, which are partial. This cannot be helped, and devices have been set as counter checks to partiality. Various institutions such as laboratories or other means, such as the scientific periodicals, scientific congresses, and scientific papers, have been designed to further scientific objectivity and impartiality. This aspect of the scientific method shows what can be achieved by institutions designed to make public control possible, and by the open expression of public opinion, even if this is limited to a circle of specialists.

Karl Popper, in his article, "The sociology of knowledge: A critique," has summarized the scientific method in this manner:

> *... it may be said that what we call "scientific objectivity" is not a product of the individual scientist's impartiality, but a product of the social or public character of scientific method; and the individual scientist's impartiality is, so far as it exists, not the source but rather the results of this socially or institutionally organized objectivity of science (1970, p.6).*

After a common consensus among scientists, the results are then put into application. Practical application of the findings is the means by which we may eliminate irrationalism from the sciences, because, as Abraham Kaplan, in his book, *Sociology Learns the Language of Mathematics* says: "the scientific method is the same everywhere; it is the method of logical inference from data provided and tested by experience" (1952, p.39.

D.W.Y. Kwok (1965, pp.21-22) has succinctly summarized the method of science as follows:

> *The scientific method operates on fundamental principles. First, the need for observation; the empirical principle. Second, to achieve exactitude in measurement ... he must employ quantitative means; the quantitative principle. Third, he deals with causal relations and often uses abstractions to represent them. For this end, he must locate meaningful recurrences of behaviour and then formulate general laws or equations, which describe and explain such behaviour: the mechanical principle of science. Fourth, is a general assumption of all scientists which may be called an attitude of mind, a principle inherent in the concept of research: the principle of progress through science... co-operation for non-personal ends, a co-operation in which all scientists of the past, present, and the*

future have a part.

Central to the above quotation is the view that there are four principles which an activity labelled under "science" must follow. Each of the four mentioned principles: [a] the empirical principle, [b] the quantitative principle, [c] the mechanical principle and [d] the co-operative principle, has its own characteristic procedures. The said procedures will be dealt with singly in chapter four of this study. The Kuhnian paradigm will then be subjected to these principles, to determine to what extent Kuhn follows them.

A summary of each of the four principles would run thus: the empirical principle deals with how the scientist obtains the empirical facts. The quantitative principles employs measurement through which a scientist arrives at quantitative estimates of the variables and magnitudes considered in their hypotheses. The mechanical principle can be summed up as the search for laws of nature, which govern infirmities in the universe. The co-operative principle is where in the attempt to overcome prejudice and to gain objectivity, members of the scientific community set forth versed and competing hypotheses and then wait the confirmations or rejection of these hypotheses by others.

1.6 HYPOTHESES

The following two negative and one positive hypothesis are posited:

First, Kuhn's account of science is at variance with scientific rationality.

Second, Kuhn's account of science is not scientifically progressive.

Third, scientific practice is sociologically determined. That is, all activities under the label "science" are socially determined.

1.7 METHODOLOGY

The research will rely on secondary/library sources. It will consist of a critical survey of written works on the Kuhnian paradigms and the scientific method. In conducting this research, the study will employ the descriptive, critical, and evaluative methods. In the descriptive method, the study will first present distinctly Kuhn's theory of science by stating its characteristics. By "descriptive'' we mean a factually grounded or informative presentation. This is opposed to the "normative'', "prescriptive" or "emotive" approach, which entails making judgements. The descriptive approach is for portrayal purposes.

The critical method involves an analysis of the Kuhnian account of science in relation to the scientific method. The approach will point out inconsistencies of the Kuhnian paradigms when gauged by the scientific method. The study will also point out the strengths of the Kuhnian account of science as a putative model of actual practice of science. Hence the critical method will be both negative and positive.The evaluative method will consist of examination and judgement of the Kuhnian paradigmatic account of science concerning its quality and significance as a model of the actual practice of science. The research will assess, appraise, or rate the extent to which the Kuhnian paradigmatic account reflects the actual practice of science.

CHAPTER TWO: KUHN'S PARADIGMATIC ACCOUNT OF SCIENCE

2.0 Introduction

The Structure of Scientific Revolutions (1962) hereunder referred to as SSR is about the authority of science. In this work, Paul Kuhn proposes a new interpretation of science. The proposal is that the authority of science ultimately resides not in a rule-governed method of inquiry whereby scientific results are obtained but in the scientific community that obtains the results. Kuhn embedded this proposal in a general account, illustrated and supported by specific episodes in the history of science, of the way scientific results are developed and abandoned (Gutting1980, p. 1).

Central to Kuhn's account of science is the concept of paradigm, which he defines as a universally recognized scientific achievement that, for a time, provides model problems and solutions to a community of practitioners (Kuhn, 1962, p.x). Kuhn argues that repeated failures of 'normal science' tradition to solve a problem or other anomalies that develop in the course of paradigm articulation produce "the tradition-shattering complements to the tradition bound activities of normal science" (Ibid., p.6). The result of paradigm-change is what Kuhn calls scientific revolutions (Ibid., pp.90-91).

The essence of Kuhn's position can be put in a few words: By denying that science's authority ultimately resides in a rule-governed method of inquiry, Kuhn argues that scientific knowledge comes from a succession of points of view, each point of view constituting a self-authenticating tradition of thought. Rules for doing science or standards of scientific judgement are not, therefore, absolutes as positivistic science claims, they are relative to a particular theoretical viewpoint. So scientific choice is only rational within the context of a single viewpoint of unquestioned authority; choice between alternative viewpoints (or paradigms), though constrained by logic and observation, necessarily involves "an element of arbitrariness", it is in the last resort a non-rational social act, an act of faith likened by Kuhn to religious conversion (Ibid., p.151).

Having cut himself from the notion that scientific progress is rooted in its logical character or in its methodology, Kuhn seeks to show that it is, at least, guaranteed by its social character, that is, by the nature of science as a social system. For him, the final constraint upon scientific choice is a social rather than a logical one: the final arbiter is the professional judgement of the scientific group. If a scientific community can be persuaded of the necessity of discarding their commitment to one fundamental standpoint, or paradigm, in favour of another, then this, in itself, is sufficient to provide a "virtual guarantee" that change will be progressive.

Kuhn's conception of science has affinities with other philosophers such as Michael Polanyi (1962), Paul Feyerabend (1975), and Stephen Toulmin (1972). Although they do not employ the concept "paradigm", these writers are sympathetic to Kuhn's view that much of the scientists' success depends upon "tacit knowledge", that is, upon knowledge that is acquired through practice and that cannot be articulated explicitly. These philosophers seem to put forward the question by Ludwig Wittgenstein in the *Philosophical Investigations:* In the absence of a competent body of rules what restricts the scientist to a particular normal – scientific tradition? (Wittgenstein, 1953, p.31).

Unlike the foregoing philosophers, what Kuhn did was to make explicit use of the principle of induction from which he drew inferences in his attempt to show that science progresses through revolutions. " Induction" is used here in the sense of Brian Skyrms (1966), who defines inductive reasoning as a logical argument in which it is improbable that the conclusion is false while the premises are true and the argument is not deductively valid. The degree of inductive strength depends on how improbable it is that the conclusion is false while the premises are true. This is contrasted with deductive reasoning in which the link between the premises and the conclusion is so strong that it cannot be broken. The strength of the claim about the link between the premises and the conclusion of the argument is what differentiates the two arguments. This is opposed to the old distinction that deductive reasoning proceeds from the general to the particular while inductive reasoning proceeds from the specific to the general. This is because logical arguments can be advanced, both general to general and specific to specific in addition to the above distinction into which both fit (Skyrms, 1966, pp. 13-14). Before it is shown how Kuhn engaged in a strong inductive reasoning, his theory of scientific progress through revolutions is first presented by explicating the major tenets used in it.

After a detailed study on the history of science (during a research sponsored by the University of Chicago during the period 1962-1965), Kuhn summed up his findings in the principle of "scientific progress through revolutions" Kuhn, 1962, p.iv) which he says occurs from time to time to bring scientific progress. These may be divided into four tenets, for easy comprehension, and then dealt with singly, as hereunder.

2.1 Paradigms Dominate Science

Kuhn maintains that a cluster of very broad conceptual and methodological presuppositions implied in a paradigm dominates every professional community in the "standard examples" through which students learn the prevailing theories of the field. The history of science is replete with examples. The foregoing claim is illustrated with the following two examples:

The first, example is drawn from astronomy as presented by W.T. Jones (1952, pp.613-634). A student who is to be initiated into the community of astronomers would have to learn the following paradigm shifts. The sun appears to rise from the east and set in the west. Ptolemy of Alexandria offered a hypothesis affirming that the earth is fixed and stationary at the centre of the solar system around which the other planets revolve. With increasing astronomical knowledge, Nicolaus Copernicus of Thorn, Italy, in his *De Revolutionbus Orbium Coelestium* (1543), that was dedicated to Pope Paul III, proposed to consider the sun as the centre of the solar system. The new theory fitted the facts better – but not well enough. John Kepler, in his *The New Astronomy or Celestial Physics, in commentaries on the motions of Mars* (1609), substituted the eclipses for circles as the paths of the planets.

Isaac Newton, in his *Philosophie Naturalis Principia Mathematica* (1687), proposed the law of gravitation whereby every object in the universe attracts every other object with a force varying increasingly to the square of the distance. The French astronomer, John Leverrier's observations revealed that mercury does not move in accordance with Newton's laws; this led Albert Einstein in his "What I Believe" (1930) to come up with the theory of relativity, which explains the erratic behaviour of mercury. The student of astronomy would also have to be well versed with the discovery of planets. He/she would be expected to be acquainted with recent reports in the electronic media that one planet has been discovered.

The second example is drawn from biological science. In the second half of the Eighteenth Century it was generally believed that the earth had been created fairly recently. The estimates of how long the earth has been in existence were based on calculations using data found in the Bible. The age of the earth was computed from biblical chronology. Although there were some differences in interpretation, the estimated age was no more than a few thousands of years. All species of living things were held to have been created strictly according to the record of Genesis and to have remained unchanged ever since. This was the idea of the fixity or immutability of species. Comte de Buffon (1797-1888) was an early doubter of the doctrine of the fixity of species. He suggested that organic life have a common ancestry. Charles Darwin's (son of a medical doctor) grandfather, Erasmus Darwin, in his book *Zoonomia*, or *the Law of Organic Life*, published in 1794, says that warm-blooded animals have arisen from one living filament which acquires new parts and improves with time. In 1809, Jean Baptise Lamarck published his Zoological Philosophy in which he querried the doctrine of fixity of species. Lamarck proposed a mechanism for evolution, which suggested that by the direct action of organs by species, changes occurred which, could be transmitted to the offspring. This is the theory of inheritance of acquired characteristics.

In 1844, Robert Chambers published a book, *Vestiges of the Natural History of Creation*. In this book, he strongly supported the idea of evolution and advanced a mechanism, which had a strong Lamarckian content. Charles Darwin was also doubtful of the idea of the fixity of species and he was inclined to the idea of evolution and he set to find out by what mechanism evolution occurs.

For any student to be admitted to the community of evolutionists, he/she must understand all the paradigm shifts, which the theory has undergone as outlined above. The above examples also serve as norms of what constitutes good science; they transmit methodological and metaphysical assumptions along with the concepts. A paradigm, such as Newton's work in mechanics, implicitly defines for a given scientific community the types of questions that may legitimately be asked, the types of explanations that are to be sought, and the types of solutions that are acceptable. According to Kuhn, such a paradigm moulds the scientists' assumptions as to what kinds of entity there are in the world and the methods of enquiry suitable for studying them. Kuhn (1962, p.10) writes: "Some accepted examples of actual scientific practice – examples which include law, theory, application and instrumentation together – provide models from which spring particular coherent traditions of scientific research".

Central to the above quotations is Kuhn's claim that it is a paradigm, which determines and governs a scientific tradition (Ibid. p.24). The scientific tradition, to Kuhn, is a synonym for 'normal science' and it consists of work within the framework of a paradigm, which defines a coherent research tradition. Scientific education is an induction into the habits of thought and activity presented by textbooks, and an initiation into the practice of established scientists. Like solving a puzzle or playing a game of chess, normal science seeks solutions within an accepted framework; the rules of the game are already established. A shared paradigm creates a scientific community – a professional grouping with common assumptions, interests, journals and channels of communication.

2.2 Scientific Revolutions as Paradigm – Shifts

Kuhn holds that in normal research, fundamental assumptions are not questioned. For instance, before Comte de Buffon (1797-1888), the idea of the fixity or immutability of species reigned unquestioned. Before Copernicus proposed in 1543 to consider the sun as the centre of the solar system, the Ptolemaic hypothesis affirming that the earth is fixed and stationary as the centre of the solar system while the other planets revolve around it was dominantly unquestionable.

According to Kuhn (Ibid., pp. 52-53), anomalies are set to one side, or are accommodated by *ad hoc* modifications. But with a growing list of anomalies, a sense of crisis leads the scientific community to examine its assumptions and to search for alternatives. A new paradigm, which challenges the dominant presuppositions, then, replaces the old one, which means that it is not merely one more addition to a cumulative structure of ideas.

Consider that example from astronomy where the sun appears to rise in the east and set in the west. The fixed stars do the same. The moon and the planets, which the early Greeks called the "wanderers", also rise and set but irregularly. How shall all this be explained? The Greeks themselves wrestled with the problem for several hundred years and concluded that the motion of the stars, the sun and the earth was directed by the "moods" of the gods (Falckenberg, 1892; p.8). Later a new paradigm was proposed – the Ptolemaic explanation of the motions of all stars. According to Kuhn, the change from the Greek mythology to the Ptolemaic account was not a cumulative structure of ideas. This is because paradigms are incompatible. For a period, adherents of two different paradigms may be competing for the allegiance of their colleagues, and the choice is not unequivocally determined by the normal criteria of research. Kuhn (Ibid., p.147) writes:

> *Though each may hope to convert the other to his way of seeing his science and its problems, neither may hope to prove his case. The competition between paradigms is not the sort of battle that can be resorted by proof.... Before they can hope to communicate fully, one group or the other must experience the conversation that we have been calling a paradigm shift. Just because it is a transition between incommensurable the transition between competing paradigms cannot be a step at time, forced by logic and neutral experience. Like a gestalt switch it must occur all at once or not at all.*

Central to Kuhn's claim in the above passage is the view that "the competition between paradigms is not the sort of battle that can be resolved by proof...." In other words, scientific change cannot be reduced to rules of logic and the so-called "scientific method". As Kuhn portrays it, a paradigm shift is thus a highly subjective process. He claims that scientific revolutions, like political revolutions do not employ the normal methods of change (Ibid., p.149). Such an analysis raises the difficulty of identifying when a change is a "revolution" and when it is not, an issue that will be dealt with in the succeeding chapters.

2.3 Observations are Paradigm-dependent

'Observation' is a very important stage in the development of any science since it provides the "raw materials" for that science. Kuhn agrees with Feyerabend (1975) that there is no neutral observation language. He argues that paradigms determine the way a scientist sees the world. For instance, Galileo saw a swinging pendulum as an object with inertia, which almost repeats its oscillating motion; his predecessors, inheriting the

Aristotelian interest in progress towards final ends, had seen a pendulum as a constrained failing object, which slowly attains its final state of rest.

Scientists with rival paradigms as the case with Galileo and his predecessors, argues Kuhn (Ibid., p.10), may gather quite dissimilar sorts of data; the very features which are important for one may be incidental to the other. The analogy of the seven blind men of India will suffice as a good illustration. Each man had his own 'view' of the elephant. The *elephant* will here stand for the *world* while the *seven blind men* will stand for the *observational stances* by different philosophers, each according to his paradigm. Rural paradigms, says Kuhn, solve different types of problems; they are, like Feyerabend's basic theories, "incommensurable" (Ibid., p.10).

2.4 Criteria are Paradigm-dependent

According to Kuhn, competing paradigms offer differing judgements as to what sorts of solutions are acceptable. He continues to argue that there are no external standards on which to base a choice between paradigms, for standards are themselves products of paradigms. He adds that one can assess theories within the framework of a paradigm, but in a debate among paradigms there are no objective criteria. Paradigms cannot be falsified and are highly resistant to change. Adoption of a new paradigm is a 'conversion'. Each revolution, says Kuhn (1962, p.6):

> *...necessitated the community's rejection of one time-honoured scientific theory in favour of another incompatible with it. Each produced a consequent shift in the problems available for scientific scrutiny and in the standards by which the profession determined what should count as an admissible problem or a legitimate problem-solution. And each transformed the scientific imagination in ways that we shall ultimately need to describe as a transformation of the world within which scientific work was done.*

Yet chapter (XIII) of the SSR he does state that there are reasons, even "hard headed arguments", for the adoption of a new paradigm. Its proponents must try to show that it can solve the problems, which led to the crisis of the old paradigm. They can sometimes point to quantitative precision or to the prediction of novel phenomena not previously suspected.

From the above passage, it is clear that if observation as well as criteria are paradigm-dependent, then there is no rational basis for choice among competing paradigms; each paradigm determines its own criteria, so any argument for it is circular. It is on this point that Kuhn's critics are most vehement, accusing him of relativism, subjectivism and irrationalism, subjects that will be dealt with in detail in the later stages of this work.

Having explicated the Kuhnian paradigmatic account of science, it can now be shown that Kuhn's reasoning is inductive, a claim made earlier.

Consider what Kuhn has said, (Ibid., p. 77-80):

> *... once it has achieved the status of paradigm, a scientific theory is declared invalid only if an alternative candidate is available to take its place. No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature. That remark does not mean that scientists do not reject scientific theories or that experience and experiment are not essential to the process in which they do so.*
> *... the decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves the comparison of both paradigms with nature and with each other. There is, in addition, a second reason for doubting that scientists reject paradigms because confronted with anomalies or counterinstances, the reasons for doubt sketched above were purely factual; they were, that is, themselves counterinstances to a prevalent epistemological theory. As such, if my present point is correct, they can at best help to create a crisis or, more accurately, to reinforce. By themselves they cannot and will not falsify that philosophical theory, for its defenders will do what we have already seen scientists doing when confronted by anomaly. They will devise numerous articulations and ad hoc modifications of their theory in*

order eliminate any apparent conflict. Many of the relevant modifications and qualifications are, in fact, already in the literature. If, therefore, these epistemological counterinstances are to constitute more than a minor irritant, that will be because they help to permit the emergence of a new and different analysis of science within which they are no longer a source of trouble.... From within a new theory of scientific knowledge, they may instead seem very much like tautologies, statements of situations that could not conceivably have been otherwise.

Though history is unlikely to record their names, some men have undoubtedly been driven to desert science because of their inability to tolerate crisis. Like artists, creative scientists must occasionally be able to live in a world out of joint – elsewhere I have descried that necessity as "the essential tension" implicit in scientific research. But that rejection of science in favour of another occupation is, I think, the only sort of paradigm rejection to which counterinstances by them can lead. Once a first paradigm through which to view nature has been found, there is no such thing as research in the absence of any paradigm. To reject one paradigm without simultaneously substituting another is to reject science itself. That act reflects not on the paradigm but on the man. Inevitably he will be seen by his colleagues as "the carpenter who blames his tools"

The same point can be made at least equally effectively in reverse: For there is no such thing as research without counterinstances. For what is it that differentiates normal science from science in a crisis state? Not, surely, that the former confronts no counterinstances. On the contrary, what we previously called the puzzles that constitute normal science exist only because no paradigm that provides a basis for scientific research ever completely resolves all its problems.... Excepting those that are exclusively instrumental, every problem that normal science sees as a puzzle can be seen, from another viewpoint, as a counterinstance and thus as a source of crisis. Copernicus saw as counterinstances what most of Ptolemy's other successor had seen as puzzles in the match between observation and theory.

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There are two distinct steps, which we must go through to understand a passage like this. First we must identify what point the writer is trying to establish; that is, we must identify his conclusion. Secondly, we must untangle the argument by which he attempts to establish his conclusion.

Kuhn's central argument is to prove that scientific 'progress' is likely to occur through scientific revolutions. Kuhn first gives his conclusion, which he repeats later in the second sentence of the quoted passage: "No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature," because such process "can at best help create a crisis or, more accurately, to reinforce one that is already very much in existence," Kuhn repeats the conclusion in the last two sentences of the second paragraph of the quoted passage: "Once a first paradigm through which to view nature has been found, there is no such thing as research in the absence of any paradigm. To reject one paradigm without simultaneously substituting another is to reject science itself."

Put more shortly, he is saying this: science does not progress cumulatively, that is, following the "methodological stereotype of falsification" as Kuhn puts it but by what he calls "scientific revolution", that is, the throwing away of views that fail to work and replacing them with new ideas (paradigms).Kuhn's thesis in this passage, then, is that what constitutes scientific progress is not a linear process of ever-changing knowledge as advocated by the falsificationists but through a revolution in scientific paradigms: "... there is no such thing as research in the absence of any paradigm."

Having gotten hold of Kuhn's conclusion, we must now untangle the argument by which he tries to establish it. We shall give a 'first reading' of the Kuhnian passage, then we shall reconstruct his argument. A first reading of Kuhn's argument would go thus: science's authority ultimately resides not in a rule-governed method of inquiry whereby scientific results are obtained but in the scientific community that obtains the results. The scientific community is determined by the paradigm they share, that is, specific scientific achievements that provide model for work in the discipline. Phenomena which do not 'fit' the expectations generated by the paradigm become puzzles to be solved by the proper application of the paradigm or the replacement of the 'unfit' paradigm with a more applicable one. By thus doing, science "progresses".

From the foregoing "first reading" of Kuhn's argument, we gather that Kuhn rejects the cumulative approaches to the progress of science in favour of progress by what he calls "scientific revolutions" (Ibid., p.6). Here is a reconstruction of the actual argument.

Schematization of Kuhn's argument:

1) Scientific progress is not cumulative;
2) Scientific progress is determined by scientific communities;
3) Scientific progress is characterized by "paradigm shift";
4) Therefore scientific progress is characterized by "scientific revolutions".

It can now be shown how each premise is related or leads to the conclusion. To do so each premise is taken singly.

Premise 1: The first premise is that the denial that science's authority or progress resides in a rule-governed inquiry is the central thesis of the book under study. This book is a sustained attack on the prevailing image of scientific change as a linear process of ever-increasing knowledge. It is specifically directed against Karl Popper's *The Logic of Scientific Discovery* (1934) in which Popper attempts to make distinction between "science" and "non-science". In this book, Popper argues that scientific statements should be only those that are capable of being falsified; that is, if a hypothesis is proposed, we should, by observation and experiment, seek to discover a counter-instance, which will conclusively falsify the hypothesis. On the other hand, Kuhn's argument is: "no theory ever solves all the puzzles with which it is confronted at a given time; nor are the solutions already achieved often perfect" (Ibid., pp. 38-39). Central to Kuhn's claim in the cited book, is the denial that science is cumulative.

Premise 2: The second premise is closely related to the first premise and is arrived at as an answer to the question: "if science's authority does not reside in a rule-governed inquiry, then, where does it reside?" Kuhn's answer is that science's authority resides in the concerned professional community. Scientists work within a community committed to a shared framework of theory, ideas and presuppositions; that is, "paradigms". Kuhn writes:

> *The study of paradigms ... is what mainly prepares the student for membership in the particular scientific community with which he will later practise. Because he there joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom evoke overt disagreement over fundamentals (Ibid., p.11).*

Premise 3: The third premise is arrived at this way. In normal science, scientists "know what the world is like" but this is not to say that repeatedly normal science does not go astray. When it does, then "begin the extraordinary investigations that lead the profession at last to a new set of commitments, a new basis for the practice of science" (Ibid., p.6). In other words, the scientific community starts looking at nature from a different paradigm. When it is said that a new science has arisen, it is the other way of saying, "the scientific community is looking at nature from a new paradigm".

From the three premises, Kuhn inferred premise 4, progress through revolution. If science is governed by the paradigm shared by a given scientific community, then if there occurs a revolution in the paradigm commitment, then the scientific community will be committed to a new science that has been brought about by that revolution.

These sets of premises are so related that if they are true, they probably imply the conclusion. Kuhn does not say that the above must be the case but that that has been always the case given his detailed study of the history of science. This makes Kuhn's argument strongly inductive; that is, it is improbable that the conclusion is false and the premises are true. Thus, Kuhn's argument is a case of a strong inductive argument because the probability that the conclusion is true, given that its premises are true, is high; that is, the evidential link between the premises and the conclusion is strong.

The above discussion is the idea of scientific progress through revolutions which Kuhn, in the preface to the SSR (1962), says "… would surely add an analytic dimension of first-rate importance for the understanding of scientific advance" (Ibid., p.vii). This kind of claim raises some questions, particularly: When does a proposition claim to be telling us about scientific revolution?

When stated generally, Kuhn's argument would be something like this:

Any proposition is scientific if, and only if, it is based on some paradigm.

The reasons why the criterion of science given above is incorrect shall now be given. In philosophy, one often deals with positions of great generality. Sometimes these positions cover such a wide range of cases that they also cover themselves. Consider this example form Jon Wheatley (1970, p.102):

> *It has been said from time to time that all truth is relative to some historical epoch. In more precise terms, this amounts to this position: For any statement P, the truth of P depends on the date (or epoch) of the utterance of P. This statement is so general, it covers such an enormous range of possible statements, that it applies to itself. That is, it is a statement about the truth of statements; what it says about the truth of statements must therefore apply to its own truth. Thus, if the truth of a statement depends on the epoch of its utterances, then the truth of the statement "The truth of a statement depends on the epoch of its utterance" depends on the epoch of its utterance. From this it follows that it is entirely possible that the statement "The truth of a statement depends on the epoch of its*

utterance" should be false. But this is self-stultifying. The whole point of the thesis was to cover all epochs. To put in another way, the thesis itself, if it is true, constitutes a contrary case to itself. Thus, the thesis is incoherent.

Kuhn's position is exactly this type. Kuhn's analysis of how science progresses if true, must be based on some paradigm; that is, it is so general that it refers to itself and one cannot but ask if the analysis is itself paradigm based. But clearly Kuhn's account of how science progresses is not based on any paradigm. So Kuhn's position is entirely self-stultifying.

Here is the same argument in more compact form:

1) Kuhn claims that any account of how science progresses must be based on some Paradigm.

2) Kuhn's thesis in the SSR (1962) is purportedly a true account of how science progresses.

3) If Kuhn's thesis is true, it must be based on some paradigm.

4) Kuhn's thesis is not based on any paradigm.

5) Thus, Kuhn's thesis is self-stultifying.

CHAPTER THREE: KUHN AND RATIONALITY IN SCIENCE

3.0 Introduction

In analyzing the sciences for philosophical purposes, philosophers of science have re-examined the question, "What is it that makes the sciences rational?" They raise the question of scientific rationality in a new form: "Do the intellectual procedures that scientists actually employ to investigate and explain natural phenomena have definite and objective intellectual merits that make their adoption rationally prudent, wise, and obligatory?" In answering this question, philosophical opinion in recent years has tended to polarize toward two extreme positions: a formalist or positivist extreme and a romantic or irrationalist one (*Encyclopedia Britannica* Vol.16, p.386).

Given their mathematical inspiration and preoccupations, the Viennese empiricists and their successors in Britain and the United States have interpreted the rationality of scientific procedures as depending only on the formal validity, or logicality, of scientific arguments. In their view, questions of rationality can be raised about the scientist's work only at the final outcome of his/her work; that is, when the scientist sets out, as the final outcome of the work, the explicit explanatory arguments in support of his novel theories or interpretation. Only then, they declare, will there be anything about science that is capable of being criticized in logical or philosophical terms.

Consequently, recent empiricist analysis in the philosophy of science distinguishes between discovery and justification. The term "discovery" refers to all stages in a scientific inquiry preceding the formulation of the new explanatory arguments that are the final outcome of the inquiry. The term justification refers to the demonstration that the scientific arguments in support of findings of the scientist have to be formally valid.

At the opposite extreme, there are the so-called romantic or irrationalists, such as Michael Polanyi, the Hungarian scientist and philosopher and Thomas Kuhn, the American physicist and philosopher, who emphasize the parts played by intuition, guess-work, and chance in scientific investigation. In opposition to positivistic science, these philosophers argue that "the modern scientist is a sleepwalker whose creative insight guides him to intellectual destinations that he could never clearly see or state beforehand " (Ibid., p.381). These philosophers have been accused by the positivists of advancing irrationalistic science, a claim that the romanticists deny vehemently. Kuhn, for instance, objects strongly to the charge of irrationality. "If science is not rational", he asks, "what is?"

According to Greg Ransom (1996,p.1), Kuhn has transformed contemporary discussions of scientific rationality by shifting philosophical attention from the behaviour of the individual scientist to the structure of the community as a whole. Kuhn points out that the rationality of the individual scientist is dependent on a social context, which is itself the product of an evolutionary process of group competition and selection. He reminds his critics that he always has maintained that there are "good reasons" and "hard-headed arguments" for choosing paradigms. In the *Postscript* to the *Structure of Scientific Revolutions*, he spells out more fully the values, which are shared by all scientists:

> *Probably the most deeply held values concern predictions: they should be accurate; quantitative predictions are preferable to qualitative ones; whatever*

the margin of permissible error, it should be consistently satisfied in a given field; and so on. There are also, however, values to be used in judging whole theories: they must, first and foremost, permit puzzle-formulation and solution; where possible they should be simple, self-consistent, and plausible, that is, with other theories currently deployed (Kuhn, 1962, p.185).

Kuhn insists, however, that these shared values provide no automatic rules for paradigm choice, since there is inevitable variation in individual judgement in applying them. Moreover, not all persons will assign the same relative weights among these values. After stating that debates over fundamental theories do not resemble logical or mathematical proofs, Kuhn concludes:

Nothing about that relatively familiar thesis implies either that there are no good reasons for being persuaded or that those reasons are not ultimately decisive for the group. Nor does it even imply that the reasons for choice is different from those usually listed by philosophers of science; accuracy, simplicity, fruitfulness, and the like. What it should suggest, however, is that such reasons function as values and that they can thus be differently applied, individually and collectively, by men who concur in honoring them. If two men disagree, for example, about the relative fruitfulness of their theories, or if they agree about that but disagree about the relative importance of fruitfulness and, say, scope in reaching a choice, neither can be conceived of a mistake. Nor is either being unscientific. There is no neutral algorithm for theory choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision (Ibid., 1962, pp. 199-200).

Kuhn offers a pragmatic justification for this variability of individual judgement. On the one hand, if everyone abandoned an old paradigm when it first ran into difficulties, all effort would be diverted from systematic development to the pursuit of anomalies and the search for alternatives – almost all of, which would be fruitless. On the other hand, if no one took alternative paradigms seriously, radically new viewpoints would never be developed far enough to gain acceptance. Variations in judgement allow a distribution of risks, which no uniform rules could achieve. Yet, the fact that there are agreed values encourages communication and the eventual emergence of a scientific consensus. Finally, these values provide standards in terms of which one can see genuine progress as one looks at a succession of theories in the history of science. He puts his conclusion thus: "That is not a relativistic position, and it displays the sense in which I am a convinced believer in scientific progress" (Ibid., pp. 205-206). Kuhn thus denies the allegations of irrationality and subjectivism.

Some of Kuhn's critics are still far from satisfied in this regard (Shapere, 1971; Scheffler, 1967; Kordig, 1971). Thus Dudure Shapere, in a review of Kuhn's recent writings, repeats his earlier epithets:

> *It is a viewpoint as relativistic, as antirationalistic, as ever.... He seems to want to say that there is paradigm – independent considerations, which constitute rational bases for introducing and accepting new paradigms; but his considering them to be 'values', so that he seems not to have gotten beyond his former view after all. He seems to want to say that there is progress in science; but all grounds of assessment again apparently turn out to be "values", and we are left with the same old relativism.... The point I have tried to make is not merely that Kuhn's is a view which denies the objectivity and rationality of the scientific enterprise; I have tried to show that the arguments by which Kuhn arrives at this conclusion are unclear and unsatisfactory (Shapere, 1971, pp. 8-9).*

Shapere does not define "rationality", but he evidently identifies it with rule-governed choice. Kuhn is called "anti-rationalistic", it seems, because he still holds that the choice of paradigms is not unequivocally specified by the values accepted throughout the scientific community. "Such name-calling, however, sheds little light on the question of how choices in science are or should be made" (Gutting, 1984, p.231). A close examination of what scientific rationality requires is needed so as to clear any conceptual lack of clarity.

This chapter examines the logical conditions of scientific rationality. It considers three requirements for rationality: "rational results must be [a] universal, [b] necessary and [c] determined by rules" (Hosfstadter, 1983, pp.14-28). After explicating these requirements for rationality, the chapter then proceeds to show that Kuhn's account of science is irrationalistic, according to this criterion.

This chapter is divided into two sections: the first section concerns itself with clarification of the term "scientific rationality" which has no technical definition in any discipline. Hence, there is need to explicate it by showing its logical structure, so as to classify or to distinguish between "scientifically rational" claims from those which cannot qualify for the same title. It is then possible to show how the scientific method captures the conception of rationality. The second section concerns itself with relating "scientific rationality", as clarified in the first section to Kuhn's account of science so as to point out the failure of his account to meet the logical conditions of scientific rationality. It should be observed here that Kuhn defends his position thus:

> *If two men disagree, for example, about the relative fruitfulness of their theories, or if they agree about that but disagree about the relative importance of fruitfulness and, say, scope in reaching a choice, neither can be convicted of a mistake. Nor is either being unscientific. There is no neutral algorithm for theory-choice, no systematic decision procedure which, properly applied, must lead each individual in the group to the same decision (Kuhn, 1962, pp. 199-200).*

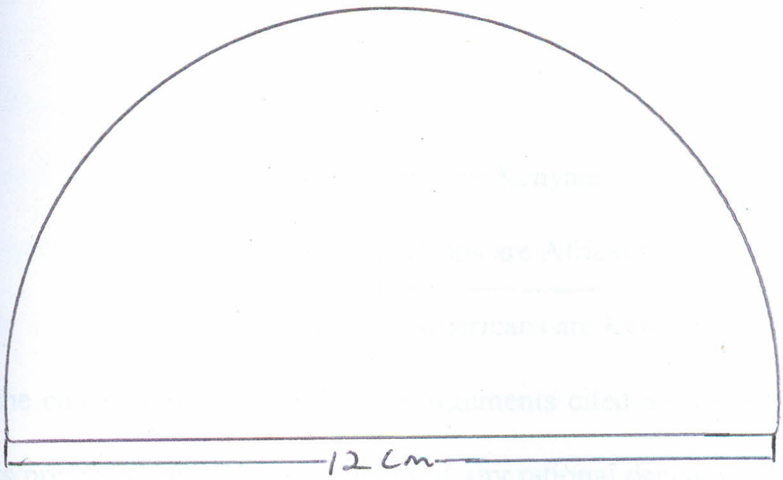
It is the concern of the second section to show that Kuhn's account of science cannot be considered scientifically rational since it does not meet the conditions of rationality as stipulated in the classical model of rationality, as hereunder discussed.

3.1 Scientific Rationality

According to John Elster, the term "rational" is regularly applied to a large variety of items, which include "beliefs, preferences, choices or decisions, actions, behavioural patterns, persons, even collectivities" (Elster, 1983, p.1). However, Harold Brown describes "a model of rationality that has been pervasive in Western thought, even though it has not been explicitly formulated" (Brown, 1990, p.3). The concern here is with what makes a specific belief, decision, and act, among others, rational, and, situations that call for rational behaviour, and a rational decision. In many cases, the terms such as 'belief' and 'decision', among others, are used to ask the same question in different ways. For example, it may be asked whether a decision to cooperate is rational, or whether it is rational to act on a decision to cooperate.

According to Douglas Hofstadter, in one of his articles in the Scientific American (Vol.No.6, 1983, pp. 14-28), "rational results must be universal, necessary and determined by rules". Hofstadter maintains that all rational thinkers must arrive at the same solution to a given problem; they all begin with the same information and, in such cases, correct reasoning can only lead to one conclusion. In general, if two individuals arrive at different results in a particular situation it must be either because they do not both have the same information, or because, at least, one of them is not preceding in a wholly rational manner. As Hofstadter suggests, mathematics and logic provide a paradigm of rationality. Given a specific problem in long division, there is no room for judgement or opinion as to the correct solution; there is simply a correct answer, and anyone, anywhere, who follows the appropriate procedures correctly will arrive at this answer. The key idea is that there exists both a definite solution and a definite procedure for arriving at that solution and all that follow that procedure must arrive at the same result, as the following example from mathematics will show.

Question: Find the perimeter of the semi-circular shape below of diameter 12cm.



Perimeter = Circumference of semi-circle + diameter

= ½ π D + D

= (½x3.14x12+12) cm

= (18.84 + 12) cm

= 30.84 cm

Similarly, in the case of logic, an argument is either valid or invalid, and there are unequivocal procedures for assessing validity as shown in the following valid argument.

All girls are boys

Mary is a girl

Therefore Mary is a boy

Implicit in the two models of rationality given above is the view that "rationality is widely thought to be modelled after the example of logical deduction" (Ransom, 1996, p.3). In such an argument, the conclusion is to be found within its premises, as the following examples would show again.

All men are mortal

Socrates is a man

Therefore Socrates is mortal

All Africans are Kenyans

All Americans are Africans

Therefore all Americans are Kenyans

Just as the conclusion of the deductive arguments cited above is to be found contained within its premises, so is the conclusion of any rational decision. The conclusion can be known universally, the first criterion of rationality.

The demand for universality is "so deeply embedded in our current understanding of rationality that to question the universality of a discipline's foundations is equivalent to questioning the rationality of that discipline" (John Henry, 1997, p.8). But the notion of 'universality' is ambiguous: some claims or principles are universal in the sense of being applicable in every possible domain, while some are only universal with respect to a limited domain. Formal logic provides the clearest example of the former, since validity of an argument is independent of any particular subject matter, and a valid argument therefore is valid everywhere. Proponents of this model of rationality have typically taken the full universality that we find in logic as an ideal. On this model, a belief or decision is rational if it conforms to a set of criteria, and if the same criteria are applicable in every context, then rational individuals need not debate over which criteria should be applied.

So much for universality. Attention is now turned to necessity as a second criterion of rationality. It is not enough that all rational thinkers arrive at the same conclusion since this might occur as a result of a massive coincidence, rather than through reasoning. "A rationally acceptable conclusion must follow with necessity from the information given" (Kant, 1963, pp. 43-44). Mathematics and logic provide the central model as has been demonstrated above. The answer to a problem in arithmetic follows with necessity from the information supplied, while the conclusion of a valid deductive argument follows with necessity from the information supplied, that is, a valid deductive argument follows necessarily from the premises, and any deductive argument is necessarily valid or necessarily invalid. Consider this example in which the conclusion is not related to the premises.

All dogs are mammals

All dogs are animals

Therefore all mammals are animals

According to Harold Brown (1990, p.14), the requirement of necessity is more fundamental than the requirement of universality since the existence of a necessary tie between the available information and a rationally acceptable result allows us to understand why all rational individuals who start at the same point must arrive at the same conclusion. Brown emphasizes:"Not only must there be a necessary tie between premises and conclusion for that conclusion to be rationally acceptable, it is also required that we accept the conclusion because we recognize the existence of that necessary connection" (Ibid., p.15).

One important example of the connection between necessity and rationality in the history of philosophy is provided by the sharp distinction that is often drawn between accepting a result on a rational basis and accepting it on the basis of experience. The grounds for this distinction typically lie in the claim that conclusions accepted on the basis of experience do not have the necessity that characterizes reasoned results. For example, John Locke believed that the perceived properties of a natural substance are necessarily determined by its essence; that is, by its atomic constitution, and he held that if we knew the essence of a substance we would be able to deduce its properties. But Locke doubted our ability to discover these essences, and argued that we lack a rational grasp of which qualities go together in a particular substance, and are reduced to relying on experience (Locke, 1984, p.645).

Similarly, David Hume holds that if we had a rational knowledge of caused connections we would grasp a necessary connection between cause and effect and we would be able to achieve this by examining the ideas of the items involved. But we cannot grasp this necessity, and we learn about caused connections only through experience (Hume, 1978, pp.86-87). In other words, the characteristic feature of rational knowledge is that it provides us with a grasp of necessary connections between the items that concern us, and experience fails to measure up to this demand. The distinction between accepting a result on a rational basis and accepting it on the basis of experience is a distinction between empiricists and rationalists.

So much then for necessity as a criterion of rationality. Brown best offers the third criterion of rationality when he says:

> *The rationality of any conclusion is determined by whether it conforms to the appropriate rules. When we proceed from a starting point to a conclusion in accordance with a set of rules, we free ourselves from the arbitrariness that is characteristic of non-rational decisions (1990, p.17).*

Consider the case given earlier of finding the perimeter of the semicircle. There are definite rules to be followed in arriving at the answer. Anybody anywhere must follow the formula that the perimeter of a semi-circle is equal to half the diameter plus the same diameter (in symbolic form: $P = \frac{1}{2} \pi D + D$). Equally, if one was required to calculate this sum: $(20x3) \div 4 + 3 - 1$, one must follow definite rules to arrive at the answer. The mathematical rule 'BODMAS' will apply here. If we have universal applicable rules, then all who begin from the same information must indeed arrive at the same conclusion, and "it is these rules that provide the necessary connection between our starting point and our conclusion" (John Ziman, 1995, p.81).

Having explicated the logical structure of the notion of "rationality", it should now be shown how the scientific method captures it. But before that is done, an explication of the steps that the scientific method follows should first be presented. The scientific method begins when there is some problem to be solved or a difficulty to be overcome in life. For example, a stranger comes to a Y-junction. He stops and employs some thinking so as to resolve the dilemma. The history of science is replete with examples of scientists who were faced with great difficulties that served as the starting point of great discoveries. For instance, in 1847, Ignaz Semmelweis, (as cited by David Lamb, 1984) wondered why there was widespread persistent fever in European hospitals. He sought to discover how the fever was transmitted from the post-mortem room to the patients in hospital.

The second step that the scientific method follows is observation. This step involves analyzing the situation very carefully and collecting all the facts bearing on the problem to be solved. In the analysis and collection of the facts, the scientific method requires that we have to be fair, impartial, and unprejudiced in our observation of the facts. This step requires that personal liking and biases should not be entertained; it is in the nature of science to be objective. Consider the case of Semmelweis above, as accounted by David Lamb (1984, 91). After observing that fever was prevalent in European hospitals, he further observed that the fever was carried to the patients on the hands of medical teachers and students coming directly from the post-mortem room. He instituted a strict routine of hand washing in a solution of chlorinated lime before the examination of patients. There followed success. The mortality rate immediately fell in the General Hospital of Vienna from 12% to 3% and later to 1%. After years of ridicule he died in an asylum in 1865. It was only when his work was independently taken up by "prestigious figures" like John Tarnier and Pierre Pasteur in France, and John Lister in England, that a hostile world was forced to admit that Semmelweis was correct. Semmelweis' observations were impartial and unbiased, that is why they were recognized by the other "prestigious figures".

The third step involves proposing a tentative solution to the problem. This is called the hypothesis or provisional theory (Patrick, 1978, p.58). Consider, for instance, this account by Lamb (1984, p. 51-52). There was a "priority dispute" over reflecting and refracting telescopes in the mid-nineteenth century, particularly concerning the observations of Neptune. The then astronomers proposed that the best equipment was the one that could make the first observation of the planet. Given the respective successes of these instruments, one could have predicted several independent multiple discoveries. For example, in an attempt to outdo his rival Lord Rosse, William Lassell, a British astronomer, constructed a telescope, which measured 24 inches in diameter. Lassell intended to demonstrate how a mere mirror could surpass the then popular German made lenses, especially in terms of the sharpness of the image. His experiment succeeded beyond his wildest expectations.

On October 10, 1846, he observed Neptune, which had been discovered only three weeks earlier. However, his more powerful telescope enabled him to observe a tiny dot, which circled around Neptune every five days. This, he concluded, was Neptune's moon, thus providing conclusive proof of the superiority of his instrument. His technical lead was short-lived. Several weeks later, G.P. Bond, using a German made instrument equipped with a mere 12- inch lens observed later Neptune's moon at Harvard University. Following the discovery of Neptune's moon, Lassell of the Cambridge University turned his attention towards Saturn, where six moons had already been discovered, all adhering to that remarkable distance ratio known as Bodes' Law and all displaying the same gap as the one between Mars and Jupiter.

Lassell maintained that there had to be another moon in that gap and directed his telescope towards it. Success came one evening in 1848 when he observed a very small moon. But that very same night Bond at Harvard also observed it. Within a short time an even more remarkable multiple discovery occurred. Believing that he had by now advanced beyond his rival, one night, Lassell observed a diaphanous black veil within Saturn's rings, forming what appeared to be dark inner ring. He spent the whole night observing this phenomenon checking and rechecking his equipment. In the morning he wrote out his report on the night's work and then picked up a copy of The Times to read as he breakfasted, only to read how his American rival had demonstrated the existence of an additional dark ring within Saturn's ring. In brief, the "priority dispute" over the said telescopes in the mid-nineteenth century was now settled: both kinds of telescopes worked. The scientific method can be summarized thus: the scientific method requires that an inquirer starts with a problem, and then makes observations. Next, the scientist develops some hypothesis and finally tests his hypothesis to come up with a theory.

It is now the concern of the following part to explicate how the scientific method captures the notion of rationality that was discussed in section 3.1. The criterion of rationality that we discussed demands that rational results be universal, necessary and determined by rules. The scientific method is also universal, necessary and determined by rules. In the scientific method, anyone who has learned the rules of understanding and testing theories can repeat the experiment and judge for himself. To check on impartiality, various social institutions like laboratories, scientific periodicals, scientific congresses and scientific papers have been designed – all of which lend the scientific method its universal objectivity.

Although science has long been considered a paradigm example of a rational endeavour (Ziman, 1995, p.86), a number of philosophers have recently raised doubts, or have been interpreted as raising doubts, about the rationality of scientific procedures. Paul Feyerabend, for instance, writes:

> *If science has found a method that turns ideologically contaminated ideas into true and useful theories, then it is indeed not mere ideologies... but the fairy tale is false, as we have seen. There is no special method that guarantees success or makes it probable.... Basically there is hardly any difference between the process that leads to the announcement preceding passage of a new law in society... (1975, p.302).*

Feyerabend's attack on the thesis that science is guided by a universal method is based on only one example from the history of science. The example he gives is the shift from geocentricism to heliocentricism. He, then, draws the conclusion that if there existed a universal method then there could not arise scientific revolutions such as the cited change from geocentricism to heliocentricism. But one can not fail to point the weakness of Feyerabend's attack on the scientific method. If a principle, say in physics, is truly universal, then we understand what we are doing when we invoke it as a reason for a decision, as the shift from geocentricism to heliocentricism. The scientific method is universal in its application. For instance, the formula for the law of gravitation symbolized as: $S = \frac{1}{2} gt^2$ is applicable everywhere and every time, that is, it is universally applicable.

The foregoing criticism on the scientific method arises because of the failure of the critics to distinguish between discovery and justification in science. Hans Reichenbach writes in his *The Rise of Scientific Philosophy* (1978, p.230),

> *The scientist who discovers a theory is usually guided to his discovery by guesses; he cannot name a method by means of which he found the theory and can only say it appeared plausible to him, that he had the right hunch, or that he saw intuitively which assumption would fit the facts.*

On these terms, a scientist proceeds by random guesses, but later invokes a process of confirmation by reference to the facts. For Reichenbach, mysticism and irrationalism are attributed to the context of discovery and the possibility of a logic of discovery is ruled out in advance.

In opposition to the above view, that scientific discovery cannot be subjected to logical analysis, Charles S. Peirce rejected "chance" accounts of discovery, arguing that even the initial stages of discovery have a rational basis. According to Peirce even the most tentative hypothesis or conjecture is selected by a rational procedure. Some are, by virtue of the climate of opinion, or level of scientific development, more or less acceptable than others. To account for this process of selection, Peirce outlined his theory of influence known as "abduction", or "retroduction", according to which a scientist does not rely on chance, luck, or genius, putting forward whatever theory comes to mind, but acts selectively, putting forward the most plausible theory among others. As he says,

> *The first stating of a hypothesis and the entertaining of it, whether as a simple integration or with any degree of confidence, is an influential step which I propose to call abduction. This will include a preference for any one hypothesis over others which would equally explain the facts, so long as this preference is not based upon any previous knowledge bearing upon the truth of the hypothesis, nor on any testing of any of the hypothesis, after having admitted them on probation. I call all such inference by the peculiar name, abduction because it legitimately depends upon altogether different principles from those of other kinds of inference (1967, pp. 236-237).*

Rejecting the occult explanation of discovery, Peirce argues that discovery does have a rational basis. The existence of a criterion of selection amongst plausible hypotheses guarantees the role of reason in the process of discovery. On these terms we might say that Johannes Kepler reasoned abductively when he conjectured the orbit of Mars. Initially he presupposed the circular motion of the planets, but found that predictions deduced from this hypothesis conflicted with the data of Tycho Brahe, so he assumed the correctness of this data and reasoned accordingly.

There are other philosophers of science who do not find any distinction between discovery and justification in science. A case in point is Peter Medawar who writes in his book, *Introduction and Intuition in Scientific Thought* to the effect that "discovery and justification make one act of reasoning not two…" (1972, p.8). There is need for a close examination on the history of science in order to verify Medawar's view. Having explicated the logical structure of scientific rationality the following section relates Kuhnian paradigmatic account of science to scientific rationality.

3.2 Scientific rationality and the Kuhnian account of science

This section relates scientific rationality with the Kuhnian paradigmatic account of science with the express aim to determine whether or not Kuhn's account of science is scientifically rational. This section first states Kuhn's claim and then proceeds to examine whether Kuhn's account of how science progresses is scientifically rational. Consider this quotation (Kuhn, 1962, pp.10-12):

> *In this essay, 'normal science' means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice. Achievements that share these two characteristics I shall henceforth refer to as "paradigms", a term that relates closely to "normal science". By choosing it, I mean to suggest that some accepted examples of actual scientific practice – examples, which include law, theory, application, and instrumentation together – provide models from which spring particular coherent traditions of scientific research.*
>
> *The study of paradigms... is what mainly prepares the student for membership in the particular scientific community with which he will later practice. Because he there joins men who learned the bases of their field from the same concrete models, his subsequent practice will seldom evoke overt disagreement over fundamentals. Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, that is, for the genesis and confirmation of a particular research tradition....*
>
> *Why is the concrete scientific achievement, as a locus of professional commitment, prior to the various concepts, laws, theories, and points of view that may be abstracted from it? In what sense is the shared paradigm a fundamental unit for the student of scientific development, a unit that cannot be fully reduced to logically atomic components, which might function in its stead? Acquisition of a paradigm and of the more esoteric type of research it permits is a sign of maturity in the development of any given scientific field....*
>
> *These transformations of the paradigms of physical optics are scientific revolutions, and the successive transition from one paradigm to another via revolution is the usual developmental pattern of mature science.*

Elsewhere, Kuhn writes:

> *When in the development of a natural science, an individual or group first produces a synthesis able to attract most of the next generation's practitioners, the older schools gradually disappear. In part their disappearance is caused by their member's conversion to the new paradigm. But there are always some men who cling to one or another of the older views, and they are simply read out of the*

> *profession, which thereafter ignores their work. The new paradigm emphasizes a new and more rigid definition of the field. Those unwilling or unable to accommodate their work to it must proceed in isolation or attach themselves to some other group. Historically, they have often simply stayed in the depths of philosophy from which so many of the special sciences have been spawned....*
>
> *As these indications hint, it is sometimes just its reception of a paradigm that transforms a group previously interested merely in the study of nature into a profession or, at least, a discipline. In the sciences (though not in fields like medicine, technology, and law, of which the principal raison d'être is an external need) the formation of specialized journals, the foundation of specialists' societies, and the claim for a special place in the curriculum have usually been associated with a group's first reception of a single paradigm. At least this was the case between the time, a century and a half ago, when the institutional pattern of scientific specialization first developed and the very recent time when the paraphenalia of specialization acquired a prestige of their own (Ibid., pp. 18-19).*

When the two passages are read alongside each other, it becomes clear that Kuhn is offering paradigm shifts as the "route to normal science". Kuhn's argument in the above passages is that normal science is determined by a particular paradigm to which a scientific community subscribes. Any paradigm shift means that 'normal science' is no longer 'normal'. When the scientific community subscribes to a new paradigm, a scientific revolution has occurred. Kuhn then adds that paradigm choice is not determined by "logical or methodological distinction" (Ibid., p.9).

Kuhn's central argument in the above passages is that normal science is determined by a particular paradigm, to which a scientific community subscribes. Any paradigm shift means the change of 'normal science' to "new science" . The adoption of a new paradigm is a conversion, which is not rule governed. Put more precisely, Kuhn's claim is that scientific development is determined by paradigm shift.

In the last sentence of the second paragraph of the first passage, Kuhn states his conclusion in the following manner: "That commitment and the apparent consensus it produces are prerequisites for normal science; that is, for the genesis and continuation of a particular research tradition". In other words, the acquisition of a particular paradigm by a group of scientists determines their worldview, that is, the paradigm determines the way scientists look at the world. If the paradigm changes, then "normal science" changes to "new science". The shift from "normal science" to "new science" is what Kuhn calls "scientific revolution". To put it in different words, Kuhn is saying that scientific development is determined by paradigmatic shifts within the scientific community. He advanced his argument on three premises, namely;

1. Normal science is determined by a scientific community;
2. The scientific community is determined by a particular paradigm;
3. Therefore, if there is a paradigm shift, then it is likely that "normal science" will change to "new science".

It should now be shown that Kuhn's argument is a strong inductive argument by showing how the three premises are related to each other. In the first premise, Kuhn's argument is that normal science is determined by a scientific community. According to John Ziman in his book, *An Introduction To Science studies* (1984, p.81),

> *This community is not a mere collection of individuals. Although it does not have an overall organization plan, it is structured around a number of* formal institutions such as learned societies, and informal institutions, such as invisible colleges. It is spanned by an elaborate communication *system, which follows standard practices in the management of publications and archives; regulates the roles of authors, editors… and has strict conventions on the style and format of papers.*

Kuhn further states that what is more about this scientific community is that they share a given paradigm. According to Kuhn, the paradigm determines their profession. He puts it in the following question: "What is the concrete scientific achievement, as a locus of professional commitment, prior to the various concepts, locus, theories and points of view that may be abstracted from it?" His answer is that the locus of professional commitment is a paradigm. From the two premises, Kuhn inferred the conclusion: if normal science is determined by a scientific community which is itself determined by a paradigm, then when the paradigm changes, normal science changes to new science. He further writes: "it is sometimes just its reception of a paradigm that transforms a group previously interested merely in the study of nature into a profession or, at least, a discipline."

Kuhn's position on paradigmatic science can be summarized thus: because a paradigm is "at the start largely a promise of success discoverable in selected and still incomplete examples" (Ibid., pp. 23-24), it is "an object for further articulation and specification under new or more stringent conditions" (p.23), hence from paradigms "spring particular coherent traditions of scientific research" (p.10) which Kuhn calls "normal science". Confronted with anomaly or with crisis, scientists take a different attitude toward existing paradigms and the nature of their research changes accordingly. Scientific revolutions are inaugurated by a growing sense that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way. The upshoot of such crisis is often the acceptance of a new paradigm. Paradigm choice is neither logical nor methodological; that is, it does not follow any rules.

Kuhn's argument can be reconstructed in the following manner. Normal science is dominated by paradigms. Paradigms are not chosen by any logical or methodological rules. To put it in one sentence: paradigmatic science is not governed by logical and methodological rules. As we stated in the foregoing section, rational results must be universal, necessary and determined by rules. Any account of science, which does not follow this demand, is necessarily irrational, this is so because science is objective in its nature because it follows the scientific method. This method enables experiments to be repeated anywhere and anytime by any person interested to investigate any scientific claim. As shown above, Kuhn's account of science is based on paradigms. Paradigm – choice is not rule or methods logical based. Therefore, Kuhn's account of science is irrational.

In his article, "Reflections on my critics" (1970, p.23), Kuhn denies that his account of science is irrational. But close examination of his paradigmatic account of science revealed that Kuhn defends a thesis that cannot be found in his book. That Kuhn's paradigmatic account of science is not scientifically rational can be schematized in the following manner:

a) Rational results must be universal, necessary and determined by rules;

b) Kuhn's account of science is not rational since paradigm shifts are not determined by rules;

c) Hence, Kuhn defends what cannot be found in his book, *The Structure of Scientific Revolutions* (1962). What does emerge clearly from Kuhn's defense of his early views is that it is a conception to which he does not now and, perhaps, never did, subscribe;

d) Consequently, if Kuhnian paradigmatic account of science is not determined by rules, Kuhn or any other person does not have the justification for describing Kuhn's account of science scientifically rational; for it does not meet the requirements of scientific rationality described in section 3.1.

Despite the failure of Kuhn's account of scientific rationality to capture the classical model of rationality, Kuhn is trying to express a third alternative- an account of scientific authority in terms of the trained scientists (SSR p. 44). Such judgment is informed by logical arguments based on methodological rules including some shared by all scientists at all times (Ibid., p. 42). But it is not determined by logical arguments, personal idiosyncrasies or prejudices. The judgement is ultimately by the carefully nurtured ability of members of the scientific community to assess rationally the overall significance of a wide variety of separately inclusive lines of argument.

This emphasis on the scientific community's judgement as the ultimate locus of science's rational authority is the most fundamental feature of Kuhn's account of science. Hence, to the extent to which Kuhn's analysis captures the wider conception of rationality as presupposed by the community of informed scientist to the same extent is Kuhn's strong inductive argument rational.

CHAPTER FOUR: KUHN AND THE ACTUAL PRACTICE OF SCIENCE

4.0 INTRODUCTION

This chapter concerns itself with the examination of the nature of the scientific enterprise by explicating how the scientist "operates" in his business. It should be noted that scientists themselves have been interested not merely in cataloguing and describing the world of nature as they find it but in making the working of nature intelligible with the help of compact and organized theories. Correspondingly, philosophers of science are obliged to consider not merely nature in isolation; that is, as a mere assemblage of empirical facts waiting to be discovered. They are also interested in the manner in which the human perceives and interprets those facts when bringing them within the grasp of an intelligible theory and the respects in which the validity of the resulting theoretical ideas (or concepts) are affected by that processing of the empirical data.

The problems posed by this interaction of humans and nature has been complex and confused. There is need, therefore, to clarify the way the scientist "operates" in his/her enterprise in order to determine how he/she interprets the empirical data that present themselves to him/her. That is done in the section that immediately follows. Before that is done, distinction is made between two aspects of science implicitly found in the scientific method.

From the scientific method one can identify two aspects of science, namely, formal science and empirical science. The former embraces the sciences of mathematics and formal logic. The latter, which is also known as natural science, embraces all the sciences called "physical" and "social", for example, chemistry, physics, economics and sociology, among others. Formal science asserts nothing about natural phenomena; it is independent of experience and none of its proofs rests on how facts actually stand. Empirical science, where the term "empirical" means "relating to experience", deals with some aspect of what can be experimentally known. The empirical sciences use observations, which are accumulated by the method of induction.

In his analysis of matter, Aristotle gave two accounts of induction, which had great influence on subsequent thought. In *Prior Analytics, ii, 23*, Aristotle talks of induction as a kind of syllogism in which we reach universal conclusion from an exhaustive survey of the cases it covers. In *Posterior Analytics 1 & 18*, he talks of induction as the establishment of a universal truth by consideration of an instance or instances, which reveal to thought the necessity of the connection. The two accounts of induction have been called summative and intuitive induction, respectively; "none of which is identified with empirical science by which universal propositions are established in empirical sciences" (*Encyclopedia Britannica*, 1973,p.18). The same mentioned work argues that Francis Bacon, David Hume and John Mill share the above view. Mill, for instance, spoke of employing a number of different methods, both modelled on Bacon's induction. In the said encyclopedia, Bacon talks of:

> *a method of agreement, in which the cause of a phenomenon is revealed by consideration that it is the only circumstances in which positive instances agree; and a method of difference, in which the cause is revealed by the consideration that it is the only circumstance in which a positive and a negative instance differ. The former was used in observational sciences while the latter was used in the experimental sciences" (Ibid., p.19).*

Mill's methods do not cover the whole range of scientific activity and the reduction of the business of discovery to rules is itself misleading. It is very difficult to find a formula, which will adequately characterize all scientific activity. Some scientists are concerned with the making of inductive generalizations from experience. Others dwell on deductive systems where generalizations may be derived from hypotheses that cannot themselves be tested directly because they deal with unobservable entities like electromagnetic waves. What can be said is that the pursuit of science is the search for knowledge and understanding through the careful examination of how it is actually practiced.

From the foregoing discussion, it is noted that science can be classified into 1). Formal science and 2) Empirical science. This chapter will concern itself with the examination of the salient and common grounds of the common understanding of what science is: "a collection of empirical and formal statements about nature, the theories and data that, at a given moment in time, comprise accepted scientific knowledge" (Kragh Helge, 1994, p.22).

This chapter is divided into two sections. In the subsection that follows immediately an examination of the scientific practice is shown. The next section concerns itself with relating "practice of the scientific enterprise", as clarified in the first section to Kuhn's account of science with the express aim of determining whether or not he has given a correct account of the actual practice of science. It should be noted that Kuhn argues that he has produced a correct account of science as it is practiced currently. During the period he was a Junior Fellow of the Society of Fellows of Harvard University, 1958-1959, he "was surprised at … exposure to out-of-date scientific theory and practice [which] radically undermined some of [his] basic conceptions about the nature of science and the reasons for its special success" (Kuhn, 1962, p. V). He adds, "during my last year as a Junior Fellow, an invitation to lecture for the Lowell Institute in

Boston provided a first chance to try out my still developing notion of science" (Ibid., p.viii).

4.1 THE ACTUAL PRACTICE OF SCIENCE

According to D.W.Y. Kwok, in his book, *Scientism in Chinese Thought* (1965),

> *the scientist operates on four fundamental principles. First, the need for observation: the empirical principle. Second, to achieve exactitude in measurement... he must employ quantitative means: the quantitative principle. Third he deals with causal relations and often uses abstractions to represent them. For this end, he must locate meaningful recurrences of behaviour and then formulate general laws or equations, which describe and explain such behaviour: the mechanical principle of science. Fourth is a general assumption of all scientists which may be called an attitude of mind, a principle inherent in the concept of research: the principle of progress through science... cooperation for non-personal ends, a cooperation in which all scientists of the past, present, and the future have a part (pp.21-22).*

Each of the four mentioned principles: [a] The empirical principle, [b] The quantitative principle, [c] The mechanical principle and [d] The cooperative principle, has its own characteristic procedures. They are therefore, dealt with in that order.

[a] The Empirical Principle

Empiricism is the belief that knowledge ultimately rests on firsthand, direct, and original experience. In the realm of natural science, it means that attending to, exploring, investigating, and scrutinizing natural phenomena attains human knowledge about a natural phenomenon. Consequently, the task of a scientist is to explain actual events, processes or phenomena in nature. According to George Patrick (1978, p.20), "the scientist in his study of any group of phenomena first collects facts; analyzes and classifies them". On the one hand, the facts in question may be discovered by using observational methods; that is, by recording them as and when they occur naturally, without employing any special contrivance affecting their occurrences. This situation is, of course, the normal case in astronomy, in which the objects of study cannot be influenced or controlled. For instance, on October 10, 1846, William Lassell observed Neptune. His powerful telescope also enabled him to observe a tiny dot, which circled around Neptune every five days. This, he concluded, was Neptune's moon. His telescope did not influence the activities around Neptune.

The facts in question may, on the other hand, be discovered by using experimental methods, that is, by devising special equipment or apparatus with the help of those processes or phenomena are caused to occur on demand and under specially controlled conditions as is the case in physics and chemistry. For instance, a scientist can experimentally show that hydrogen combines with oxygen at two atoms of hydrogen tone atom of oxygen. This is symbolized as: $4H + 0_2 \longrightarrow 2H_20$. Whichever way the scientist uses to obtain empirical facts, a philosophical difficulty at once arises about the results of the scientists' empirical studies: for a philosopher of science must ask how such raw empirical facts can be sifted, stated and described in a way that throws light on the scientist's own theoretical problems. Do all empirical facts serve as raw materials for science? Is a scientist concerned with every particular empirical event or only with general phenomena or regularities recognizable in those events?

Going by this principle, the scientist is required to analyze the situation at hand very carefully and collect all the facts bearing on it. He/she must be fair and impartial and unprejudiced in the observation of the facts. Prejudice leads the scientist astray in the reflective thinking of his/her daily life. The history of knowledge is replete with examples in which custodians of knowledge committed 'sins' to knowledge because of prejudice. Here, the well-known case between the Church and Copernicus on the shift from geocentricism to heliocentricism can be cited. This freedom from prejudice is an ideal, which is very difficult to realize. In the physical sciences the idea of objectivity has seen realized in a remarkable fashion by a great army of patient, persistent, and unprejudiced workers. The rich contributions, which they have made to knowledge, attest to the fruitfulness of the scientific method.

[b] The Quantitative Principle

If we ask, "Why are some areas of knowledge more precise and definite than others?" we soon discover that *measurement* is science's principal means of reducing vagueness in favour of clarity and precision. Measurement is a procedure through which the scientist arrives at quantitative estimates of the variables and magnitudes considered in their theories. The *Encyclopedia Britannica* puts it thus:

> *By now, there is a well-developed body of knowledge upon which scholars are agreed about many of the techniques and precautions to be employed in practice in the measurement of empirical quantities, in the calculation of probable errors or significant deviations, and so on (p.384).*

Historically, the first scientific measurements were of "long-short" distances and of "heavy-light" weights. Once distance was precisely measured, then the three measurements of length, breadth, and thickness made it possible to also measure volume – thus to change the vague polarity of "large – small" into precisely measured amounts. Measurement is the criterion, which most sharply differentiates the physical sciences from the social and moral sciences. In areas such as law, theology, psychology, sociology and economics, where precise measurement is lacking, much attention is given to the definition of terms so that all can agree as to their meaning. Here is an example from theology.

Among the ancient Israelites, there was a section governing the sacrifice of the "red heifer" as shown in the *Bible* (Numbers 19: 2-9). But how is a "red heifer" to be defined? Five rabbinical schools of thought arose, and as a result a "red heifer" was defined in the following five ways as shown by Herbert J. Searles in his book *Logic and Scientific Methods* (1956, p.44-45):

1. A heifer is red when every hair on its body is red;
2. A heifer is red when it is almost all red;
3. A heifer is red when the majority of its hairs are red;
4. A heifer is red when a considerable number of its hairs are red;
5. A heifer is red when one hair is red.

Although measurement classifies natural sciences as more exact than other sciences, there are still unresolved philosophical disputes. For instance, some philosophers regard any scientific theory concerned with measurable (quantifiable) magnitudes as intrinsically superior to a qualitative one, however rich and well organized the latter may be. This is a popular misconception, which is shared by many writers as the following quotation from John Mills (1972, p.41) attests:

> *Science today is quantitative rather than qualitative. It expresses the relationship of the intensities of the electric current and of the illumination of an incandescent lamp and compensates for its inability to answer the question "how" by its wealth of data as to "how much". Research monograph and textbooks alike emphasize the observable quantitative relationship and rarely venture far into the speculative hinterland where "how" must precede "how much". As we teach science today in our schools the effort of learning the quantitative relationships too frequently leaves neither the instructor nor the student leisure for fruitful inquiry or speculation as to the mechanism itself.*

[c] The Mechanical Principle

The aim of science is not only to discover and describe events and phenomena in the world but also, and more importantly, to explain scientifically these events and phenomena as they occur. Ernest Nagel (1961, p.4) observes, "Science seeks to discover and to formulate, in the general terms, the conditions under which events of various sorts occur, of such determining conditions being the explanations of corresponding happenings."

From Nagel's observation, the formal structure of science can be noted. Every natural science has statements, which include also formal and mathematical statements. These may be mathematical algorithms or procedures. The formal structure of science has dominated recent debate in the philosophy of science. The debate is explicitly based on a presupposition inherited from Rene Descartes and Plato, that the intellectual content of any natural science can be expressed in a formal propositional system having a definite and essential logical structure. The logical structure is what Nagel concisely called "the structure of science." Nagel has written a book titled: *The Structure of Science* (1961) in which he explicates the logical structure of science.

The same techniques were taken over into the philosophy of mathematics by a pioneer German logician Gottlob Frege, and into symbolic logic by Bertrand Russell and his collaborator Alfred North Whitehead. From 1920 on, the Viennese positivists and their successors, attempted to empty them in the philosophy of science hoping to demonstrate the validity of formal patterns of scientific inference by the straightforward extension of methods already familiar in deductive logic (Jon Wheatley, 1970, pp.99-105). The search for a logical structure in science is based on the expectation that it would be possible to demonstrate the existence of formal structures that were essential to any science and thereby identifying the science's laws, principles, hypotheses, and observations (Cannavo, 1974, pp. 113-114).

Underlying the mechanical principle is the basic scientific axiom of experimental science that, circumstances being unchanged, a like cause will produce a like result. The scientist is, then, interested in discovering the laws, which govern events in the universe. These laws are referred to as laws of nature. The laws of nature are statements of the mechanical phase of nature. They state the uniformity of correlation and sequence which events manifest. Here are some examples of laws of nature. These examples have been drawn from A.F. Chalmers' work (1980, p.36).

1. All iron rusts when exposed to air (provided there is moisture also);
2. All metals conduct electricity;
3. All poison kills.

One characteristic of the laws of nature is that they apply to all members of a given class without exception. For instance, a scientist to arrive at the claim that "All poison kills", he must have tested all kinds of poison available at all times and at all places. Hence, laws of nature must be spatio-temporal (*Encyclopedia of Philosophy*, Vol. 3 & 4, 1967, pp. 411-413). Since laws of nature apply to all places and times, a scientist can use them as a basis for prediction. For example, a scientist can successfully predict that given any piece of metal, that piece of metal will conduct electricity in future instances. The mechanical principle can then be summed up as the search for laws of nature, which govern uniformities in the universe.

[d] **Cooperative Principle**

In their struggle to overcome prejudice and to gain objectivity, members of the scientific community set forth varied and competing hypotheses – and then await the confirmations or disconfirmations of these hypotheses by others. According to Henry J. Ehlers (1976, p.151), "a scientist is not a prophet. He does not enunciate a fruit from the housetops and expect others to believe him". The scientist reports his/her assumptions, experimental procedures, and logically derived conclusions as accurately as he/she can. His/her colleagues then check these assumptions, and repeat his/her experiments under various and varied conditions. Only then are his/her original conclusions accepted, and, in most cases, they are accepted only with further revisions and modifications, that may have been found necessary.

Scientists report their findings in scientific 'publications'. A scientific publication is more than a mere statement that "so-and-so" has discovered "such-and-such" facts. Ehlers states:

> *Any scientific publication worthy of the name must include a clear and open description of all the relevant details of the methods whereby the data were gathered, or of the thinking and the assumptions on which the deductions are drawn. In this way it is possible for others to repeat the observations or the deductions (Ibid.)*

The reason for transforming private knowledge into public knowledge is that single individuals are more likely to be mistaken than groups of individuals. Although it is generally true that single individuals are more likely to be mistaken than groups of individuals, sometimes the individuals are more likely to be mistaken than groups of individuals, sometimes the individual is right and the group is wrong.

At the centre of the cooperative principle is the view that science is a social process (David Wield, 1986; John Ziman, 1989). In the nineteenth century, for instance, science expanded successfully into new fields of inquiry. This was greatly aided by the establishment of social centres to carter for scientific development. The *Encyclopedia Britannica* (Vol. 16, p.373) puts it thus:

> *This was greatly aided by the establishment of new and reformed universities in which research was fostered, as well as teaching, and of communication through specialist journals and societies. National and international meetings, for both general science and specialists, became common by the end of the century. The principle of socially organized research, rather than inquiries by isolated individuals, became effective.*

The Encyclopedia adds that in the early twentieth century,

> *Science was professional in its social organization, reductionist in style, and positive in spirit.... Almost all research was done by highly trained experts, employed wholly or mainly for this work within special institutions. Communities of scientists, organized by discipline and by nationality, enjoyed a high degree of autonomy in the setting of goals and standards of research and in the certification, employment, and rewarding of their members (Ibid.).*

From the foregoing discussion, it can be summarized that the cooperative principle is that principle, which governs the scientist in his/her operations within the larger social set up. So much for the principles, that a practicing scientist follows in the execution of his/her duty. It is now the concern of the next section to examine whether or not Kuhn has given a correct description of the actual practice of science as described in the foregoing section.

4.2 KUHN AND THE ACTUAL PRACTICE OF SCIENCE

This section relates the Kuhnian paradigmatic account of science with the actual practice of science with the express aim of determining whether or not Kuhn has given the correct description of science the way it is practiced. This section first states Kuhn's claim and then proceeds to point out whether or not Kuhn's account of science reflects the actual practice of science. Consider this quotation (Kuhn, 1962, pp.176-178):

> *If this book were being rewritten, it would therefore open with a discussion of the community structure of science, a topic that has recently become a significant subject of sociological research and that historians of science are also beginning to take seriously. Preliminary results, many of them still unpublished, suggest that the empirical techniques required for its exploration are non-trivial, but some are in hand and others are sure to be developed. Most practicing scientists respond at once to questions about their community affiliations, taking for granted that responsibility for the various current specialties is distributed among groups of at least roughly determinate membership. I shall therefore here assume that more systematic means for their identification will be found. Instead of presenting preliminary research results, let me briefly articulate the intuitive notion of community that underlies much in the earlier chapters of this book. It is a notion now widely shared by scientists, sociologists, and a number of historians of science.*
>
> *A scientific community consists, on this view, of the practitioners of a scientific specialty. To an extent unparalleled in most other fields, they have undergone similar education and professional initiations; in the process they have absorbed the same technical literature and drawn many of the same lessons from it. Usually the boundaries of that standard literature mark the limits of a scientific subject matter, and each community ordinarily has a subject matter of its own. There are scholars in the sciences, communities, that is, which approach the same subject from viewpoints. But they are far rarer there than in other fields; they are always in competition; and their competition is usually quickly ended. As a result, the members of a scientific community see themselves and are seen by others as the men uniquely responsible for the pursuit of a set of shared goals, including the training of their successors. Within such groups communication is relatively full and professional judgement relatively unanimous. Because the attention of different scientific communities is, on the other hand, focused on different matters, professional communication across group lines is sometimes arduous, often results in misunderstanding, and may, if pursued, evoke significant and previously unsuspected disagreement.*
>
> *Communities in this sense exist, of course, at numerous levels. The most global is the community of all natural scientists. At an only slightly lower level the main scientific professional groups are communities: physicists, chemists, astronomers, zoologists, and the like. For these major groupings, community membership is readily established except at the fringes. Subject of highest degree, membership in professional societies, and journals read are ordinarily more than sufficient. Similar techniques will also isolate major subgroups; organic chemists, and*

perhaps protein chemists among them, solid-state and high-energy physicists, radio astronomers, and so on. It is only at the next lower level that empirical problems emerge. How, to take a contemporary example, would one have isolated the phage group prior to its public acclaim? For this purpose one must have recourse to attendance at special conferences, to the distribution of draft manuscripts or galley proofs prior to publication, and above all to formal and informal communication networks including those discovered in correspondence and in the linkages among citations.... Usually individual scientists, particularly the ablest, will belong to several such groups either simultaneously or in succession.

Communities of this sort are the units that this book has presented as the producers and validators of scientific knowledge. Paradigms are something shared by the members of such groups. Without reference to the nature of these shared elements, many aspects of science described in the preceding pages can scarcely be understood.

There are two distinct steps we need to take in order to clarify a passage like this. First, we must identify what point the writer is trying to establish; that is, one must identify the writer's conclusion. Secondly, we must unveil the argument by which he/she attempts to establish his/her conclusion. As Jon Wheatley (1970, p.89) puts it, "it is frequently the case in philosophy that we cannot fully understand some thesis until we understand the argument which leads up to it". Attention now is drawn on how one can go through the two steps in understanding the above Kuhnian passage.

Kuhn gives his conclusion in the first sentence of the last paragraph of the quoted passage: "Communities of this sort are the units that this book has presented as the producers and validators of scientific knowledge". In other words, the practice of science is determined by the activities of the scientific community, or the actual practice of science is equivalent to the activities of the scientific community. Having got hold of Kuhn's conclusion, attention is now turned to untangling the argument by which he tries to establish it. To do this, what might be called a "first-reading" of the Kuhnian passage is given. Then the same argument is reconstructed.

Kuhn is of the opinion that the role played by scientific community in any practice of science is immense. That is why he opens his passage with this sentence: "If this book were being rewritten, it would therefore open with a discussion of the community structure of science…." A scientific community is determined by the paradigm its members share. In case of questions concerning their conception of science, "most practicing scientists respond at once to questions about their community affiliations, taking for granted that responsibility for the various current specialties is distributed among groups of at least roughly determinate membership". The concerned community consists of the practitioners of a scientific specialty. "As a result, the members of a scientific community see themselves and are seen by others as the men uniquely responsible for the pursuit of a set of shared goals, including the training of their successors". Since these communities share the same goals, judgement in scientific matters are "unanimous". He concludes by saying, "without reference to the nature of these shared elements, many aspects of science described in the preceding pages can scarcely be understood".

From the foregoing "first reading" of Kuhn's argument, one gathers that Kuhn's account of science lays emphasis on the scientific community: "a community which shares same goals; that is, paradigms" (Kuhn, 1962, p.178). A reconstruction of the same argument is now desirable and the schematization of Kuhn's argument follows below.

1. Scientific change is determined by "paradigm shifts";
2. Scientific communities are determined by the paradigms they uphold;
3. Therefore, scientific change is not universal.

It is now shown how each premise is related or leads to the conclusion. Each premise will be taken singly.

In the first premise, Kuhn reasons that in normal science scientists "know what the world is like." But when scientists start questioning this "normal science" that becomes the start of a scientific change; that is, scientists start looking at nature from a different paradigm. The second premise is related to the first premise in this way. According to Kuhn, scientists work within a community committed to a shared framework of theory; and "the members of a scientific community see themselves and are seen by others as the men uniquely responsible for the pursuit of a set of shared goals, including the training of their successors" (Ibid., p.177). The foregoing explains why we have the community of "physicists, chemists, astronomers, zoologists, and the like" (Ibid.).

From the two premises, Kuhn inferred the conclusion. If scientific change is determined by "paradigm shifts" and since "scientific communities" are determined by paradigmatic-adherence, then, in cases of competing paradigms, scientific change will not be universal, since each competing paradigm will have its disciples. This argument is a case of a strong inductive argument. It is improbable that the conclusion is false and the premises are true. The evidential link between the premises and the conclusion is strong. It is now the concern of the following part to examine whether Kuhn's account captures the actual practice of science.

In section 4.1 above, it was stated that the scientist operates on four fundamental principles; namely, 1) the empirical principle; 2) the quantitative principle; 3) the mechanical principle; and 4) the cooperative principle. Consequently, any account of science, which does not follow these principles, does not reflect the actual practice of science. The four principles are implicitly entailed in the scientific method, which is rule-governed. The fourth principle is also sociological in nature, since it deals with the societal organization of science. Kuhn's account of science centres on the cooperative principle since he lays great emphasis on scientific communities. Therefore, although Kuhn's account of science does not satisfy the strict criteria of science according to the scientific method, it meets some broader criteria for 'scientific accounts' latent in the sociological practice of science (Barry Barnes, 1982, p.viii; Kragh Helge, 1994, p.23).

That Kuhn's account of science meets some broader criteria for scientific accounts latent in the sociological practice of science is a sharp move from the traditional view of the sociology of science associated with the work of Robert Merton which makes a sharp distinction between science as a cognitive system and science as a social as social system and thus opening up the possibility of sociological studies of the development and evaluation of specific ideas.

This means, to the extent that Kuhn's account of science meets the "cooperative principle" of science, as argued in this chapter, to the same extent, Kuhn's account of science reflects the actual practice of science. The same argument can be presented in the following schemata:

1. Scientific practice is governed by the scientific method;
2. The scientific method requires the practicing scientist to follow the following four principles:
   1) the empirical principle,
   2) the quantitative principle,
   3) the mechanical principle,

4) the cooperative principle;

3. Kuhn's account follows only the cooperative principle, since Kuhn emphasizes the role of the 'scientific community' in his notion of science;

4. The analysis of scientific community reveals that it is sociological in its nature;

5. Therefore, to the extent that Kuhn's account of science is sociological in nature, then to the same extent, Kuhn's account of science captures the actual practice of science.

CHAPTER FIVE: KUHN AND THE SCIENTIFIC PROGRESS

5.0 Introduction

Philosophers of science are generally in agreement that science is a progressive enterprise. But these philosophers of science disagree on the nature of scientific progress. The problem of the nature of scientific progress has preoccupied philosophers of science in the last three decades. For instance, Larry Laudan (1977, p.2) writes:

> *For a long time, many have taken the rationality and progressiveness of science as an obvious fact or a foregone conclusion, and some readers will probably still think it bizarre to believe that there is any important problem to be solved here. Although this confident attitude has been almost inescapable given the cultural biases in favour of science in modern culture, there have been a number of recent developments, which bring it into serious question.*

In answering the question: "How does science progress?" philosophers of science have assumed two positions: first, the position that scientific progress is cumulative in its nature and second, the position that scientific progress is revolutionary in its nature. These two positions shall hereafter be referred to as cumulativistic and revolutionaristic, respectively.

Cumulativism in science is the view that scientific progress is additive in its nature. In other words, scientific cumulativism is a piecemeal process by which scientific items have been added, singly and in combination to the ever-growing stockpile that constitutes scientific technique and knowledge. Carlo Lastrucii, in his book, *The Scientific Approach* (1967) writes;

> *Science is cumulative, it is an integrated system built up in an orderly manner wherein each fact, principle, theory, law, etc, supports other facts, theories, laws etc. But science is not a mere accumulation. A cookbook, telephone directory, a stock-market report, are all assemblages of accumulated facts-but they are not science (p.13).*

Many philosophers of science including Karl Popper (1975), David Lamb (1984), and Larry Laudan (1978), among others, share Lastrucci's stance. The mentioned philosophers share the view articulated by Lamb to the effect:

> *Scientific progress is the result of a chain of discoveries of relative importance. Each discovery is dependent upon previous work, even when it is associated with dramatic revolutions in science. Although it is cumulative, scientific development is far from regular. There are periods of stagnation, periods of rapid acceleration, and periods in which fruitful chains of connections are made (p.53).*

Cumulativistic science is opposed to revolutionalistic science. The latter is the view that science progress by one paradigm replacing its competitor. The replacement does not follow any rules; it is chaotic. Kuhn states:

> *Confronted with anomaly or with crisis, scientists take a different attitude toward existing paradigms, and the nature of their research accordingly.... Scientific revolutions are inaugurated by a growing sense that an existing paradigm has ceased to function adequately in the exploration of an aspect of nature to which that paradigm itself had previously led the way (Kuhn, 1962, pp:90-91).*

Kuhn's interpretation of scientific progress places "paradigm" central to the whole structure of scientific development. Kuhn stipulates how a scientific revolution is arrived at. We briefly present Kuhn's view on how scientific revolutions arise.

According to Kuhn, revolutions are responses to problems within traditions of research. They pivot around an accumulated cluster of recalcitrant anomalies. The group of scientists trained for normal research is at the same time a sensitive detector of anomaly. Hence much normal research tends to focus upon anomaly. Over time, these recalcitrant anomalies accumulate as the by-products of normal research and they can, in some circumstances, generate a malaise, or even a sense of crisis, among practitioners. The existence of an accumulating residue of problems, which persistently resist all attempts at assimilation, may, according to Kuhn, at last prompt the widespread suspicion that something is at fault at the heart of normal research itself.

The response of a crisis of this kind typically involves a change in the character of research. Speculation becomes more acceptable. Novel and radically deviant procedures and interpretations are tried. Paradigms and the activities and judgements based upon them are called into question. They are not, however, discarded:

> *No scientific community ever simply throws aside its tools and abandons research. Only when a new paradigm is agreed upon, as an adequate response to current difficulties and acceptable foundations for future work, only then can the existing basis for research be set on one side. At this point a large scale reordering of practice and perception occurs, reflecting the requirements exemplified in the new paradigm; and the conceptual fabric undergoes an analogous reconstruction. The scene is now set for a new sequence of normal science to develop: a scientific revolution has occurred (Kuhn, 1962, pp.90-91).*

Kuhn's position can be put in a few words: '' when a scientific revolution has occurred, science can be said to have progressed."

From the foregoing discussion, a dilemma emanates. Either we accept that science progresses cumulatively and deny the revolutionaristic conception of scientific progress or we deny the former and uphold the latter. Whichever position we uphold, the conclusion must be that science is progressive. In the first section of this chapter, an examination of what constitutes scientific progress is shown. The next section concerns itself with relating "scientific progress", as clarified in the first section, to Kuhn's account of science with the express aim of determining whether or not he has given a correct account of how science progresses. It should be noted that Kuhn dedicates a whole chapter to show that science progresses through revolutions. The chapter in question is chapter XIII: "Progress through Revolutions" (Kuhn, 1962, pp. 159-ff.)

5.1 THE NATURE OF SCIENTIFIC PROGRESS

According to the *Oxford Advanced Learner's Dictionary* (1995), the term "progress" is defined as "forward movement or advance or development, especially towards a better state". But it is questionable whether progress has always been for the better. According to Charles Frankel's article, "The Idea of Progress" in *The Encyclopedia of Philosophy Vol.5&6* (1967, pp. 483-487), in many societies there has been a popular conviction that man's condition has changed in the course of history but for the worse; that is, their conditions have retrogressed. Characteristically, when men have believed in a golden age, they have put that age in the past rather than in the future as the saying goes "old is gold". In contrast, in modern societies, change and innovation have a different place in the popular imagination. Not everyone assumes that all change is necessarily for the better, but it is widely assumed, even by conservatives, that only a society which has a general capacity to change is capable of surviving: "They have supposed that this improvement would be cumulative and continuing and that although temporary setback, accidents and disasters might take place, human knowledge, power, and happiness would increase over the long run" (Ibid., p.483). The foregoing claims can be summarized thus: progress is necessarily change, although not all change is for the better.

The emergence of the idea that "human knowledge, power, and happiness would increase over the long run" is the product of a variety of circumstances, such as the accumulation of an economic surplus, the increase of social mobility, and the occurrence of major inventions that have dramatically increased human power over nature. Over and above these, however, the idea of progress is peculiarly a response to the emergence of the unique social institution of organized scientific inquiry (Ibid.).

History of science testifies that the idea of scientific progress has its roots in the works of Francis Bacon, Rene Descartes and Blaise Pascal. For instance, in 1647, Pascal in his *Nouvelles experiences touchant le vide* (translated by Peter Rush as *New Experiences Related to Nothing*, 1923) said,"the experiments which gives an understanding of nature multiply continually... from whence it follows that only each man advances in the sciences day by day, but that all men together make continual progress in them as the universe grows older "(Pascal, 647, p.93).

Pascal believed, however, that such progress took place only where the experimental methods of the sciences were relevant. In the eighteenth century, however, and particularly in France, an increasing number of intellectuals came to believe that the methods and spirit of science should be applied to all fields. In consequence, the idea of progress came to include a concept of social and moral progress (*Encyclopedia of Philosophy Vol. 5&6*, 1967, p.483).

When we talk of scientific progress, it is useful to distinguish between two motifs. The first motif is in showing that the sciences, usually some particular science, had uncovered fundamental truths that had been previously unknown to humans and that progress would now take place if only people/humans accepted these truths as guides to practice. Consider this extract from the article "New hope in treating arthritis" by Wandera Ojanji as extracted from The Daily Nation, May 10,2001.

Scientists have made a breakthrough in the treatment of rheumatoid arthritis, a disease characterized by pain, inflammation and stiffness of the joints. Medical researchers have developed a combination of plant fats (Sterols and sterolins – named aesterinol (in the U.S) and Moducare (in South Africa). Sterinol has been proved effective in the management and treatment of rheumatoid arthritis and other autoimmune diseases, a disorder that occurs when the immune system begins to attack the body....

Sterols and sterolins enhance preferentially the activity of T help cells that fight foreign organisms in the body that cause disease. The sterols and sterolins also enhance the release of other factors that introduce inflamation, the body's defense mechanism.... (Daily Nation, Thursday, May 10, 2001).

From the above extract we gather that if scientists and other practitioners correctly implemented the use of sterols and sterolins then arthritis will be greatly reduced or completely done away with. Or, consider this case from Skyrmean logic:

When an argument is such that the truth of the premises guarantees the truth of the conclusion we shall say that it is deductively valid. When an argument is not deductively valid but nevertheless the premises provide good evidence for the conclusion, the argument is said to be inductively strong. How strong it is depends on how much evidential support the premises give to the conclusion (Brian Skyrms, 1966, p.7).

The above reasoning was arrived at after the discovery that the "old logic" which distinguished the deductive from the inductive as the former proceeding from the general to the particular while the latter as advancing from the particular to the general, was faulty. It is now agreed that logical arguments can be advanced, both general to general and specific to specific in addition to the above distinction into which both fit (Ibid., pp.13-14). The uncovering of the truth that both the deductive and inductive can advance from general to specific and vice versa, tallies with the first motif of science, hence,Skyrmean logic is advancement in logic.

A second motif in the theory of scientific progress does not associate progress with any particular discoveries of science or reason but with the unique self-corrective methods of science. The presupposition in this second motif of scientific progress is the view that science is not infallible; that scientists can also make mistakes but is ready to self-correct them upon new evidence. For instance, for many years chloroquine has been used to cure malaria in Kenya. But it has been found out that is no longer effective since many malaria victims have become resistance to its use. It has been replaced with another sulfur based drug sulfadoxine pyrimethamine (SP), because it gives a fast relief from malaria symptoms, particularly fever (*Daily Nation*, Thursday May 10, 2001, p.26).

Consider this example from astronomy which has been extracted from George Patrick (1978, p.63):

> *.... the Ptolamaic system seemed to explain the facts fairly well and prevailed throughout the Middle Ages, furnishing finally the cosmological foundation for Dante's great poem, The Divine Comedy. But, with the increase of astronomical knowledge in the sixteenth century, Copernicus became dissatisfied with this theory and made another hypothesis, resulting in the new Copernican System. He proposed to consider the sun as the centre of the solar system, the earth and the other members of the system revolving around the sun in circles.*

From the foregoing quotation, we realize that Copernicus was correcting the mistakes, which were entailed in the Ptolemaic system. Johannes Kepler who, in turn, was corrected by Isaac Newton, also later corrected the Copernican system. In short, science is an ever self-correcting process. The view that science progresses by self-correction raises one major issue, which needs to be addressed. Objectors to "scientific progress" have argued that to talk of science progressing by self-correction involves a self-contradiction. The belief that there is a scientific progress is usually attached to the argument that science is continually self-corrective. But if science never does anything but correct itself, is there any sense of speaking of scientific progress? Does not the concept of progress presuppose a fixed end or standard, and does not science emphasize fallibilism and thus deny that there can be fixed ends or standards? According to this reasoning, "progress", then, becomes a meaningless term,

Upon close examination, the above objection fails "once it is recognized that progress can also refer to the solution of particular problems, not only to the movement towards a general absolute goal" (*Encyclopedia of Philosophy*, 1967, Pp.487). For instance, meaning can obviously be assigned to the statement that science has made progress in determining the causes of malaria or in describing the characteristics of the other side of the moon. *The Encyclopedia of Philosophy* adds, "once scientific progress is defined in terms of the solutions to particular problems, sense can be given to the notion of cumulative scientific progress, for the general scientific capacity to solve problems has also tended to grow" (Ibid.)

The accumulation of increasingly well tested and continuously powerful ideas by the sciences is an obvious fact of their history. For instance, Claudius Ptolemaeus' astronomy, as presented in his principal work, *The Almagest*, has survived for many centuries. Although it was found to be an inadequate explanation of planetary movement, Copernicus revised it. Kepler refined the Copernican system. Kepler's views were found to be inadequate in explaining the motion of Mars. Consequently, Newton reviewed Kepler's view. The foregoing chronological account of the cumulative history of astronomy is best exemplified by this extract from George Patrick (1978, Pp.62-64).

> *Previous to the advent of the Greek scientists, mythological explanations satisfied the minds of the scientists. The Greeks themselves wrestled with the problem (planetary motions) for several hundred years. Finally, Ptolemy of Alexandria, about A.D.150, undertook a complete scientific explanation. He offered a hypothesis or theory to explain all the motions of the stars. This affirmed that the earth is fixed and stationary at the centre of the system, the sun, moon and stars revolving around it, a complicated system of cycles and epicycles explaining the peculiar motions of the planets.*

> *This theory, called the Ptolemaic system, seemed to explain the facts fairly well and prevailed throughout the Middle Ages, furnishing finally the cosmological foundation of Dante's great poem,* The Divine Comedy. *But, with the increase of astronomical knowledge in the sixteenth century, Copernicus became dissatisfied with this theory and made another hypothesis, resulting in the new Copernican system. He proposed to consider the sun as the centre of the solar system, the earth and the other members of the system revolving around the sun in circles. This new theory fitted the facts better – but not perfectly. Kepler, studying the motion of Mars, substituted ellipses for circles as the paths of the planets. This explained still more facts, but left others unexplained. Then Newton came and proposed a wonderful and far-reaching law for all the heavenly bodies, namely, the law of gravitation, according to which every object in the universe attracts every other object with a force varying inversely as the square of the distance. This new law was found at once to explain in a marvelous way all the motions of all the heavenly bodies – even the motions of those mysterious and erratic bodies called comets. It explained also the falling of objects upon the earth, the trajectories of balls and bullets and the movements of the tides.*
>
> *With the coming of Newton's great law, the scientific world was satisfied. The secret of the motions of bodies celestial and terrestrial was laid bare. Not until the present century did any doubts arise. But increasingly accurate instruments finally detected something wrong with the planet Mercury. Leverrier observed that it does not move in accordance with Newton's laws. Its elliptical orbit itself turns in a manner bewildering to the Newtonian physics.*
>
> *So then another revision is made and now Einstein comes forward with his famous theory of relativity, explaining not only the erratic behaviour of Mercury but also a great number of other facts difficult hitherto to understand. The part played by deduction in scientific method is illustrated in the verification of Einstein's law. If the relativity theory is valid, then, the light of a distant star passing near the sun should be deflected from its straight-line course. Astronomers waited for a total eclipse of the sun and the deflection was shown to occur in accordance with Einstein's law.*

Basic to the above quotation is the view that no theory in science can claim finality in itself. Scientific theories have and still undergo refinement every time new evidence is found. Underlying the above claim is the presupposition that science is not dogmatic since it can change with time upon new evidence as it is attested by the following sentence from the last paragraph of the quoted passage: "So then another revision is made and now Einstein comes forward with his famous theory of relativity, explaining not only the erratic behaviour of Mercury but also a great number of other facts difficult hitherto to understand". But it should be noted that a "refined" theory does not necessarily mean that it accurately and completely describes nature. Kepler's substitution of ellipses for circles as the paths of the planets, for instance, did not negate heliocentricism. Instead, Kepler's theory added more knowledge to the understanding of the planetary motions.

A review of the theories on the motions of the celestial bodies might show that science is a history of discarded theories, but, as James Jeans commented that science is "ever progressing through a succession of theories, each of which covers more phenomena than the predecessor it displaced, towards the goal of a single theory which shall embrace all the phenomena of nature" (p.19). Although the history of science testifies to the fact that scientific progress is cumulative, doubt has been thrown on the conclusion that scientific progress is cumulative. It has been argued that the history of science is the record of revolutions in scientific theory so radical in character it is impossible to establish the continuity between the ideas of one generation and the ideas of a later one.

If the above claim were true, it would be impossible to establish a concept of progress, since such a concept presupposes a measure of continuity in the sequence of events under examination. Underlying this view is the thesis that theories are examined using a specific theoretical framework. When this theoretical framework changes, observations are simply run through a different set of conceptual categories. Accordingly, it makes little sense, it is argued by some philosophers of science, that the sciences have improved or extended their knowledge, for all that has happened is that one body of beliefs has been substituted for another.

This point of view raises some epistemological and methodological questions. It appears to leave out of account, say, and the consideration that, fundamental principles of Newtonian physics can, with appropriate modifications, be absorbed into modern physical theories. If science progresses through revolutions, one would wonder why Newtonian physics continue to provide reliable instruments for the explanation and prediction of events in large sectors of macrophysics. Attention is now turned to relating scientific progress to Kuhn's account of science to determine whether Kuhn has captured the nature of scientific progress discussed in the foregoing section.

5.2 KUHN AND SCIENTIFIC PROGRESS

This section relates the Kuhnian paradigmatic account of science with the nature of scientific progress discussed in section 5.1. This section first states Kuhnian's claim and then proceeds to consider/determine whether or not Kuhn's account of science reflects the nature of scientific progress. Consider Kuhn's chapter XIII that is wholly dedicated to showing that science "progresses through revolutions". In chapter XIII: "Progress through Revolutions", Kuhn writes:

> *The preceding pages have carried my schematic description of scientific development as far as it can go in this essay. Nevertheless, they cannot quite provide a conclusion. If this description has at all caught the essential structure of a science's continuing evolution, it will simultaneously have posed a special problem: why should the enterprise sketched above move steadily ahead in ways that, say, art, political theory, or philosophy does not? Why is progress a prequisite reserved almost exclusively for the activities we call science? The most usual answers to that question have been denied in the body of this essay. We must conclude it by asking whether substitutes can be found.*

Notice immediately that part of the question is entirely semantic. To a very great extent the term 'science' is reserved for fields that do progress in obvious ways....

It can, however, only clarify, not solve, our present difficulty to recognize that we tend to see as science any field in which progress is marked. There remains the problem of understanding why progress should be so noteworthy a characteristic of an enterprise conducted with the techniques and goals this essay has described.... Does a field make progress because it is a science, or is it a science because it makes progress? Ask now why an enterprise like normal science should progress, and begin by recalling a few of its most salient characteristics. Normally, the members of a mature scientific community work from a single paradigm or from a closely related set. Very rarely do different scientific communities investigate the same problems. In those exceptional cases the groups hold several major paradigms in common. Viewed from within any single community, however, whether of scientists or of non-scientists, the result of successful creative work is progress. How could it possibly be anything else? We have, for example, just noted that while artists aimed at representation as their goal, both critics and historians chronicled the progress of the apparently united group. Other creative fields display progress of the same sort. The theologian who articulates dogma or the philosopher who refines the Kantian imperative contributes to progress, if only to that of the group that shares his premises. No creative school recognizes a category of work that is, on the one hand, a creative success, but is not, on the other hand, an addition to the collective achievement of group. If we doubt as many do, that non-scientific fields make progress, that cannot be because individual schools make none. Rather, it must be because there are always competing schools, each of which constantly questions the very foundations of the others. The man who argues that philosophy, for example, has made no progress emphasizes that there are still Aristotelians, not that Aristotelianism has failed to progress. These doubts about progress arise, however, in the sciences too. Throughout the pre-paradigm period when there is a multiplicity of competing schools, evidence of progress, except within schools, is very hard to find.... In short, it is only during periods of normal science that progress seems both obvious and assured. During those periods, however, the scientific community could view the fruits of its work in no other way.

With respect to normal science, then, part of the answer to the problem of progress lies simply in the eye of the beholder. Scientific progress is not different in kind from progress in other fields, but the absence at most times of competing schools that question each other's aims and standards makes the progress of a normal-scientific community far easier to see. That, however, is only part of the answer and by no means the most important part. We have, for example, already noted that once the reception of a common paradigm has freed the scientific community from the need constantly to re-examine its first principles, the members of that community can concentrate exclusively upon the subtlest and most esoteric of the phenomena that concern it. Inevitably, that does increase both the effectiveness and the efficiency with which the group as a whole solves

> *new problems. Other aspects of professional life in the sciences enhance this very special efficiency still further.... In its normal state, then, a scientific community is an immensely efficient instrument for solving the problems or puzzles that its paradigms define. Furthermore, the result of solving those problems must inevitably be progress. There is no problem here. Seeing that much, however, only highlights the second main part of the problem of progress in the sciences. Let us therefore turn to it and ask about progress through extraordinary science. Why should progress also be the apparently universal combatant of scientific revolutions? Once again, there is much to be learned by asking what else the result of a revolution could be. Revolutions close with a total victory for one of the two opposing camps. Will that group ever say that the result of its victory has been something less than progress? That would be rather like admitting that they had been wrong and their opponent's right. To them, at least, the outcome of revolution must be progress, and they are in an excellent position to make certain that future members of their community will see past history in the same way. Section XI described in detail the techniques by which this is accomplished, and we have just referred to a closely related aspect of professional scientific life. When they repudiate a past paradigm, a scientific community simultaneously renounces, as a fit subject for professional scrutiny, most of the books and articles in which that paradigm had been embodied, scientific education makes use of no equivalent for the art museum or the library of classics, and the result is a sometimes drastic distortion in the scientists' perception of his discipline's past. More than the practitioners of other creative fields, he comes to see it as leading in a straight line to the discipline's present vantage. In short, he comes to see it as progress. No alternative is available to him while he remains in the field.*

Central to the above quotation is Kuhn's claim that scientific progress is achieved when revolutions have occurred. Kuhn puts his conclusion in a form of question: "Why should progress also be the apparently universal commitant of scientific revolutions?". He adds, "revolutions close with a total victory for one of the two opposing camps. Will that group ever say that the result of its victory has been something less than progress?" Put more precisely, Kuhn's claim is that science is said to have progressed if there has occurred revolution, or that science progresses by revolutions. In view of Kuhn's stated conclusion, we must now untangle the argument by which he tries to establish it. After a first reading of the Kuhnian passage, a reconstruction of the same argument is made.

A first reading of Kuhn's argument would go thus: In normal research, fundamental assumptions are not questioned. Anomalies are set to one side, or accommodated by *ad hoc* modifications. But with a growing list of anomalies, a sense of crisis leads the scientific community to examine its assumptions and to search for alternatives. A new paradigm may then be proposed which challenges the dominant presuppositions. When a major change of paradigm does occur, it has such far-reaching effects that it amounts to a revolution. When a revolution has occurred, science is then said to have progressed. In his own words: "Scientific progress is not different in kind from progress in other fields, but the absence at most times of competing schools that question each other's aims and standards makes the progress of a normal-scientific community far easier to see. He adds, "to them (Scientific community), at least, the outcome of revolution must be progress, and they are in an excellent position to make certain that future members of their community will see past history in the same way."

From the first reading of the Kuhnian passage, we now proceed to show how Kuhn advanced his argument. He advanced his argument on the following premises:

1. Revolutions are responses to problems within traditions of research;
2. When recalcitrant anomalies accumulate they generate a crisis;
3. Revolutions occur after crisis;
4. Therefore, science progresses after revolutions have occurred.

In the first premise, Kuhn argues that when revolutions occur, they are responses to problems within a given paradigm. Scientific communities detect these problems in that given paradigm, that is, a group that upholds that paradigm. Since scientists are people who are trained in that paradigm, it is easy for them to detect any problem or anomaly in the working of that paradigm.

The second premise is related to the first premise this way. The presence of an anomaly or two is not sufficient to cause a revolution. For the anomalies to lead to a revolution, they must have accumulated to the extent that scientific community has to rethink the role played by the prevailing paradigm. The third premise is related to the second premise in that when there is repeated failure of "a normal science" tradition to solve a problem or other anomalies that develop in the course of paradigm articulation produce the tradition – shattering complements to the tradition-bound activities of normal science. The most pervasive of such tradition-shattering activities in the history of science Kuhn calls "scientific revolution". The conclusion that Kuhn draws is that if revolutions are governed by the presence anomalies and enough anomalies have accumulated, then, a scientific revolution occurs. When a scientific revolution has occurred, science can be said to have progressed. This is because, "confronted with anomaly or with crisis, scientists take a different attitude toward existing paradigms, and the nature of their research…" (Ibid., pp. 90-91).

Kuhn's position can be put briefly thus: with the accumulation of "enough" anomalies, a scientific revolution occurs and hence scientific progress. In other words, scientific progress is as a result of the accumulation of anomalies. It is now time to examine whether Kuhn's account captures the nature of scientific progress.

In section 5.1, it was stated that the history of science testifies to the advancement of science through the accumulation of scientific knowledge. Consequently, any account of scientific progress, which contradicts the cumulative element of scientific progress, is not a correct reflection of the history of science. Kuhn's account of science emphasizes the importance played by scientific revolution. *The Structure of Scientific Revolutions*

(1962) is a discussion on how science progresses through revolutions and not accumulation of scientific knowledge. In the preface to the said book, he says that his ideas on scientific revolutions "...would surely add an analytic dimension of first-rate importance for the understanding of scientific advance" (Ibid., p.vii). Since his account of science is not reflected from the history of science, it appears not to be a correct and accurate account of how science progresses.

A close examination of how revolutions occur reveals that they arise after an accumulation of anomalies. It is questionable whether revolutions themselves cannot accumulate to result in scientific progress. Therefore, Kuhn defends one thing, and his work implies another thing. That Kuhn's account of scientific progress is not reflected in the history of science can be schematized in the following way:

a) Kuhn claims that science progresses through revolutions;

b) Close examination of how the Kuhnian revolutions arise reveals that revolutions are a result of the accumulation of anomalies;

c) Therefore, Kuhn defends one thing and his work implies another thing;

d) Science progresses cumulatively as testified by the history of science;

e) Since Kuhn denies that science progresses cumulatively, then, his account of scientific progress is at variance with the history of science;

f) But since close examination of the Kuhnian scientific revolutions reveals that they are a result of cumulative anomalies and since Kuhn does not embrace cumulativism in science, then, Kuhn's account of scientific advancement is a case of a self-defeating thesis.

CHAPTER SIX: GENERAL CONCLUSION, FINDINGS AND RECOMMENDATIONS OF THE STUDY

6.0 **Introduction**

The aim of this study has been to determine whether or not Kuhn's paradigmatic account of science meets the requirements of the scientific method and, whether or not Kuhn or any other person has any firm basis to call Kuhnian paradigmatic account of science scientifically rational and progressive. The study also aimed at determining to what extent Kuhn's account of how scientific practice captures the social practice of science.

The Kuhnian paradigmatic account of science falters as a model of scientific rationality and progress because of the endemic epistemological presupposition of the "quest for certainty" latent in the Platonism which the idea of paradigm presupposes, that is, the idea that paradigms are moving to higher and perfect forms. The central doctrine in Platonism is the idea of *Forms*. Plato's Forms are sometimes referred to as *Ideas*, but Plato does not mean ' ideas' in a person's mind rather ideal forms or perfect examples—the perfect circle or perfect beauty. To avoid confusion, the word "Forms" and not "Ideas" is used.

Forms are the ultimate reality. Things change, people grow old and die, but Forms are eternal and unchanging. Thus, according to Popkin (1986), Plato could agree with Heraclitus that the world of our experience is constantly changing; but he could also agree with Parmenides, who insisted that the real world, the eternal and unchanging world, was not the same as the world of our experience. According to Plato, it was a world of Forms, a world of eternal truths. There were, in other words, two worlds: 1. The in which we live, a world of constant change or a world of Becoming, and 2. A world of Forms; an unchanging world, the real world of Being. We can see here Plato's close connection with Heraclitus, holding that ultimate reality (the Forms) must be changeless and eternal. Furthermore, also in accordance with Parmenides, it is only such changeless and eternal things that truly can be known.

That Forms are eternal and changeless is exemplified by this extract from Plato's *The Symposium:*

> *You see, the man who has been thus far educated in matters of Love, who has beheld beautiful things in the right order and correctly, is coming now to the goal of Loving: he will certainly catch sight of something wonderfully beautiful in its nature; that is the reason for all his earlier labours: First, it always is, and neither comes to be nor passes away, neither waxes nor wanes. Second, it is not beautiful this way and ugly that way, nor beautiful at one time and ugly at another; nor beautiful in relation to one thing and ugly in relation to another; nor is it beautiful here but ugly there. Nor will he perceive the beautiful in an image like a face, or hands or some other part of a body. Nor will he find it in a theory or in any scientific understanding. It is not anywhere in another thing, as in an animal or in earth, or in heaven or in anything else.*
>
> *But itself by itself with itself, is always one in Form; and all the other beautiful things share in that Form, in such a way that when those others come to be or pass away, the Form does not become the least bit smaller or greater, nor suffers any change (211a-b).*

From the above passage, Plato thought that it is the world of Forms that is real. But this is not to say that the world we live in, the world of Becoming is unreal. It is, however, less than real; without those qualities of eternity and necessity that are the marks of true reality.

Plato's position regarding the Forms can be briefly restated thus: knowledge consists in the apprehension of those qualities of the world, which never change, never alter. He believed that the world contained such constituent elements-the Forms. He suggested that our ordinary concepts (for example, Wisdom, Justice, Beauty and Goodness) include the use of general terms, and that in order for our ordinary statements to be meaningful, one must know what these general terms signify. To do this, Plato insisted, one must do more than merely point to various particular things. Those things would only be, at best, examples of things that fall into general classifications, but would not themselves be classifications. In short, Plato is saying that we have corresponding images to every concept. But it should be realized that we have some concepts without any corresponding images, for example, "God", "liberty" or even "slavery". These are abstract concepts to which there are no corresponding images although we distinguish cases of the application of these words from cases of their non-application. The "Kuhnian paradigm" is a case of an abstract concept without any corresponding "paradigm" (image). In other words, Kuhn's account of paradigms presupposes the "quest for certainty", that is, the search for an ideal paradigm to which all paradigms should correspond. This may not be attainable (John Dewey, 1968, p. 765; Richard Bernstein, 1967 pp. 380-385).

That Kuhn's paradigmatic account of science presupposes the idea of Platonism is implicitly shown in this passage from Kuhn (1962):

> *at the start [a paradigm is] largely a promise of success discoverable in selected and still incomplete examples (p.23), [it is] an object for further articulation and specification under new and more stringent conditions (ibid.); [hence from paradigms] spring particular coherent traditions of scientific research (p.10).*

Kuhn's central argument in the above passage is that from paradigms "spring particular coherent traditions of scientific research,"wherein Kuhnian paradigmatic science assumes a platonic stance.

In *The Structure of Scientific Revolutions* (1962), Kuhn stipulates how science progresses from what he calls "normal science" to "scientific revolution" and then back to normal science. Kuhn's argument is that a prevailing paradigm may sometimes fail to solve problems that may face a scientific community. Kuhn argues that repeated failures of the paradigm to solve a problem or other anomalies lead scientists to search for paradigm which can account for the anomalies. The new paradigm accounts for the earlier paradigm and the anomalies that faced the earlier paradigm. The assumption here is that the new paradigm is more perfect that the earlier one. The presupposition underlying the choice of one paradigm and not the other is that the "chosen paradigm" is more "certain" to solve the problems or anomalies that led to the abandonment of the earlier paradigm. In other words, the new paradigm is more ideal than its predecessor(s). This Platonism is a metaphysical position, which cannot be defended in science and its philosophy, hence the failure of the said search for certainty latent in Kuhn's paradigmatic account of science. Despite the fact that Kuhn's paradigmatic account of science falters due to the Platonism, which the idea of paradigm entails, Kuhnian science explicitly presents the role played by the scientific community in the activities labeled under "science".

6.1 Findings of the study

The study pointed out two conclusions on the basis of Kuhnian paradigmatic account of science in relation to the scientific method. The two conclusions pointed out are:

1). Kuhnian paradigmatic account of science does not meet the *conditio sine qua non* demanded by the scientific method, that is, a). Kuhn's account of science does not capture the classical model of scientific rationality but the wider conception of rationality entailed in scientific judgment; and,

b). It does not reflect the requirements of "scientific progress" since it is not cumulative.

2). Although Kuhnian paradigmatic account of science does not meet the above conditions; it is nevertheless scientific, at least minimally, since it captures the social practice of science.

6.2 Recommendations of the study

The study makes the following recommendations on the "Kuhnian paradigm", scientific progress, and the nature of scientific inquiry.

1). Recommendation on "Kuhnian Paradigm"

The recommendation on the Kuhnian paradigm is in relation to the exclusive use by Kuhn, of the Platonic Forms in his account of science, even with their limitations. The Platonic Forms presuppose that there must be a substance to which every concept should correspond. On the other hand, that there should be substances to which concepts correspond is a mistaken notion (Dewey, 1968, p.765).

It should be realized that we have concepts without corresponding images, for example, "God" and "Justice", among others. These are abstract concepts to which there are no corresponding images although we can distinguish cases of the application of these words from cases of their non-application. The "Kuhnian paradigm" is a case of an abstract concept without any corresponding image and it should not be considered unscientific on that basis.

Kuhn's "irrationalistic" science, while it may seem to be suggested by a half century of a deep study of discarded theories, is a logical outgrowth of an abstract concept which is scientifically untenable still, his use of the concept is not conceptually clear. Almost all commentators agree that Kuhn's use of this concept is extremely loose and variable. For instance, Margaret Masterman (1970, pp. 59-89), finds twenty-two meanings of "paradigm" in the SSR (1962). Consequently, the author recommends that if knowledge is to advance, then conceptual clarity must be acknowledged.

2). Recommendation on scientific Progress

For Kuhn the final constraint upon scientific choice is a social rather than a logical one: the final arbiter is the professional judgement of the scientific group. If a scientific community can be persuaded of the necessity of relinquishing their commitment to one fundamental standpoint, or paradigm, in favour of another, then this in itself is sufficient to provide a "virtual guarantee" that change will be progressive.

I this study it is being recommended that, as much as it is recognized that the social element is one element in rational decision-making, the demands of the "rules" must be met before the social element comes into play. When there are rules and principles to follow, we know what we are doing, and their absence we have no coherent basis for making important decisions. It is further recommended that science should be viewed as an accumulative process rather than cases of revolutionary episodes. Contrary to Kuhn's claim that science progresses by revolutions, the study recommends that scientific progress is cumulative since the history of science is a witness to the foregoing claim.

3). Recommendation on the nature of scientific inquiry

In order to understand explanations in philosophy, it is inadequate to simply label them, for example, as "scientifically progressive" or "scientifically rational". An examination must first be carried out thoroughly to find out not only what science is but also what science does. Such an examination should not ignore the practice currently existing, under the label. The history of science is a witness as to how science is practiced and how it progresses. While we must acknowledge the positive contribution to the philosophy of science on the part of Kuhnian science, the functional completeness and comprehensiveness of scientific inquiry expressed in the scientific method cannot be over-estimated.

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