FALSIFIABILITY AND CORROBORATION IN KARL POPPER'S PHILOSOPHY OF SCIENCE

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CERTIFICATION

THIS IS TO CERTIFY THAT THE THESIS -

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IS A RECORD OF ORIGINAL RESEARCH CARRIED OUT BY

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DEDICATION

To the evergreen memory

of my father

who slept in the Lord

on Thursday,

31st August, 1989

And

All lovers of Wisdom

And

Progress

DECLARATION

I declare that this Thesis represents my original work in the Department of Philosophy University of Lagos.

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ABSTRACT

There are a number of crucial issues involved in the study of the philosophy of science. These include the problem of methodology of science, the factors that could enhance the advancement of science, the question of truth and certainty in science as well as the issue of rationality of scientific discoveries. A number of idealogies have emerged in response to the challenges posed by the above issues.

The relativists, for instance, see scientific truth in terms of a period of reference, environment and orientation. To the realist, a statement in science is either true or false - there is no mid-way between the two. The instrumentalists are rather interested in the function that theories play in science. They see theories as necessary instruments in science essentially meant to make prediction.

The logical positivists are interested both in the methodology of science as well as the relationship between science and metaphysics. On methodology, they advocate induction as the procedure in scientific research. They, in agreement with this methodology, use their principle of verifiability and confirmability to dismiss the propositions of metaphysics as nonsensical, and as having no bearing in the acquisition of knowledge.

Popper came to the scene amidst the above unsettling problems to propose a methodology that was both intellectually tasking as well as controversial. His thesis of 'falsification and corroboration' (which is incidentally the focus of this research) is no doubt very controversial. He posits that at any point in time that a scientist is at work, he is either falsifying or corroborating a theory. And all theories or laws in science, he says, are forever conjectures, as none of them is immune from refutation in the immediate future.

He did not agree with the Positivists that metaphysical propositions are nonsensical and meaningless. But he emphasised the need to demarcate the two, since each of them has a different method of approach. He thus uses falsification as a criterion of demarcation between science and metaphysics. Any theory that is falsifiable belongs to the family of empirical science while the non-falsifiable ones are metaphysical. Popper also rejected the principle of induction as part of the methodology of science. This principle, he argues, can lead to infinite regress.

Popper's thesis, as earlier on remarked, is very controversial and in fact, unacceptable to us. In the course of this research, we disagree entirely with his thesis of falsification. We want to state too, contrary to his position, that induction is a methodology of scientific investigation. Even his falsifiability and corroboration thesis is arrived at inductively. We want to state too, that scientific knowledge is cumulative - it is this that makes for progress in scientific endeavour. Besides, Popper's position that theories remain conjectures forever is not acceptable to us. Critical attitude, as advocated by him, can quite bring progress to science; but our own conception of the dynamism of science is not in the context of constant changes of scientific theories, as he wants us to believe.

Thus, the purpose of this study is to have a critical evaluation of Popper's thesis of 'falsification and corroboration' with a view to establishing its rationality or otherwise. We also want to articulate the features that are necessary for the advancement of science as an academic enterprise. We believe too that at the end of the research we shall be able to enunciate an appropriate and relevant science culture for Nigeria.

INTRODUCTION

For a proper appreciation of Popper's work in philosophy of science, it is necessary to have an insight into his early life which will reveal to us the processes of his intellectual development, especially how he came to be interested in the logic of scientific discovery which is the focus of this research.

Popper was born on July 28, 1902, at a place called Himmelhof in the Ober St. Veit district of Vienna. He was brought up in an academic environment as his father, Simon Siegmund Carl Popper, like his two brothers, was a Doctor of Law of the University of Vienna. Beside his legal profession, Popper's father was interested in philosophy and had in his collection of books materials on philosophy, especially those on the works of Plato, Descartes, Spinoza, Kant and Nietzsche. The young Popper, as a result, had an early exposure to books even before he grew up to read them.

At the age of 20, Popper was to learn cabinet works under an old master cabinet maker in Vienna. This man was so much interested in developing his pupil especially intellectually, hence he was in the habit of asking him questions on various issues to which Popper responded to the best of his ability. To Popper, this constituted his first exposure to the theory of knowledge. Through this 'lecture', Popper said he became aware for the first time of the extent of his ignorance, and that it is only the realisation of its infinite nature that can make him attain whatever knowledge he aspires to. This fact, among others, occupied Popper's thought on his writing desk. It did not take Popper and his master much time to discover that he (Popper) was unfit in the kind of work he was learning. Popper, as a result, had to abandon the work at the completion of his apprenticeship in October, 1924.

Popper was convinced, even at the early age of his life, that there are philosophical problems. And these are genuine problems that cannot be seen as mere puzzles arising out of the misuse of language. Some of these problems, stated Popper, are childishly obvious. As a child, Popper said he stumbled into these kind of problems: such as that of the solar system and the infinity of space. The other problem he said he encountered was on the origin of life which has not yet been solved in spite of Darwin's theory. These kind of problems, Popper stated, confront both the old and the young. And in spite of the fact that experimental work could be carried on them, they remain essentially philosophical problems.

Popper did not attach much importance to words and their meanings. He strongly believe in the principle of avoiding arguments on words and their meanings as such are both specious and insignificant. Rather than laying emphasis on words and their meanings, he advised that we should be pre-occupied with the issue of facts, assertion about facts, theories and hypotheses, the problems they solve and problems they raise. To quarrel about words and their meanings, Popper believes, is the surest "path to intellectual perdition: the abandonement of real problems for the sake of verbal problems."[1]

Popper believes that science abhors dogmatism. Its progress is ensured only by criticism. As Bartley III pointed out, Popper posited that

> philosophers ... should not demand and search for infallible intellectual authorities, but should instead try to build a philosophical program for counteracting intellectual error. 2

1) Karl Popper "Intellectual Autobiography" in the Philosophy of Karl Popper , Book I, Editor P.A. Schilpp (Illinois, The Library of Living Philosophers, Inc. 1974), p. 12

2) W. W. Bartley III "Rationality Versus the Theory of Rationality" in the Critical Approach to Science and Philosophy in Honor of Karl Popper Edited by Mario Bunge (Glencoe, The Free Press of Glencoe; 1964), p. 21

Thus the concern of philosophers should be on how their intellectual life and institutions can be arranged with the aim of exposing their beliefs, conjectures and ideas to maximum criticism.

Popper averred that his interest in demarcation was initially limited between scientific and pseudo-scientific theories. It was at a later date he stated that this interest (of demarcation) was extended to metaphysics. And the basic idea in this issue centres on his belief that if one proposes a theory, one should state under what condition such a theory will no longer be tenable. And this state of affairs is practicable only when theories in empirical science are involved. And as Wisdom pointed out, Popper believes that "no hypothesis in empirical science can be completely certain beyond the possibility of refutation."[3] It is this possibility of refutation that makes a theory or hypothesis scientific.

Popper admitted he was close to a number of logical positivists such as Rudolf Carnap and Hans Hahn (who was incidentally his teacher); in spite of this he was vehemently opposed to their ideas both on the issue of induction and the criterion of demarcation between science and metaphysics. As Popper puts it, the logical posivists,

> were trying to find a criterion which made metaphysics meaningless, nonsense, sheer gibberish, and any such criterion was bound to lead to trouble since metaphysical ideas are often the forerunners of scientific ones. [4]

Popper's interest on demarcation was not to degrade metaphysical proposition as meaningless or insignificant, but to show that the method of enquiries between science and metaphysics differs.

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3) J. O. Wisdom "Some Overlooked Aspects of Popper's Contributions to Philosophy, Logic and Scientific Method in the Critical Approach to Science and Philosophy. p. 121

4) Popper "Intellectual Autobiography", In the Philosophy of Karl Popper, Book I, p. 63

Among the views that Popper was opposed to was the view that the future is contained in the past or the present, though he concedes that they impose severe restriction on it. The universe, he said, is such that there are objective problems, objective achievements, just as we have knowledge in objective sense. Thus, there should be a clear distinction between feelings from subjective and objective perspectives, solutions and achievements. Subjective theory sees man's work fundamentally as expression of his inner state and sees self-expression as an aim. But through our attempt to see objectively the work we have done - that is to see it critically - and to do it through a comparison between our action and their objective results, we will then be able to transcend our talents and ourselves. As with our children, so with our theories and ultimately with all the works we do: our products become largely independent of their makers. We may gain more knowledge from our children or our theories than we even ever imparted to them. This is how we can lift ourselves out of the morass of our ignorance.

Popper, no doubt, delves into many areas of the theory of knowledge and the methodology of science. The focus of this research, however, is on theories: its "falsification and corroboration". In the course of the research, we shall see the methodology of science that Popper favours and how relevant such is in scientific enquiries.

For a proper presentation of the issues involved in this research, we shall in the first chapter examine some controversies in philosophy of science. In doing this, the views of the realists, the relativists, the instrumentalists and those of the logical positivists will be examined. The essence of this exercise is to determine to what extent the ideas of Popper were influenced by those of his predecessors and contemporaries.

Chapter two introduces Popper's views - his critical as well as his substantive views. The issues to be examined here include his opposition to induction, his thesis of deduction, his view on probability, his demarcation thesis between science and metaphysics and the position of theories in scientific investigation.

In chapter three, we shall look into the issue of falsifiability and corroboration. In the course of doing this, issues such as simplicity, laws and convention, precision and testability are to be evaluated.

In chapter four, we shall examine some criticisms of Popper as seen by Thomas Kuhn, Paul Feyerabend and Imre Lakatos. Beside the critical views of these scholars on Popper's methodology, we shall see the alternative views they proffer.

Chapter five concludes the research. In this chapter, we shall make a final and general critical appraisal of Popper's thesis of falsifiability and corroboration. It is also in this chapter that the position of the researcher will finally emerge. I should perhaps mention that each chapter will end with a critical summary.

A relevant science culture for Nigeria should be the preoccupation in chapter six. In this chapter, we shall evaluate the efforts and the constraint science, as an intellectual and developmental-induced culture, has faced in the Nigerian context. We shall, at the end, recommend measures that could be taken for the realisation of befitting science culture for the country.

LITERATURE REVIEW

There are several books on Popper's philosophy of science, written by him and other scholars in the discipline. There are also several books and journal articles on the same subject matter. Books by Popper will remain our primary source in the research assisted by our secondary source, which covers a whole range of books, articles in learned journals and books. The details will be reflected in the bibliography.

In the literature review, we selected books and articles in reputable journals and books that are relevant to the research. It is to be remarked that no one source of the material used for the research treated in details the thesis of this research, hence the need to consult as many books and articles as possible. As a matter of fact, this research remains very exhaustive and critical as far as falsifiability and corroboration are concerned in Popper's philosophy of science. By this study, we shall see the rationale as well as the loopholes in Popper's thesis in philosophy of science. And more importantly, the methodology of scientific discoveries as well as the factors that could enhance the growth of science will be laid bare.

Reichenbach's The Rise of Scientific Philosophy (Los Angeles University of California Press, 1951) is a book essentially on the general characterisation of philosophy of science. It examines such issues as methodology of science, observation, prediction, induction and deduction. In the first part of the work, the book is useful at least in the exposition of some common features of science; but does not give adequate information on Popper's thesis of falsification.

Bertrand Russell's book, The Problem of Philosophy (New York, Oxford University Press 1980), bothers with the general interest problem of philosophy. He, however, delves into the recurring issues in the philosophy of science such as induction, generalisation, uniformities, probability and experience. Thus, the book recommends itself to the research only as far as the appreciation of common issues in science are involved.

Newton-Smith's The Rationality of Science (London, Routledge and Kegan Paul, 1981) could be said to have made a bold attempt in the examination of fundamental issues in the philosophy of science generally. Some of these issues are observation, induction, deduction, reasoning and criticism. The author went further to evaluate the position of Popper on the issue of methodology in science which to him is not rational enough. Newton-Smith, it must be pointed out, did a very scanty exposition of Popper's thesis on philosophy of science, which is a major weakness of the book as far as this research is concerned.

Thomas Kuhn's book, The Structure of Scientific Revolution (Chicago: the University of Chicago Press, 1962), represents a major work on the methodology of science. Apart from stating his advocacy of what he called normal science, he branched out to criticise Popper's falsification theory. But Kuhn did not carry out a detailed critical analysis of Popper's thesis, except that his theory of falsification is naive. Thus, the book serves more in the understanding of Kuhn's philosophy of science than anything else.

Paul Feyerabend's "Against Method" (Great Britain, The Thetford Press Ltd., 1984) was essentially an attack on philosophers of science who pre-occupied themselves with recommending methodologies for science. He was very vehement on attacking Popper's falsification theory which he believed is a distortion of procedure in science. He equally attacked Popper's rejection of adhoc hypothesis and inconsistencies in the structure of scientific theories. Feyerabend rejected induction and any other method for that matter in science but failed to carry out a convincing logical analysis of how science could do without method.

David Stove's book, Popper and After: Four Modern Irrationalists (London, Pergamon Press Limited, 1982), was essentially an attack on the position of Popper, Feyerabend, Kuhn and Lakatos as far as the methodology of science is concerned. He called the four 'the Irrationalists'. He exposed the illogicality of Popper's position that all theories remain forever conjectures. He disagreed with the 'rrationalists' too that scientific knowledge is not cumulative. The book served a good purpose in the critical evaluation of Popper's work. But Stove did a very poor job on the exposition of Popper's thesis.

Mario Bunge's Scientific Research II, the Search for Truth, (Berlin, Springer-Verlag, 1967) treated many cardinal issues in philosophy of science including explanation, prediction, observation, measurement and experiment. All such issues are relevant in our general appreciation of philosophy of science. But we cannot rely on it for a critical study and application of Popper's philosophy of science.

Arthur Pap, in his book, An Introduction to the Philosophy of Science (New York, the Free Press of Glencoe, 1962), did a thorough exposition of the cardinal concepts in the philosophy of science. We see such concepts as verifiability, confirmability, induction and deduction properly examined. A proper understanding of these concepts is valuable for a thorough critical analysis of Popper's thesis of deduction and falsification.

Francis Miller's Chemistry: Structure and Dynamic, (New York, McGraw-Hill, Inc., 1984) is a very useful material that introduced us to the research. Through the bock we are able to note the relationship as well as the differences between chemistry and other sciences such as physics and biology. From the book, we see a link between the ancient and the modern in the evolution of modern sciences such as chemistry. For instance, the phlogiston theory of the old championed by Stahl and others and the oxygen discovered by Lavoisier. The book is an asset in assessing the position of Popper on the issue of progress in science.

J. W. N. Watkins Paper's "Metaphysics and the advancement of Science" in the British Journal for the Philosophy of Science, Vol. 26, 1975, focuses on Popper's thesis of falsification and corroboration. He restated Popper's position on a falsification of scientific statements. The article also touches on Popper's criterion for the corroboration of hypothesis, but Watkins failed to examine thoroughly Popper's deductive thesis which is the alternative he proposed to induction.

Parel Tichy's article "On Popper's definition of Verisimilitude" in the British Journal for the Philosophy of Science, Vol. 25, 1974 centres on Popper's epistemology, which he characterised as optimistic scepticism. It is a scepticism because of the much doubt it entertains on the validity of theories, and optimistic because of its admission that science nevertheless, makes progress; that there are ways of always improving on our false theories. While one may agree with Tichy on the fact that Popper has not absolute faith in a theory, one does not see a proper interpretation of Popper's thesis in his assertion that we have ways of improving our false theories. It is to be noted that Popper asked for absolute rejection of theories that are false, not an improvement.

J. O. Wisdom's "The Refutability of Irrefutable Laws", in the British Journal for the Philosophy of Science, Vol. 6, 1955 focuses on Popper's thesis of refutability which is a criterion for knowing an assertion that belongs to the family of empirical science. Wisdom disagreed with Popper that refutation is a qualificiation for laws that are scientific. He believes that in Physics, for instance, that the law of conservation of energy cannot be falsified and yet is still seen as a scientific law. The article is an asset for a critique of Popper's work; but its concentration is on falsification, leaving other aspects of Popper's thesis such as deduction, demarcation, and probability which are equally crucial in this research.

P. K. Feyerabend's article "A Note on Two Problems of Induction" in the British Journal for the Philosophy of Science, Vol. 19, 1969 examined the issue of induction and said it is unworthy as a method of science. The association of events over a period of time, he stated, remains the principles governing the laws of induction. This method, to him, is a poor and unrewarding approach in the study of science. He was in agreement with Popper that progress in science can be achieved by critical evaluation of old theories. He, nonetheless, rejects all methods of science, including Popper's falsification thesis. The article is an asset in understanding Feyerabend's position on philosophy of science, but weak as a criticism of Popper as there is no rigour in his rejection of Popper's falsification theory.

E. G. Zahar's "The Popper Lakatos controversy in the light of Die-Beiden Grund probleme Der Erkenntnis theorie" in the British Journal for the Philosophy of Science, Vol. 34, No. 2 1983 focuses on Popper's opposition to the positivists and instrumentalists. He highlighted Popper's disgust of the stance of the positivists that scientific theories are nothing but summaries of past experimental results. Popper also, in this article by Zahar, was shown to disapprove the position of the instrumentalists that hypotheses in science are nothing more than 'computational devices' essentially meant to predict facts. Zahar, in the same article, stresses Lakatos' opposition to

Popper's falsification theory which he (Lakatos) believes is rendered impotent by the existence of 'ceteris paribus' clause in scientific hypotheses which, in fact, make them irrefutable. The article is an asset, especially in knowing Popper's stance as far as the positivists' and the instrumentalists' views on hypotheses in science are concerned.

J. H. Harries' "Strong Scientific Theories" in the Philosophy of Science, Official Journal of the Philosophy of Science Association, Vol. 43, No. 3, 1978 pre-occupied with the issues of theories, their formulation, status and refutation as seen, especially by Popper. To this extent, the article is an asset to the research.

D. B. Madan's "Inconsistent theories as scientific Objectives" in the Philosophy of Science Official Journal of the Philosophy of Science Association, Vol. 50, No. 3, 1983 was interested in the things that make theories objective and the essence of consistency in scientific theories. He stresses the point that scientific method is basically opposed to inconsistent theories. The article is relevant only in the fact that it buttressed Popper's position on the rejection of inconsistency in theory formulation.

Michael Ruse's "Karl Popper's Philosophy of Biology" in the Philosophy of Science, Official Journal of the Philosophy of Science Association, Vol. 44, No. 4, 1977 looks into Popper's thesis especially as it affects the evolutionary laws of human beings as well as human destiny. Popper, as shown by Ruse, denied both the laws of evolution as well as the possibility of human destiny. To Popper, laws require repeatability and the laws of evolution cannot meet up with these requirements. The account of the evolutionary theory, insists Popper, does not meet up with the features of scientific theories. The article is useful at least for a proper appreciation of the qualities that scientific laws should possess in the light of Popper's thesis.

Asike J. I.'s "Scientific Facts and the theoretical Framework" in Uche, Vol. 8, 1984 focuses on theories and the role they play in the development of science. Asike stated that it is through theories that our everyday observations are interpreted. He accepted Popper's position that our view of the world is theory impregnated. Criticism, he stated, which is in agreement with Popper, is a vital factor in the progress of science. The article, though stating Popper's position on theories and criticisms, failed to stress Popper's falsification and corroboration theses which are basic in the understanding of his concept of theories.

H. Lary Putnam's "The Corroboration of Theories" in the Philosophy of Karl Popper, Book I is another piece on Popper's conception of theories and its corroboration. It looks into Popper's conception of induction, general laws as well as the role of observation in science. It is a good piece on Popper's perception of the status of theories. But the article did not examine thoroughly the features a good theory should abhor or possess.

The impression that one can have from the above review is that there have been efforts to make Popper's idea available to all. There is no single work on Popper, however, that has brought into proper perspective the implication of his falsification and corroboration thesis, his rejection of induction and his recommendation of criticism as the guarantor of progress in science. Nonetheless, all the efforts so far made at evaluating Popper's theses of falsification and corroboration can be seen as genuine philosophical endeavours that can lead to the expansion of knowledge. It is to be remarked, once more, that no one source is adequate for the purpose of this research which informed the need to make reference to varied sources.

Our interest in this research is essentially to see the rationale of Popper's theses of falsification and corroboration; to show that induction rather than falsification is a method of science. And also that Popper was not consistent in his rejection of induction as 'falsification and corroboration' can only proceed by inductive method. There is no work on Popper so far that has sufficiently highlighted this point as we shall do in this research.

CHAPTER ONE

SOME CONTROVERSIES IN THE PHILOSOPHY OF SCIENCE

Introduction:

In this Chapter, we shall examine some of the issues in the sciences as regards their methodology which directly or indirectly contributed to the formation of Popper's ideas. In so doing, we shall examine the theses of the realists, the instrumentalists, the relativists and finally, the logical positivists. The theses of the first three schools of thought were not so much developed, hence the limitation in their discussion in scientific literature.

Unlike the first three schools of thought, logical positivism as a school of thought was quite famous and controversial. In the mid 20th Century it was a dominant philosophy, especially in the areas of science and epistemology. Popper's critical views were actually shaped by the theses of the positivists, and as noted in the introduction some set of scholars took him to be a positivist until the English Edition of his book '<u>Logik de forschung</u>' (<u>The logic of Scientific Discovery</u>) appeared.

Our discussion will start with realism, relativism and instrumentalism to be followed by logical positivism.

1. REALISM AND RELATIVISM

Realism as a terminology represents a number of positions in the Philosophy of Science. The central thesis of the term is that "Scientific propositions are true or false where truth is understood in terms of cleaned-up version of the correspondence theory of truth."[1] To the realist, a statement in science is either true or false. There is no mid-way between the two.

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1. Newton Smith <u>The Rationality of Science</u> (London Routledge and Kegan Paul, 1981), p. 40

The Realists see science as a dynamic enterprise. A theory in science is either taken to be true or false on the basis of the data and evidence collected on it. Their tradition in the Philosophy of Science is forward-looking, and they advocate those things they believe are essential for the development of science.

In a situation where we are faced with a pair of incompatible theories, where we cannot decide on the basis of available data, the realists believe that all we can do in such a situation is to seek further information on the two with the hope that it will tilt the balance in favour of one. "Until such evidence becomes available the appropriate attitude.... is one of agnosticism."[2] The realists believe strongly on the power of observation and experience in scientific research. It is only when these are thoroughly made use of that one should see oneself as carrying out a scientific investigation. An investigation that makes use of neither observation nor experience is speculative and should as such be seen as pseudo-science. Truth or falsehood to the Realists is not relative; there is no mid-way between the two. Their position differs from Relativism as we shall presently see.

2. RELATIVISM

The Relativists have a different picture of what truth or falsity in science should be. They perceive truth in terms of environment, in terms of period, and in terms of one's orientation.

2. Ibid. P. 40.

In their treatment of truth or falsity there is no objective criterion. To them,

> ... What is true depends in part or entirely on something like the social perspective of the agent who entertains the hypothesis or on the theory of the agent. On this picture, as one passes from age to age, or from society to society, or from theory to theory what is true changes.. 3

The relativists see nothing permanent about truth, as environment or time can change what was hitherto true to falsity and vice versa.

The thesis of the relativists resemble so much the view of Protagoras who posits that man is the measure of all things, "of those which exist that they exist, and of those which exist not that they exist not"[4]. Protagoras gave rise to the thesis of the sophists that everyone should measure matters according to his nature and needs, given the fact that man is the measure of all things. They (the Sophists), almost in agreement with the view of the relativists, assert that man should accept the fact that all his alleged knowledge was only relative to his outlook. Also the consequence of Protagoras' reduction of knowledge to perceiving "is that knowledge is not only relative but is relative to each perceiver at the moment of the conjunction of the motions involved."[5] Man decides what is and what is not. Protagoras believes,

> that matter is in flux and as it flows additions are made continuously in the place of the effluxions, and the senses are transformed and altered according to the times of life and to all the other conditions of the bodies. 6

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3. Ibid, p. 35
4. Milton C. Nahm, Selections from Early Greek Philosophy (New York, Appleton-Century-Crofts 1964) p. 225.
5. Ibid. p. 221
6. Ibid. p. 225.

Similarly, men see different things at different times as a result of their differing dispositions. These views of Protagoras are almost in agreement with the thesis of the relativists. While the relativists were borthered with the nature of theories, instrumentalists see theories in terms of weapons for making prediction. The detail is given below.

3. INSTRUMENTALISM

Instrumentalism is a more developed perspective compared with realism and relativism. Some of the known instrumentalists are Mary Hesse and Duhem.

The instrumentalists believe that theories are necessary weapons in the sciences. They do not see it appropriate to evaluate theories with the categories of truth or falsehood. Hesse, for instance, argues that theories possess the status of instruments, tools or calculating devices in issues relating to observation. The assumption inherent in this view is that theories are made use of to relate and systematize observation statements. Predictions could be made in the process; but the issue of truth does not arise. It is meaningless to talk about truth in this matter. Prediction to the instrumentalists exhaust the goal of science. To them, as Newton-Smith remarks, "theoretical sentences are incapable of being true or false."[6] The reason for this, according to him, is that theories lack meaning and truth-value.

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6. Newton-Smith, p. 33

The thesis of the instrumentalist was influenced by a strong form of verificationism. They could not successfully verify the truth of theoretical sentences, which is an evidence that these sentences can neither be true nor false. To them,

> Scientific laws and theories are not proper descriptive statements; they are nothing but instruments to derive observational predictions from other observational statements. 7

The method of science, they posit, cannot yield anything more than tentative, revisable hypothesis. They insist that experimental findings cannot also contradict scientific laws, just as crucial experiment aimed at refuting theories is impossible. Poincare, one of the instrumentalists, sees scientific theories only as terminological conventions,

> useful for systematizing observational data in the way a catalogue systematizes books in a library, but having no descriptive, empirical content. 8

As far as theories are concerned, the instrumentalists do not see them as articles of faith.

Osiander, another instrumentalist, in the fashion of their thesis, believes that progress in science, even in astronomy can be achieved only by tolerance. There should be a kind of situation where everyone should feel free to propose an alternative to the existing theories through

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7. Jerzy Gierdymin "Instrumentalism and its critique. A Reappraisal" in the Boston Studies in the Philosophy of Science, Vol. XXXIX, Essays in Memory of Imre Lakatos, Edited by R. S. Cohen Et al. (Boston, D. Reidex Publishing Company, 1976) p. 182

8. Ibid. p. 183

greater simplicity avoiding friction in all its ramifications. Old theories should however be replaced by "better ones without any of the theories being either conclusively refuted or demonstrated..."[9] This makes for progress in science.

The instrumentalists do not see physical laws as even true. The laws of physics they see as neither true nor false but an approximations. Physical laws, they say, are always undetermined on the strength of the evidence we have. To this extent it is only wise to say that they are indeterminate. They (physical laws) are unceasingly undergoing modification and improvements. There are some factors that were not adequately considered previously. Those factors will, from time to time, be receiving attention, in addition to the new ones emerging.

Instrumentalists maintair that common sense laws are simple generalisations of everyday experience. It is therefore easy to determine if they are true or not. There is no difference between its language and our everyday experience. We can thus determine its truth or falsity. In contrast, physical laws are formulated in a highly idealized and precise language of mathematics. Predictions made from them assume the same idealized mathematical form. We cannot as a result append "true" or "false" to such laws.

Another issue involved in physical laws is that there is always the measurement of instruments. And there is a limit to our precision in our measuring procedures, and as such the outcome is always imprecise. For this reason Duhem states that "physical theories are merely means of classifying and bringing together approximate laws."[10]

9. Ibid. P. 188

10. Ibid. P. 199.

The objects that theories in physics deal with are too complex and imprecise to be correctly determined. An instrumentalist, says Feyerabend, may even see that

> the functions of words like 'gravitation', 'force' and 'gravitational field' are exhausted by their giving an abbreviated description of the spatiotemporal behaviour of physical objects. 11

It is even possible for an instrumentalist to deny the existence of these objects and simply see object words merely as instruments meant for the ordering and predicting of sense data.

While realism, relativism and instrumentalism were concerned with the nature of truth in science and the use that theories could be made of, logical positivism, in addition to enuciating methodologies such as verification and confirmation, was also interested in ridding science of metaphysical influences. Their thesis is nonetheless more developed than the ones already treated as we shall presently see.

4. LOGICAL POSITIVISM

Logical positivism is the name given in 1931 to a set of philosophical ideas formulated by a set of scientists and scientifically minded individuals. The positivists were then known as the Vienna Circle. Members of the circle believed that they were championing the empirical tradition which has common features with British empiricism.

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11. Paul Feyerabend "Realism and Instrumentalism Comments on the logic of factual support" in the Critical Approach to Science and Philosophy in Honour of Karl Popper. Edited by MARIO BUNGE (London, The Free Press of Glencoe, 1964), p. 280

This later culminated in an antimetaphysical stance. Their central interest was on science and scientific method. In the circle were such big names as Hans Hahn (a mathematician), Otto Neurath (an economist) and Philip Frank (a physicist). There were also Moritz Schlick, Friedrich Waismann, Victor Kraft, among others.

The emergence of logical positivism as a school of thought was a land mark in the development of the philosophy of science with emphasis on methodology. The positivists see their mission as a rescue operation, saving science from all traces of metaphysics. They thus made desperate efforts to 'free' science of all metaphysical elements. Whether they succeeded in doing this is another issue altogether.

In our discussion of the thesis of positivism, we shall look at their positions on such issues as the status of metaphysics, verification principles and confirmation: the predominant concern of their methodology was to discredit metaphysics as a source of knowledge.

4(i) ATTACK ON METAPHYSICS

The logical positivists relegated metaphysics to the background. They followed the footsteps of David Hume who dismissed metaphysics as an illusion that should be committed to the flames. He (Hume) noted that three basic issues are involved in the acquisition of knowledge.

> These are particular precepts, empirical generalizations and the mathematical constructs. Particular precepts and mathematical constructs ... are certain even though the former only assure us of particular existing realities here and now. Mathematical constructs ... are quite certain and evident though they do not depend on reality existing anywhere in the world. 12

On this basis, Hume divided significant propositions into two classes. The first of this was formal propositions such as those of logic or pure mathematics which are held to be tautological; and the second was factual propositions which are expected to be empirically verifiable. The logical positivists adopted this division. They (Logical Positivists) were also influenced by Russell's classifications of language expressions into (a) true statements, (b) false statements and (c) meaningless expressions. Bertrand Russell contends that,

> A mind, which believes, believes truly when there is a corresponding complex not involving the mind but only its objects. This correspondence ensures truth and its absence entails falsehood. 13

The positivists aver, obviously under the influence of the above thesis, that if a proposition did not express something formally true or false, or such a thing that can be empirically tested or verified, that such a proposition is not worth its name. Such propositions belong to the arena of metaphysics. As Rudolf Carnap puts it,

> the positive result is worked out in the domain of empirical science; the various concepts of the various branches of science are clarified; their formal - logical and epistemological connections are made explicit. In the domain of metaphysics, including all philosophy of value and normative theory, logical analysis yields the negative result

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12. K. C. Anyanwu "Logical Positivism: Language and Meaning" in The Nigerian Journal of Philosophy, Journal of the Department of Philosophy, University of Lagos Vol. I, No. 1, 1981, p. 21
13. Bertrend Russell, The Problems of Philosophy (New York, Oxford University Press, 1980), p. 75

> that the alleged statements in this domain are entirely meaningless. 14

Carnap believes that the elimination of metaphysics from meaningful branch of knowledge was made possible with the development of logic and logical reasoning. He stated it clearly that the stance of the positivists on metaphysical propositions as meaningless was strict in every sense of the word. A statement can be logically false or contradictory, yet could be said to be meaningful. In a strict sense, however, a sequence of words is meaningless if it does not in every sense of language classification constitute a statement. From his analysis Carnap concludes that the alleged statements of metaphysics are in actual fact pseudo-statements

The Positivists believe that for philosophy to become a respectable discipline like science, it must have to get rid of meaningless and non-sensical statements. And the only positive way to do this is to purge itself of metaphysics. They (positivists)

> thought that metaphysics had some values as an attitude toward life though its propositions did not increase human knowledge. What the positivists found objectionable was the pretention of metaphysics to perform the function it could not possibly do, namely, to offer cognitive knowledge of reality. 15

The positivists identify knowledge with science and mathematics to the exclusion of other areas such as ethics and metaphysics.

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14. Rudolf Carnap "The Elimination of metaphysics Through Logical Analysis of language" in <u>Logical Positivism,</u> Edited by A.J. Ayer (Glencoe, The free Press of Glencoe, 1963) p. 60-61.

15. Anyanwu p. 21.

Thus,

> a sentence makes a cognitively meaningful assertion and thus can be said to be either true or false only if it is either (1) analytic or self-contradictory or (2) capable at least in principle of experiential test. 16

According to this criterion of cognitive meaning most of the formulations of traditional metaphysics and large parts of Epistemology are bereft of cognitive significance, no matter how veritable they may be in non-cognitive import, in view of their emotive appeal or the inspiration they offer morally.

Epistemological terminologies such as idealism or realism are equally meaningless to them. Such a concept to them is unclear. They have a similar attitude towards Ethics. They shunned and despised any thought of transcendental Ethics that sought to institutionalize a mode of value beyond the world of experience. Any proposition that had to do with values they dismissed as belonging to the province of the transcendental which in all its forms has no relevance to human practical life. Carnap and Ayer were particularly hostile to the propositions or assertions of Ethics. Such assertions are no statements at all. We are even not making an empirical statement when we say, for instance, that stealing is bad. Such an assertion, they believe, is either the expression of our feelings about stealing - our rejection of the

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16. Carl G. Hempel "The Empiricist criterion of meaning" in <u>Logical Positivism</u>, p. 108.

act - or in the alternative, an attempt to discourage others from doing the same. Thus we are not giving any scientific or empirical information when we say that stealing is wrong. This kind of assertion cannot be verified empirically. Thus the positivist verification principle is a direct attack on metaphysical knowledge.

4. (ii) VERIFICATION PRINCIPLE

To appreciate the strong attack of logical positivism on traditional philosophical system, including logical atomism, it is imperative for us to understand what they meant by 'analytic' and 'synthetic' propositions, and also the criterion they used to determine when a proposition is cognitively meaningful.

The propositions that require some kind of empitical investigation before their confirmation are termed 'synthetic' whereas those whose truths follow from their meaning are called analytic. It is thus the stance of the positivists that any proposition that is significant should either by analytic or synthetic. It cannot be both. All the propositions that are analytic belong to formal logic, and as a result true by virtue of their formal structure. Synthetic propositions, on the other hand, are like the propositions of science. They need to be investigated empirically before their truth could be established. An analytic proposition derives its name from the fact that its predicate is contained in the definition of the subject term, the whole essence of the proposition being to assert something of the subject that is obtained by analysis of the subject term. As a result, we are able to verify such propositions by the examination of the words they contain.

Synthetic propositions can only be investigated by observation and empirical investigation, because they are derived by the joining together of two logically unrelated things. By our investigation we can establish whether this relation is true or not. The Positivists believe, however, that analytic propositions are trivial, while synthetic propositions are informative. The analytic propositions which may appear to be making references to items in the world, turn out upon analysis to be making real claims about the world. Thus they are true only by virtue of their logical form or by definition or simply by saying that they are assertions about words. In contrast synthetic propositions are informative. They make claims about reality. Thus, according to the logical Positivists, for a statement to be cognitively meaningful it must be either analytic or empirically verifiable. With the help of mathematics and logical techniques it will be easy to say that a proposition is analytic.

> By leaving aside sentences expressing analytic statements-for a sentence to have 'cognitive' meaning it was held that it must express a statement that could at least in principle be shown to be true, or false, or to some degree probable, by reference to empirical observation.[17]

As to when to say that a proposition is analytic, the problems has been taken care of with the help of mathematics and logical

17. Ibid. p. 240.

techniques. But as to when to say that a synthetic proposition is significant, that is when it purports to make assertions about the world, it has to pass a test of being empirically verified before being admitted as significant.

Schlick believes that philosophy found itself in a chaotic state,

> due to the unfortunate fact that in the first place, it took certain formulations to be real questions before carefully ascertaining whether they really made any sense, and, in the second place, it believed that the answers to the questions could be found by the aid of special philosophical methods, different from those of the special sciences. 18

He believes that we cannot by philosophical analysis decide whether anything is real. This can be decided, he argues further, only by the usual methods of daily life and of science, which is experience. The meaning of a question is clear when only we are able to state the exact conditions under which it is to be answered in the affirmative, or as it may turn out under which it is to be answered in the negative. He believes that it is quite impossible to give the meaning of any statement unless we describe the facts which just have to exist, given that the statement is true. Where this fact is absent then the statement is false. Thus what determines the truth or falsity of empirical propositions

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18. Moritz Schlick "Positism and realism", in the <u>Logical Positivism</u>, p. 86.

according to Ayer "... is their agreement or disagreement with reality".[19] Schlick strongly believes that the meaning of a proposition consists in this alone - that is when it expresses a definite state of affairs. And there has to be an emphasis on this state of affairs so as to make our questions or assertions [illegible] clear. Definitions help us further to understand a proposition and the meanings of the words that occur in it. In a definition we state definite conditions under which a proposition is determined to be true or false. When certain data stated in a definition are found in a proposition then it is true, but where absent it is false. All these verifications of truth or falsehood of a proposition can only be possible by observation and experience which are basically features of empirical science.

The Positivists made a distinction between propositions which are verified and those which are verifiable; that is between 'practical verifiability' and 'verifiability in principle' If it is said, for instance, that 'there are men on the Planet Mars' the proposition has not been verified by anyone but it is verifiable. We are in a position to describe steps by which we can verify it, and ascertain if there are men there. If we are able to confirm that there are men there, then our proposition is true but where otherwise it is false. Yet the proposition is significant, since we have described the conditions under which its truth or falsity could be known. Though

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19. A. J. Ayer "Verification and Experience" in the Logical Positivism, P. 228.

we cannot at present verify the proposition for our lack of means of travel to Mars, the assertion is verifiable in principle and as such significant. If we, on the other hand, say that God exists in a heavenly place' there is no relevant observation we could make which would prove the falsity or the truth of the proposition. Since there is no conceivable one to verify this assertion even in principle, the sentence expresses no proposition, and as such it is not a cognitively significant statement.

On the basis of this, the positivists stated that the propositions of traditional phylosophy are not only false but non-sensical. They believe that the principle of verifiability negates metaphysics and whatever it stands for and that only scientific method can actually guarantee the acquisition of knowledge on and about the world. Experience here is inevitable as "... the question of truth or falsity can be decided only by experience, confrontation with observation - sentences."[20] When there is an agreement between our observation and prediction then we know that our proposition is right, but where there is a clash we know that it is false.

By this principle of verifiability the logical positivists were able to make a distinction between science and metaphysics,

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20. Victor Kraft, The Vienna Circle, The Origin of Neo-Positivism, A Chapter in the History of Recent Philosophy (New York, Greenwood Press Publishers, 1969), p. 121

a statement is meaningful and scientific if it can be verified, meaningless and metaphysical if it cannot be verified. Verification is not the only methodology posited by the positivists. Carnap was particularly interested in testing as well as confirming propositions, hence the emergence of confirmability as one of their theses.

4(iii) CONFIRMABILITY IN SCIENCE

Carnap made a distinction between the testing of a sentence and its confirmation. According to him we can test a sentence only if we are aware of a given procedure by which we could confirm or negate it. The confirmation thesis of the logical positivists agree with their thesis,

> that the decision about the truth or falsity of a statement can follow only the testing of a statement - a test undertaken to find whether it agrees with the experimental facts or not. 21

To the positivists experiment is an inescapable feature of the sciences. It is through experiment that a thesis is confirmed or rejected; for this reason they see empirical testability as the criterion of scientific knowledge. Anybody of knowledge that cannot be exposed to empirical testability is a Pseudo-Science. In this procedure observation is very imperative. Our statements depends on:

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21. Victor Kraft "Popper and the Vienna Circle", in The Philosophy of Karl Popper, Book I, Edited by P.A. SCHILPP (ILLINOIS, The Library of Living Philosophers, 1974) p. 190.

what we have observed. Thus,

> an observational statement expresses a present subjective experience; it is a simple assertion of what presently is given to someone. Such a statement always has the form: 'here' and 'now' such and 'such' ... 22

The words 'here' and 'now' and 'such' and 'such' are meant to emphasize the concrete situation as reported by the agent making the observation. This kind of statement loses its special meaning immediately at any point in time, it is no longer expressed within the original situation - as the words then cease from pointing at the same data. The validity of such a statement is only within the period of the experience.

Neurath and Carnap have a different conception of an observational statement (a protocol sentence). They do not see it just as an assertion as Schlick puts it, but a report about it.

> In a protocol sentence an assertion assumes an objective form without demonstrative words. A protocol sentence reports that a certain person has made an assertion at a certain time and place. 23

In a research work, this kind of objectivity is important as an easy reference could be made, and an experiment repeated. Equally, for the testing of a hypothesis or theory to take place genuinely the knowledge of objective facts is necessary. As Kraft Points out,

22. Ibid. P. 192.

23. Ibid. P. 192.

> statements about those, however, transcend the empirically - given because they use general concepts (whereas experiences are something unique). They always include a theory, because they speak about objects and events of the External World. 24

The knowledge of the external world cannot be logically deduced from individual experiences.

Schlick has a different conception as to the status of an observation. He ascribes absolute validity to the empirical statement of an observation. The important factor here is that the experience has to be correctly recorded and reported. An assertion, he posits cannot be false, cannot be erroneous, for whoever experiences something knows it and cannot be deceived by it."...assertion is the only statement about reality which is not hypothetical. For this reason, Schlick declared it to be the foundation of knowledge".[25] Thus Schlick's conception of knowledge is influenced by his perception of the status of observational statements. He has faith in the power of our sensation as it relates to experiencing objects and making such known through statements - which he called observational statements. It is only such statements that can give us reality that is not hypothetical. He believes that it is only in science that we have this special feature of empiricism. He puts it thus,

> Every science (in so far as we take this word to refer to the content and not to the human arrangements for arriving at it) is a system of cognition, that is, of true experiential statements. And the totality including the

24. Ibid. P. 192.

25. Ibid. P. 193.

> statements of daily life, is the system of cognitions. There is, in addition to it, no domain of 'philosophical truths'. Philosophy is not a system of statements; it is not a science. 26

Thus empirical testability made possible by observation remains what demarcates scientific knowledge from extra - scientific assertions. The essence of testing hypothesis is actually to know that which is able to agree with our prediction. When a hypothesis is confirmed it means it has been able to undergo a number of tests in addition to agreeing with our prediction. In essence, as Kraft points out, "the theories which prove the most successful are just those which are in best agreement with inter subjectively consistent observations-reports." 27

Carnap states that though there may not be 'truth' there is confirmation. He sees truth and falsity as timelessly valid whereas Corroboration's validity is only with reference to the particular time the test was carried out and confirmed. Carnap is believed to have weakened the position of verification by the above position.

The Positivists were interested in epistemological problems. All other areas of Philosophy, they asserted, are inaccessible to scientific knowledge, as their propositions cannot be confirmed or rejected

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26. Moritz Schlick "The turning point in Philosophy" in the <u>Logical Positivism</u>, P. 56.

27. Kraft, <u>The Vienna Circle</u>, P. 194.

through the empirical scientific method.

They see traditional problems of philosophy as meaningless pseudo-problems.

> It is the task of philosophy to expose this fact and, beyond that, to clarify the concepts and sentences of science and to eliminate those discovered as being meaningless. - Philosophy therefore is an activity, 'a system of acts' -not a 'body of knowledge' of ... true empirical sentences. 28

Thus the positivists see philosophy not as a science but as the analysis of language. Schlick goes on to express this thus,

> By means of philosophy, statements are explained, by means of science they are verified. The latter is concerned with the truth of statements the former with what they actually mean. The content, soul and spirit of science is lodged naturally in what in the last analysis its statements actually mean; the philosophical activity of giving meaning is therefore the alpha and omega of all scientific knowledge. 29

This is the same as saying that philosophy supplied the foundation of science.

Because of the emphasis of the Positivist on data collection, observation and experience are indispensable to scientific investigation. As a consistent observation of a phenomenon takes place over a period of time an inductive generalisation is made. The system adopted by the Positivists, as far as scientific research is concerned, agrees with the inductive method; as in induction

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28. Kraft, Popper and the Vienna Circle, P. 199.

29. Schlick, P. 56.

observation, collection of data and a subsequent generalisation fit into their requirement of science. Imre Lakatos stresses this point when he said" according to inductivism only those propositions can be accepted into the body of science which either describe hard facts or are infallible inductive generalisations...,"[30] Knowledge obtained through this process, they believe, is reliable.

The foundation of knowledge in science is located no where except in experience, they believe. The knowledge that comes from experience has a solid foundation that can hardly be faulted. That which is given in experience constitutes the absolute foundation of knowledge. Weimer also perceives perceptual experience as "... that ... taken to be self-authenticating and therefore as a foundation that is not in need of further foundation"[31] The assumption is that the factual basis from where the observation language in science is constituted is derived from perceptual experience.

To the positivists, reality is the exclusive preserve of scientific phenomena. To be real to them, is synonymous to fitting into

30. Imre Lakatos "History of Science" in the Method and appraisal in the Physical Sciences: The critical background to modern science, Edited by Collin Howsen (London, Cambridge University Press.

31. Walter B. Weimer, Notes on the methodology of Scientific Research, (New Jersey, Lawrence Erlbaum Associates, Publishers, 1979) P. 21.

a spatio-temporal system of the inter-subjectively observables. Kraft stresses this point thus,

> ...Philosophy does not investigate a special domain of reality. As regards empirical reality, that is the concern of the special sciences; and non-empirical transcendental reality is no possible object of knowledge. The traditional subject matter of metaphysics, absolute being, as well as absolute values and norms cannot constitute a special domain of knowledge. 32

Whatever questions asked or assertions made about such a domain lacks factual content. Philosophy, he (Kraft) believes, cannot be a special science since it is not a system of truth. It is not centred with the methodology of inductive experience but with reason and therefore cannot fit into the structure of science.

A critical summary of the theses examined so far will be done below. It is in the process of this that the views examined so far will be evaluated.

5. CRITICAL SUMMARY

The position of the realist, as noted in the course of this work, is simple. They maintain that a statement in science is either true or false-there is no mid-way. The relativists have a different conception of what truth or falsity is all about in science. Truth or falsity to them should be evaluated in terms of environment, period or one's orientation.

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32. Ibid. p. 188.

Thus one can say that they do not have objective criteria for truth or falsity.

Instrumentalism is a more developed-idea when compared with relativism. Instrumentalists see scientific theories as necessary weapons in research activities. Theories, according to them, are used to systematize our observations. Sense data thus constitute vital factors in their system of thought.

Logical positivism, no doubt, is a well-developed school of thought in science. The positivists see themselves as a set of people, that were out to 'purify' science and free it from the influence of metaphysics. They thus use meaningfulness and meaninglessness as criteria to discredit metaphysics in particular and to some extent philosophy. Verification and confirmation were the two very important methodologies used most effectively in the pursuit of their objective.

The position of the realists poses a number of problems as to the actual status of scientific theories. If we insist on seeing scientific theories as truth instead of as attempts to reach at that, what then happens when a theory is discovered not to be a solution any longer to the problem it had solved previously? It is safer to see truth in science as an ideal which every theory in science tries to get at. Rudolf Carnap seems to agree with this

line of reasoning when he stated that "truth in science is a regulative ideal; we may aspire to it, but even if we fell upon it, we would be unable to know it as such".[32] A theory which is seen as an answer to a problem today may prove incapable of doing the same the next day. This is not unusual. Man becomes more complex, and knowledgeable with the passage of time. In the same way our environment and physical conditions are not static. These factors are capable of rendering a valid theory invalid at any point in time. When Lavoisier discovered oxygen for instance he called it 'acid', for he thought that oxygen was an essential component of acid. It was later established that hydrogen and not oxygen is the essential component of acid hence acid is seen as "any substance having in its molecule a hydrogen atom..."[34]
The ... position of Lavoisier was taken to be true until it was proved otherwise

It does not seem very appropriate to insist on the use of the terms true or false in describing scientific theories. Truth is too ideal a term to use in describing a system that is as dynamic as science. Science and scientists have long overcome the trouble of using 'truth' to describe their theories. As earlier on remarked, probability is the term that has taken over. And for the fact that scien-

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33. Weimer p. 41

34. Graham, et al Book 3, p. 94.

tists operate with many variable factors, they can control only to a limited degree: they cannot claim to make a prediction that should go with the appelation of truth.

The position of the relativists looks absurd in a way. They do not seem to see science in an objective perspective. To attach the truth or falsity of a theory to one's environment and orientation lacks reasonable support. Newton's law of universal gravitation applies everywhere no matter the society in which it is tested and the orientation such a place may have. In fact scientific laws have universal application. They are not limited to any given environment. When the need for an abandonement of a given law arises as a result of its inadequacies in a number of ways such abandonement is universal. It is only ignorance that will make a given community to be operating a scientific law that is obsolete.

The position of the instrumentalists is also not appealing. One cannot agree with their thesis that the essence of scientific theories is prediction. As Newton - Smith puts it ".... We aim in science at more than prediction".[35] Science predicts quite alright, but that does not exhaust its aims. Besides prediction, which is a fact in science, there is also the need to explain. As Newton-Smith argues further,

35. Newton-Smith, P. 30.

> We are not satisfied merely to know, say, when a freely falling body dropped from some height at some time will strike the earth's surface, we want to know why it falls at the rate it does. 36

We are not satisfied for instance, to know that sodium placed in a flame turns yellow; we are anxious, too, to establish the factors that were responsible for this action. Thus, besides our desire to predict we also want to explain things in science. It is not enough to predict that an animal locked up in an enclosure will die, we equally need to explain that the death is caused by suffocation resulting from lack of air, specifically oxygen.

The thesis of the instrumentalist that experimental findings cannot contradict scientific laws lacks support from the history of scientific development. The issue of number of chromosones in a human cell is a case in point. Initially it was accepted to be 48, until a team of biologists working on human cells established it to be 46. In the same way it was through experiment that Ptolemy's theory of the position of the sun and the earth was refuted by the more suitable theory of Copernicus which is that the earth rotates round the sun.

36. Ibid, P. 31.

Tolerance as advocated by the instrumentalists as far as scientific theories are concerned is received with mixed feelings. A theory which has some defectiveness could be tolerated, granted that there is prospect that its status could be strengthened by more serious in-puts. But where a theory is seen to be very far away from reality there seems to be no basis for tolerating it. There is a competition between rival theories in science: whichever one proves more effective in tackling the problem at hand is preferred to the others. This is what makes for progress in science.

The thesis of logical positivism as it relates to science, metaphysics and philosophy has generated a lot of controversies. The first impression one gets from their theses is that whatever we can say, or could be said, is reducible to elementary statements. In this case, as Anyanwu points out, "all abstract scientific statements and hypothesis are ultimately short hand descriptions of observable events"[37] This assumption cannot be sustained. It is not possible to translate all statements about physical objects into statement about sense data. Such a practice is not feasible. In science there are a number of assumptions that cannot be reduced into sense experience.

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37. Anyanwu, P. 23.

A hypothesis may derive support from other things other than the experiences from sense data. As Hempel rightly argued,

> the support that may be claimed for a hypothesis need not all be of the inductive - evidential kind ... it need not consist entirely or even partly of data that bear out test implication derived from it. Support may also come from above; that is from more inclusive hypothesis or theories, that imply the given one and have independent evidential support. 38

He gave an example with the hypothetical fall on the moon, which was reached without any checking of its implications experimentally on the moon. Its strong support came deductively from Newton's theory of gravitation and of motion in conjunction with the information that the radius and the mass of the moon are 272 and 0123; that the gravitational acceleration near the surface of the earth is 32.2 feet per second. 39

It is really puzzling that the Positivists laid a lot of emphasis on observation without qualification. They were even carried away to the extent that all knowledge from experience and observation they see as valid. In laying emphasis on observation they were not careful enough to free science from the charge of being subjective. There is the need for us to make a distinction

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38. Carl Hempel, Philosophy of Natural Science (Princeton, Prentice-Hall, Inc; 1966) P. 30.

39. Ibid. P. 30.

between objective science on one hand and our knowledge on the other. We could be aware of facts by observation, but the interpretation given to this is really what matters: the experiences that are realised from this could be subjectively or objectively interpreted. If an observer says, for instance, that this is a raven and points to a bird in front of him, this could be seen as an observational statement, which was produced by the observer because of the impression, sensation and perception he had. As Feyerabend (who incidentally gave the above instance) says, the sentence made above is not about impression, it is rather about a bird which is neither a sensation nor the behaviour of some sentient being. In the same vein, argues Feyerabend,

> it may be admitted that the observation sentences which a scientific observer produces are prompted by his impression. However their content will again be determined, not by these impressions, but by the entities allegely described. 40

Thus the emphasis on observation should be on the interpretation given to experiences which really determines its objectivity.

There are, besides, many approaches to the study of science. To restrict the method of science to what we just observe or experience empirically will definitely not be in the interest of science. If it is said that science starts and ends in the laboratory we have to explain what we mean, its extent and scope. It is difficult to,

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40. P. F. Feyerabend "Explanation, reduction and empiricism" in the Minnosata Studies in the Philosophy of science, vol. III, Scientific Explanation, space, and Time, Edited by Herbert Feigl p. 35.

carry on with the enterprise of science without our reason. Thus observation, experiment, reasoning are all essential features of science. As Sir George Thomson pointed out,

> some sciences are observational such as for example, astronomy where apart from the moon, no one has yet seriously tried to set into action forces in the objects observed. On the other hand you have the experimental science of physics which might in some respects be regarded as rather similar but in which it is possible to make laboratory experiments under carefully controlled conditions.41

Some sciences too are equally mathematical which have to do more with reason than observation and experiment. Even though we may concede the fact that before a hypothesis is verified that there is the need for experiment and observation, it has to be noted that observation in science is not as simplistic as the positivists want us to believe, and it does not as has already been pointed out exhaust scientific enterprise. Besides it needs to be made very clear if an observation can just take place a priori in science. As Nagel and Cohen state 'even apparently random' observation requires the use of hypothesis to interpret what it is we are sensing. We can claim indeed that we 'seen' the fixed stars, the earth eclipsing the,

> Moon, bees gathering nectar for honey, or a storm approaching. But we shall be less ready to maintain that we simply and literally see these things unaided by any theory, if we remember how comparatively recent in human history are these explanations of what it is we see. 42

41. Sir George Thompson "Some thoughts on the scientific method" in the Boston Studies in the Philosophy of Science, Vol. II. in honour of Philip Frank, Proceedings of the Boston Colloguium for the Philosophy of Science. Edited by R. S. Cohen et al. (New York, Humanities Press, 1964) P. 81.

42. M. R. Cohen and E. Nager, An Introduction to Logic and Scientific Method (London, Routledge and Kegan Paul, Ltd, 1963) P. 216.

They argued that except we identify observation with an immediate ineffable experience we just need to employ hypotheses even in observation; for the objects of our seeing, hearing and so on have meaning for us only when we are able to link up what is directly given in experience with what are not objects of perception classified in view of known similarities between them. Such similarities are those that are known to be significant basically as a result of the theory we hold. It is for this reason that a whale is noted as a mammal, not fish despite certain superficial resemblances that exist between whales and fish.

Observation can be erroneous. There is thus the need to note the conditions under which observations are made; if such are not to be rendered unreliable and worthless. Observation in science especially is not a very casual affair. As Cohen and Nagel noted too,

> All but primitive observations are carried on with the aid of specially devised instruments. The nature and limitations of such instruments must be known. Thus readings must be corrected and interpreted in the light of a comprehensive theoretical system. 43

The logical positivists just kept on harping on observation, unmindful of how subjective such could be, and how observation can really be an asset to scientific research. They also failed to take special notice of the fact pointed out by Reichenbach that "... the empiricist principle includes the application of mathematics to physical reality." 44 This is an application of reason.

The distinction that the positivists made between science and philosophy and the criteria they applied in doing this are unconvincing, and so is their assault on metaphysics. Their criterion for meaning —

44. Hans Reinchenbach "Rationalism and Empiricism" in modern philosophy of science, selected essays by Reichenbach, Editor Maria Reichenbach (London, Routledge and Kegan Paul, 1959), p. 143.

fulness is definitely unacceptable. Empirical tests as demanded by them do not exhaust what is meaningful. Some critics have even pointed out, as Anyanwu stressed,

> that the verification principle of the logical Positivists is not itself subject to verification. They point out that in the way people use the word 'meaning' metaphysical statements are meaningful. In fact the verification principle was not established as a result of an empirical investigation hence one wonders if the principle is not a metaphysical one. 45

Besides, their reference to experience as criterion for confirmation of a hypothesis leaves much to be desired. They did not in making this assertion consider the fact that sense experience is private, and that it will be absurd to reduce the cognitive meaning of experience to pure privacy.

Their stance on metaphysics is rooted on superficiality and parochialism. Metaphysics is part and parcel of life. As Philip Frank points out it (metaphysics) is "... of the highest value for human life".45 Metaphysics cannot be divorced from science. Interpretations in science go beyond the observable, and this is nothing but its metaphysical features. And the "Metaphysical interpretation of science ... is a search for the reality behind the physical phenomena".46 In buttressing his point Frank quoted the famous statement of a German Physicist Max Planck who believed strongly in the metaphysical interpretation of science. He (Planck) said

45. Anyanwu, P. 25.

46. Philip Frank, "Metaphysical interpretation of science, Part II in The British Journal for the Philosophy of Science, Vol. 1, No. 2, 1950, P. 60

> Every physical theory contains two types of terms - the terms denoting sense observation (red, warm...) and the symbols forming the general principles (potential, force...). The positivists, according to Planck, argue that beyond these two realms of concepts, nothing can be said about physical science, but the adherent of a metaphysical interpretation assumes ... a third realm which tells us about the 'physical reality' behind the symbols and behind the sense observation. 47

There is no way sense observation can exhaust what there is in an object. To everything that we experience there is a metaphysical reality behind it. When we are able to appreciate the analogy of doctrine then the process of the acquisition of metaphysical knowledge will be clear, for instance, the physical law of conservation is in consonance with the metaphysical law that it is impracticable for something to become nothing and vice versa.

As Frank points out, in the physical theories and formulation by differential equation, method of integration is capable of enabling us to predict and to handle phenomena, but does not give us understanding. The alternative left is to resort to metaphysics for interpretation. This is because 'behind the phenomena and the equation' there is in existence the real world and to describe this reality we apply analogy that makes use of everyday language. And, in fact, physical phenomena are nothing more than mental occurrences. Thus the "so-called metaphysical interpretation of science have been attempts to make the general and abstract principles of science more intelligible."[48] Those abstract principles of science are interpreted in the language of metaphysics that is close to the language of common sense.

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47. Ibid. p. 62

48. Ibid. p. 63

Aristotle appreciated the close link between philosophy and science and states that the two are nearly the same. Our investigation, he believes, starts naturally from what is easily knowable and more evident to us. From there we proceed to what is self-evident and intrinsically more intelligible. We proceed this way for it is a part fulfillment for something to be knowable to us and another for it to be objectively intelligible. The interpretation of things metaphysical is meant to be self-evident and intrinsically clear and should be based on objectively intelligible principles

Metaphysics occupies a very dominant position in man's life which makes its import on science very prominent. As Edward Brutt points out there is no way man can escape from metaphysics: the only escape man has from metaphysics is for him to avoid saying something. But man is always anxious to go beyond what he knows and understands, the forces that influence his life. The knowledge of metaphysics is inevitable for this and ...

> since human nature demands metaphysics for its full intellectual satisfaction, no great mind can wholly avoid playing with ultimate questions especially where they are powerfully thrust upon it by considerations arising from its positivists investigations, or by certain vigorous extra-scientific interests such as religion. 49

Even Newton in all his attempts to avoid metaphysics could not be successful. He (Newton) was not in love with hypotheses - that is explanatory propositions which were not immediately deducible from phenomena. Despite his position, he found himself following his

49. Edward A. Brutt, The Metaphysical Foundation of Modern Physical Science, (London Routledge and Kegan Paul Ltd. 1964) P. 224.

illustrous predecessors and gave answers or assumed answers, to such fundamental questions "as the nature of space, time and matter, the relations of man with the objects of his knowledge and it is just such answers that constitute metaphysics."[50] In the absence of making a number of assumptions about the cosmos, assumptions that are obviously metaphysical, a researcher, especially in Physics and astronomy cannot make reasonable advancement.

It is thus difficult for us to accept the distinction made between natural science and philosophy by the positivists, even to the extent of diminishing the status of philosophy in general and metaphysics in particular. It is amazing to think that science which emerged from philosophy should be used to destroy Philosophy. The true situation is perhaps Descartes' comparison of philosophy to a tree, as noted by Frank,

> The roots are metaphysics, the trunk is physics, and the fruits on the branches are applied science-Medicine, Mechanics and Ethics. 51

This analogy represents the relationship between Philosophy, Science and Metaphysics and as Weimer equally noted.

> Philosophy at one time or another has been the mother of virtually all intellectual disciplines including the physical sciences and the nascent psychological science. In most cases the birth pangs have been traumatic with the mother jealously trying to retain sovereignty... and the offspring childishly distantiating itself from its intellectual heritage.

He (Weimer) went further to assert that philosophy and the sciences in their present form enrich one another, as their relationship is

50. Ibid. p. 21.

51. Frank, p. 68.

52. Weimer, p. 72.

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symbiotic. By this statement one can see how shaky the position of the positivists is as it relates to philosophy in general and metaphysics in particular and how hollow their criteria of determining what constitutes science is.

It is to be remarked however that the positivists were in their divorce of metaphysics from science influenced by the conventional practice of science at the point in reference. At this point in time scientists were not given to assuming any principle about reality or the universe, and were in agreement with Kuhn's thesis applied to existing scientific paradigms. Nonetheless it is to be noted that every scientific paradigm or major scientific theory made an assumption of the universe or reality. And this assumption of the universe or reality is invariably a metaphysical principle. This is the point the Positivists did not give serious thought to, hence it is easy to dislodge their stand-point.

Popper's position on metaphysics and other issues raised in this chapter will be known as we treat Popper's critical as well as his substantive views in the next chapter. Topics to be covered here include induction, deduction, the method of science, issues in probability as well as simplicity.

> in inferring universal statements from singular ones, no matter how numerous; for any conclusion drawn in this way may always turn out to be false: no matter how many instances of white swans we may have observed, this does not justify the conclusion that all swans are white.[1]

There is always the possibility that one day we may come across a swan that is black. In that case, our generalisation that all swans are white is then refuted.

Popper sees a number of problems associated with the thesis of induction. There is the issue of how the words we make use of to give a description of the world are related to the world as we experience it. There is equally the issue of examining the justification or otherwise of inductive inferences. We are equally confronted with the problem of how to establish the truth of universal statements whose basis is experience, such as the hypotheses and theoretical systems of empirical science.[2] We can hardly lend support to the view that the truth of these universal statements is known by experience. This is because it is clear that an account of an experience - of an observation or the result of an experiment - can first and foremost be only a singular statement and not a universal one. Hence, people who say of a universal statement that,

> we know its truth from experience usually mean that the truth of this universal statement can somehow be reduced to the truth of singular ones, and that these singular ones

1. Popper, The Logic of Scientific Discovery, (London, Hutchinson & Co., Publishers Ltd., 1959), p. 27.
2. Ibid, p. 28.

> are known by experience to be true;
> which amounts to saying that the universal
> statement is based on inductive inference.3

To be able to justify inductive inference, we need first of all to establish a principle of induction. This principle involves providing a statement by means of which we should be able to put inductive inferences into a logically acceptable form. The advocates of inductive methodology are of the view that a principle of induction is a '<u>sine qua non</u>' condition for scientific research. They see it as the determinant of the veracity of scientific theories. In the absence of induction, they reason, science will be lacking a veritable instrument by which the truth or falsity of a scientific theory is established. In such a state of affairs (absence of induction) science will have no more features than there are in the arbitrary creation of the poet's mind. One of the strongest articles of faith in the principle of induction is that the future will be like the past. It is in actual fact a way of predicting the future after a meticulous observation of the past and noting a number of regularities in given events. But is it reasonable to believe that the future will always be like the past? Popper's position is that it is very reasonable to believe that the future in many fundamental ways will be very much different from the past. However, he says,

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3. Ibid, p. 28.

CHAPTER TWO

Popper's Critical and Substantive Views

In this Chapter, we are to examine Popper's critical views on methodology in science as well as the methodology that he favours. In doing this, we shall look into such topics as induction, deduction, demarcation of science and metaphysics and issues in Probability. As earlier on remarked the methodology of science that Popper advocates is an outcome of his critical attitude to other methods especially induction as championed by the positivists.

1. INDUCTION AND THE LOGIC OF KNOWLEDGE

The methodology of science is not a settled matter among practitioners as well as philosophers of science. How can the procedure adopted by a scientist in his research programme be described? There is a popular and almost overwhelming belief that induction is an essential feature for practitioners of the empirical sciences. This procedure entails a progressive movement from the particular to the general. Popper dismisses this thesis. He believes it lacks a strong basis as it is riddled with confusion and inconsistency. He fails to be convinced by the position that induction which is purportedly an inference drawn from singular statements called particulars to general statements called universals constitute the method of science. This position he believes, lacks justification, for there is no way it follows that we are justified from a logical point of view, symbiotic. By this statement one can see how shaky the position of the positivists is as it relates to philosophy in general and metaphysics in particular and how hollow their criteria of determining what constitutes science is.

It is to be remarked however that the positivists were in their divorce of metaphysics from science influenced by the conventional practice of science at the time they wrote. At this point in time scientists were not given to assuming any principle about reality or the universe and were in agreement with Kuhn's thesis applied to existing scientific paradigms. Nonetheless it is to be noted that every scientific paradigm or major scientific theory made an assumption of the universe or reality. And this assumption of the universe or reality is invariably a metaphysical principle. This is the point the Positivists did not give serious thought to, hence it is easy to dislodge their stand-point.

Popper's position on metaphysics and other issues raised in this chapter will be known as we treat Popper's critical as well as his substantive views in the next chapter. Topics to be covered here include induction, deduction, the method of science, issues in probability as well as simplicity.

> It is perfectly reasonable to act on the assumption that it will, in many respects, be like the past, and that well-tested laws will continue to hold (since we have no better assumption to act upon); but it is also reasonable to believe that such a course of action will lead us at times into severe trouble, since some of the laws upon which we now heavily rely may easily prove unreliable. 4

Popper strongly repudiates the veracity of any proposition in which the premise supports the conclusion without entailing it. A scientist who has established that when 1,000 randomly selected samples of sodium were placed in a flame it turned yellow, has, in the view of Popper, no justification to assert that the hypothesis that sodium turns a flame yellow is probably true.[5]
If it happens that one piece of sodium were unable to turn a flame yellow we are right to reject the hypothesis. This is because the premise, 'this piece of sodium is unable to turn the flame yellow' leads us to the conclusion that 'it is not every piece of sodium that turns yellow when placed in a flame. On the basis of this, Popper posits that the method of science does not entail evidence gathering but rather 'Conjecture and refutation.'

Popper remarks that his approach to the principle of induction is through Hume. He agrees with Hume that the principle of induction cannot be logically justified; as it is difficult to establish the validity of the arguments

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4. Popper, Conjectures and Refutations, The Growth of Scientific Knowledge, (London, Routledge and Kegan, Paul), 1963, p. 56.

5. W. H. Newton-Smith, The Rationality of Science, (London, Routledge and Kegan Paul, 1981) p. 44.

> that those instances, of which we have had no experience, resemble those of which we have had experience.' Consequently, 'even after the observation of the frequent or constant conjunction of objects, we have no reason to draw any inference concerning any object beyond those of which we have had experience. 6

Popper agrees entirely with Hume's refutation of inductive inference but he "felt completely dissatisfied with his psychological explanation of induction in terms of custom or habit."[7] He sees his explanation as psychologically inclined. What the explanation tries to give us is a causal explanation of a psychological fact, "the fact that we believe in laws, in statements asserting regularities or constantly conjoined kinds of events - by asserting that this fact is due to (i.e. constantly conjoined with) custom or habit." 8 Popper views this reformulation of Humes theory as unsatisfactory as the psychological theory was mistaken and is refutable on purely logical grounds. Hume's central thesis, says Popper, is that of repetition, which is based on similarity, and the idea is used in a very uncritical way;

> We are led ... to think of sequences of unquestionable like events slowly forcing themselves upon us as does the tick of the clock. But we ought to realise that in a psychological theory such as Hume's, only repetition-for-us, based upon similarity-for-us,,can be allowed to have any effect upon us. We must respond to situations as if they were equivalent; take them as similar, interpret them as repetitions. 9

6. [illegible] Popper, Conjectures and Refutations, [illegible] p. 42.
7. Ibid, p. 42
8. Ibid, p. 42
9. Ibid, p. 44

Popper argues that the kind of repetition that Hume envisages can hardly be perfect. What he believes we can get are cases of similarity, not that of perfect sameness. Besides, repetition may not have the same effect on all creatures. And for logical reasons,

> there must always be a point of view - such as a system of expectation, anticipations, assumptions, or interests - before there can be any repetition; which point of view, consequently, cannot be merely the result of repetition.[10]

There is thus the need for us to replace for the purposes of a psychological theory of the origin of our beliefs, the naive ideas of events that are similar by the idea of events to which we react by interpreting them as similar.[11] And since the situation is this way, Popper asserts that Hume's psychological theory of induction leads to infinite regress that is similar to the one he (Hume) discovered and employed to rock the foundation of the logical theory of induction. We achieve similarity by a response which involves interpretations (though inadequate) and anticipations or expectations (that may not be fulfilled). Thus, we can never explain anticipations, or expectations as outcome of many repetitions as Hume wants us to believe. This is because even the first 'repetition-for-us' has to be based upon 'similarity-for-us' and consequently upon expectations - clearly the kind of thing we wished to explain. What we see from this is the involvement of infinite regress in Hume's psychological theory.

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10. Ibid, pp. 44 - 45

11. Ibid, p. 45.

Popper reasons that Hume failed to finish up properly the task he started in which he refuted the theory of induction. Having succeeded in refuting this theory, he (Hume) was faced with the problem of how we obtain . our knowledge, since induction lacks logical validity and rational status. And in looking for an answer, he failed to consider seriously, the fact that we acquire knowledge by non-inductive procedure. So having cast out the logical theory of induction by repetition he struck a bargain with common sense, meekly allowing the re-entry of induction, by repetition in the guise of a psychological theory.[12]

Popper rejects this in its entirety. He does not accept that we can explain our propensity to expect regularities as the result of repetition, instead repetition is the outcome of our propensity to expect regularities and to search for them.[13] Popper sees the belief in inductive logic as essentially the outcome of a confusion between psychological and epistemological problems.

The function of a scientist, he says, is that of proposing and testing ideas. A logical analysis of an idea at a primary stage is effort in the wrong direction. Besides,

> 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 empirical psychology, but it is irrelevant to the logical analysis of scientific knowledge. 14.

12. Ibid, p. 46.
13. Ibid, p. 46.
14. Popper, The Logic of Scientific Discovery, p. 31.

The latter's central interest is not with the question of fact, but rather with the issues of justification or validity. The kind of questions it asks include: Is it possible to justify a statement? If there is a basis for such justification, how can it be done? Can such a statement undergo tests? Is the statement logically dependent on certain other statements, or does it perhaps contradict them? Before a statement can be logically exposed to this kind of examination, it must have been presented to us. Somebody must have formulated it and submitted it for logical evaluation. Thus, the crucial task of the logic of knowledge in contrast to its psychology entails essentially the investigation of the methods applied in those systematic tests to which every new idea must be subjected if it is to be seriously entertained.

Popper is not impressed in the least by all the talk which centres on a logical method of having new ideas. This talk and belief lack strong foundation. Every discovery, he avers, has an 'irrational element' or a 'creative intuition'. Einstein according to Popper, reasons in the same vein when he talks about the search for those highly universal laws from which a picture of the world can be obtained by pure deduction. The attainment of these laws does not entail any logical procedure. We are capable of reaching the laws instead by intuition based upon something like an intellectual love of the objects of experience.[15]

Popper also refutes the view that basic statements are justifiable by reference to perceptions: he sees this as a kind of psychologism. He did not doubt the fact that

15. Ibid, p. 32.

the decision to accept a basic statement is causally linked with our experiences, especially our perceptual experiences. It is inconceivable, however, to attempt to justify basic statements by these experiences. It is quite in order to believe that experiences can motivate a decision and invariably an acceptance or a rejection of a statement, but there is no way a basic statement can find justification in them.

Similarly, empirically definable concepts were also denied by Popper. It is thus non-practicable to posit an assertion that will actually express a definite datum of experience as unique and particular. It is essential for this reason that perceptual judgements cannot claim any privileged position. No matter in what form an assertion is, it is a hypothesis. Victor Kraft stressed this view of Popper when he quoted him thus,

> any attempt such as Schlick's to found science upon assertions carrying the quality of absolute conviction appears to him therefore as psychologism and thus as doomed to futility from the start. Feelings of absolute conviction like the feeling of self-evidence are purely psychological matters ... 16

Popper's position on hypothesis agrees with his view that inductive test is not possible in science. What he believes in is deductive test as far as scientific hypotheses are concerned. This test does not lead us to infinite regress. The detail is given below

2. DEDUCTIVE TESTS

There are different ways through which theories could be critically tested. First of all there has to be a new idea which is yet to be corroborated. Secondly, there has to be an anticipation, a hypothesis and a theoretical system. Then the researcher should embark on his test bearing the above

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16. Victor Kraft, The Vienna Circle, The Origin of Neo-Positivism, A Chapter in the History of Recent Philosophy, (New York, Greenwood Press Publishers, 1969,) p. 124.

facts in mind. His conclusions are expected to emerge logically on the basis of the tests he had carried out. The next stage is a comparison which he has to do between the various conclusions that he has drawn. This kind of comparison is also to be done with other relevant statements. By carrying out this assignment the logical relations (such as equivalence, derivability, compatibility or incompatibility) that exist between them have to be noted.

The testing of a theory could undergo four different possible lines. Foremost, we have a logical comparison of the conclusions we have reached. If this is successfully done we will be in a good position to test the internal consistency of the system. And consistency (internal) of a system is very crucial if the theory must have any information to offer. The next line of action is the investigation of the logical form of the theory. We have to do this basically so as to ascertain if the theory in question possesses empirical or scientific character or if it is, for instance, a mere tautology or metaphysical theory. The third stage involves other competing theories. And it is a comparison of our theory in question with others. The essence of this third stage is to establish if this theory is a bold step in scientific advancement, that is, if it could scale through some crucial tests. The fourth and the final stage constitute testing the theory by way of empirical application of the conclusions that could be derived from it. The utility of the final test is to corroborate to what extent the novel consequences of the theory, (whatever may be new in what it says)

stand up to the demands of practice, whether raised by purely scientific experiments or by practical technological application. What we have stated so far are procedures of testing theories deductively; "with the help of other statements previously accepted certain singular statements - which we may call 'predictions' - are deduced from the theory especially predictions that are easily testable or applicable."[17]
The next step we are to take is to seek a decision as regards these derived statements by a comparison between them and the results of practical applications and experiments. If the decision is positive this decision, that is, if the singular conclusions become accepted or verified, then the theory is taken to have passed our test for the time being.[18] In the circumstance we cannot but accord it a tentative acceptance. In contrast, however, a negative or false decision falsifies the theory from which they were logically deduced. We have to note that whatever positive rating we accord to the theory is very temporary as any negative decision can render such positive rating null and void. But "so long as a theory withstands detailed and severe tests and is not superseded by another theory in the course of scientific progress, we may say that it has 'proved its mettle' or that it is corroborated by past experience."[19]

In the whole of the steps and tests carried out or to be carried out, there are no traces of induction. There is no—

17. Popper, The Logic of Scientific Discovery, p. 33.

18. Ibid, p. 33

19. Ibid, p. 33

where an argument from the truth of singular statements to the truth of theories is made. The situation is this way, for in the view of Popper, as expressed by Munz:

> there is neither a logical nor psychological induction... the falsity of a proposition can be inferred from empirical evidence and this inference is a purely deductive one. 20

There is no rule for the application of inductive logic which he says is riddled with self-contradiction such that it is incapable of fitting into the mold which deduction does. Deduction, on the other hand, possesses known rules and procedure that can be followed with lots of ease. There is much certainty in its passage, it is not beset with probabilities, andit obeys especially the rules of logic. As pointed out by Agassi,

> the problem of induction is an expression of the wish to be assured not by a condescending parent or priest, not by blind faith, but by rational means. Can we justly predict and explain the success of science? This is the problem of induction. Popper says it is insoluble in a positive manner. For this reason he rejects induction in preference to deduction. 21

Popper was anxious to carve out an enduring and authentic method for empirical science. It wasfor this reason that he rejected induction. He was also anxious to demarcate science and its method from all other disciplines especially metaphysics, having however rejected the position of the positivists on thestatus of metaphysics.

3. DEMARCATION

Popper gives immense attention to the issue of demarcation. His interest on this matter centred on his desire to

20. Peter Munz "Popper and Wittgenstein" in the Critical Approach to Science and Philosophy in Honour of Karl R. Popper, (London, The free Press of Glencoe, 1964) p. 89.
21. Joseph Agassi "Positive Evidence in Science and Technology" in Philosophy of Science, Official Journal of the Philosophy of Science Association, Vol. 37, No. 1, March 1970, p. 266.

ensure that there is a proper appreciation between science and non-science. He was dissatisfied with the efforts of the positivists especially Rudolf Carnap to show that the demarcation between science and metaphysics fits into that between sense and nonsense. As remarked by Agassi,

> the reason is that the positivistic concept of 'meaning' or 'sense' (or of verifiability, or of inductive confirmability) is inappropriate for achieving this demarcation - simply because metaphysics need not be meaningless even though it is not science. 22

He did not see metaphysics as a term of 'intellectual abuse' for theorizing that can neither be classified as belonging to logic nor empirical science as the positivists tend to believe. If the positivists' stand is taken seriously, it will be discovered that their efforts to destroy metaphysics will equally have adverse effect on science as most scientific theories which have the features of metaphysics will be destroyed alongside.

The use of inductive logic or verificationism by the positivists to demarcate science and metaphysics (which they see as meaningless) is a complete failure, according to Popper. It is wrong too, he argues, for the positivists to believe that in the absence of inductive logic ... the barrier between science and metaphysical speculations is broken. Inductive logic is not capable of differentiating between science and metaphysics. **Popper sees** ... the issue of demarcation as that which is rooted on the need to establish the basis of distinguishing between the empirical sciences

22. ... Popper, Conjectures and Refutations, ... p. 253.

on the one hand; mathematics and logic, as well as a metaphysical system on the other. The inclinations of the Empiricists and Epistemologists to induction, Popper reasons, is because of their conviction that it is only through its methodology that a sufficient ground of demarcation between science and metaphysics could be found.

The Positivists of the old order, for instance, are prepared to accept as scientific or meaningful those concepts which they are convinced are derived from experience - those concepts or ideas that are logically reducible to elements of sense experience such as sensation (sense data), impression, perception, visual or auditory memories, among others. To this class of the Positivists, the above exhaust the meaningful sources of knowledge. Anything outside them is senseless. The modern Positivists have a different outlook and conception as to what constitutes science. Their attitude to science and what is scientific is not just in terms of a system of concepts but rather a system of statements.

Rudolf Carnap, for instance, believes that the development of modern logic has given more insight to the issue of validity and justification of metaphysics.

There have been positive as well as negative results from the researches carried out in applied logic or the theory of knowledge which focus is the clarification of the cognitive content of scientific statement and ultimately the meanings of the terms that occur in the statements by means of logical analysis.

Carnap says that,

> the positive result is worked out in the domain of empirical science; the various concepts of the various branches of science are clarified; their formal - logical and epistemological connections are made explicit. In the domain of metaphysics, including all philosophy of value and normative theory, logical analysis yields the negative result that the alleged statements in this domain are entirely meaningless. 23

He believes that the development of logic in the recent decades provides sufficient tools with which a complete elimination of metaphysics is possible since metaphysical statements cannot be divided into fruitful or sterile, true or false. In his own words:

> In the strict sense ... a sequence of words is meaningless, if it does not within a specified language, constitute a statement. It may happen that such a sequence of words looks like a statement at first glance; in that case, we call it a pseudo statement. Our thesis now is that logical analysis reveals the alleged statements of metaphysics to be pseudo-statements. 24

It is the above criterion of seeing metaphysics as meaningless in contrast with 'meaningful' scientific statements that the logical positivists applied for the demarcation of science and metaphysics. And this demarcation methodology favours inductive logic.

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23. Rodolf Carnap "The Elimination of Metaphysics through Logical Analysis of Language" <u>Logical Positivism</u>, p. 61.

24. Ibid, p. 61.

Popper failed to see the validity of the proposition of the positivists as far as metaphysics is concerned. He reasons that if the terms meaningless or non-sensical are used to describe those things that are outside the empirical science, then it is pointless to see metaphysics assuch since it has always been seen as non-empirical. However, it is obvious that the positivists meant to say more about metaphysics than that its statements are non-empirical. Kraft says:

> metaphysics means a claim to knowledge such as is inaccessible to empirical science, knowledge which transcends the latter:for metaphysical sentences it is altogether impossible to specify a method of verification, they are not reducible to what may be empirically given, and therefore they are without specifiable meaning. They are merely combinations of words which look like meaningful sentences; they are mere pseudo-statements. 28.

As must have become clear when the positivists use such derogatory words as 'meaningless' or 'nonsensical' to describe metaphysics, it is doubtful if their aim was really to demarcate science and metaphysics. It is more plausible to believe that their actual intention, far from a demarcation between science and metaphysics, is to destroy metaphysics as a branch of knowledge. It is remarkable, however, to note that at each point they (the positivists) attempt to explain what "meaningful" meant, such attempts led to the same result -- a definition of a meaningful sentence (in contrast to a meaningless or psuedo sentence) which does nothing but reiterate the criterion of demarcation which is

28. Kraft, The Vienna Circle, p. 33.

> Hume that there is no such logical justification: there can be none, simply because they are not genuine statements. 30

The only impression one can deduce from the above position is simply the difficulty of the inductivists' criterion to provide for a suitable demarcation between scientific and metaphysical systems. By this position, it has no better option than to give equal status to the two, as the conclusion we can deduce from their thesis is that the two systems are those of meaningless pseudo-statements. Thus, in their attempts to wipe out metaphysics, the positivists failed woefully. They rather permitted the erosion of science by metaphysical principles.

Popper demonstrated a lot of caution as far as the issue of metaphysics is concerned. He was not involved in the fruitless effort to destroy it as a source of knowledge. His interest was rather in the search for a very suitable method of demarcating metaphysics from science, to state in very clear language the features and characteristics of each of them so as to make for easy identification of either empirical or metaphysical propositions. The Vienna Circle follows

> Wittgenstein's <u>Tractatus Logico-Philosophicus</u>.. in the distinction between meaningful and meaningless sentences and this distinction was determined by verifiability: a statement is meaningful if it can be verified; statements of which non verification is possible are meaningless, pseudo-propositions. 31

30. Ibid, p. 37

31. Victor Kraft "Popper and the Vienna Circle" in <u>the Philosophy of Karl Popper</u>, Book I, p. 139.

This verifiability is their criterion of demarcation.

Popper rejects this criterion of demarcation. This meaning-criterion of verifiability excluded not only metaphysical sentences but also natural laws. In his words,

> this criterion is too narrow (and too wide): it excludes from science practically everything that is, in fact characteristic of it... No scientific theory can ever be deduced from observation statements, or be described as a truth-function of observation statements. 32

Popper perceives the problem of meaning as a pseudo-problem. Demarcation is what constitutes a real problem to him. Falsifiability and testability as introduced by him were not intended to tackle the issue of meaningfulness. The issue (meaningfulness) is not a strong factor in the study of the logic of scientific discovery in contrast to the problem of demarcation which is at the root of successful scientific research. In essence, his criterion of demarcation is to be understood as a proposal for an agreement or convention. He was not in doubt that his proposal would generate controversy: he believed strongly, however, that discussion can resolve whatever misgivings that might crop out from his proposal. Nonetheless whoever sees science in the sense of absolute truth or the possibility of complete verification of fact will definitely disagree with Popper's thesis on demarcation, as he did not for instance, believe as Tucker points out that "... we can conclusively verify assertions."[33]

32. Popper, Conjectures and Refutations, p. 40.

33. John Tucker "Clashes between Paradigms for Logic" The Nigerian Journal of Philosophy, Vol. I, No. 2, 1981, p. 82,

Popper's approach to science is dynamic as opposed to the dogmatic posture of the positivists. He sees it as a crucial task to always analyse the logical consequences of any problem he is faced with so as to pinpoint its fertility - its ability to clarify theproblems of the theory of knowledge. This methodology elicits favourable responses only to those who are favourably disposed to critical thought and logical rigour, those

> who seek practical applicability ... attracted by the adventure of science, and by discoveries which again and again confront us with new and unexpected questions challenging us to try out new and hitherto undreamt of answers. [34]

This informed his position on the issue of metaphysics: why he did notfall into the trap of the positivists to call it nonsensical or meaningless. (He equally refutes the view that metaphysics serves no purpose to the empirical sciences, as there are evidences that along with metaphysical ideas which have impeded the advancement of science, there have been others - such as speculative atomism - that have aided it.[35]) If viewed from the psychological standpoint, one can adduce that scientific discovery cannot thrive in the absence of faith in ideas that are most often speculative in nature and sometimes quite hazy. This kind of faith may have no direct relationship with science and it can be seen as nothing but metaphysics.

34. Popper, The Logic of Scientific Discovery, p. 38.

35. Ibid, p. 38.

Despite Popper's obvious sympathy with metaphysics, he was still convinced that it is simply imperative to develop appropriate language for empirical science so as to demarcate it from metaphysical ideas - even though that metaphysics has contributed to the development of science.

However, the positivists concept of 'meaning' or 'sense' or of verifiability or of inductive confirmability cannot achieve a basis for demarcation. Metaphysics, though not a science, cannot be seen to be meaningless. Popper equally refuses to accept the widely held view that science is characterised by its observational basis, or by its inductive method, whereas pseudo-sciences and metaphysics were characterized by their speculative method or, as Bacon said, by the fact that they operated with 'mental anticipation' - something very close to hypothesis. Popper refutes this thesis based on his conviction that modern theories of physics, such as Einstein's theory (widely discussed in the year 1919) were fundamentally speculative and abstract, and very far away from what fit into the so-called 'observational basis.' There is no convincing evidence that this has 'observational basis. The same things applies to Newton theory.

Surprisingly, however, many superstitious beliefs and rule of thumb procedures that could be seen in popular almanacs and dream books have a lot to do with observations and have often been based on something like induction.
Astrologers, for instance, have often made a bold claim that their 'science' has its foundation on inductive material. Popper believes, however, that such claim lacks basis but

that there has not been any serious effort to debunk the astrologer's alleged inductive materials. It is worthy of note, however, that astrology did not enjoy the acceptance of modern science. Its features are not in agreement with the accepted theories and methods. Popper's criterion of demarcation agrees with his general attitude to the methodology of science and status of hypotheses. A hypothesis, in his view, has to be falsifiable if it must pass as an empirical science. Thus falsification becomes his criterion for demarcation.

3.1 FALSIFICATION AS A CRITERION OF DEMARCATION

To have a clear view as to what constitute the ideas of empirical science is necessary in view of the fact that there are theoretical systems that have almost the same logical structure as those acceptable within the horizon of empirical science. The issue could be looked at by accepting the fact that there is a great number "presumably an infinite number - of 'logically possible worlds. Yet, the system called 'empirical science' is intended to represent only one world: the 'real world' or the world of experience."[36] For a better understanding, there are three basic requirements that our empirical system ought to satisfy. Foremost, it must be synthetic. This is to ensure its representation of a non-contradictory possible world. The second requirement is on the need for it to satisfy the criterion of demarcation. This is to say that it must not be metaphysical, by which we mean that it has

36. Ibid, p. 38.

to represent a world of possible experience. Thirdly, it must be a system that is distinguishable in some ways from other such systems as the one that represents our world of experience. The only possible way to distinguish the system that represents our world of experience is simply by the fact that it has been submitted to tests, and has survived the tests. And this means that the deductive method has been applied. Popper argues that

> what we usually call 'scientific knowledge' is ... information regarding the various competing hypotheses and the way in which they have stood up to various tests. 37

And the epistemology whose primary interest is the analysis of the method or procedure peculiar to empirical science, can rightly be classified as a theory of the empirical method.

Popper sees the criterion of demarcation deducible from inductive logic (that is the positivists dogma of meaning) as corresponding to the requirement that all the statements of empirical science (all meaningful statement) be capable of being finally decided with respect to their truth and falsity - that is they must be conclusively decidable. What this means is simply that their form must be such that to verify or falsify them must be logically possible. In Schlick's own words,

> a genuine statement must be capable of conclusive verification while Waisman says 'if there is no possible way to determine

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37. K. R. Popper, The Open Society and its Enemies, Vol. I, (London, Routledge and Kegan Paul, 1977), p. 29.

> whether a statement is true then
> that statement has no meaning whatsoever;
> for the meaning of a statement is the
> method of its verification. 38.

As earlier on stated, Popper was unimpressed by this inductive argument, and the induction he meant is not mathematical induction. He believes that in the study of empirical sciences there is neither 'inductive procedures' nor 'inductive inferences'. It was on the basis of this position that he rejected inferences to theories from singular statements that are verifiable by experience. Such is definitely logically inadmissible.

To avoid the pitfall of the positivists, he believes we need to make use of a criterion that allows us to admit to the circle of empirical science even statements that cannot be verified. However, only those systems that could be tested by experience can be accepted as scientific or empirical. Thus falsifiability rather than verifiability is to be taken as a criterion of demarcation.

> According to this view ... a system is
> to be considered as scientific only if it
> makes assertions which may clash with
> observations; and a system is, in fact,
> tested by attempts to produce such clashes,
> that is to say by attempts to refute it. 39

Testability is just the same as refutability (falsifiability), either can be used as a criterion of demarcation. It is his belief, as pointed out by Bottm, "... that no theory can ever

38. Popper, The Logic of Scientific Discovery, p. 40.

39. Popper, Conjectures and Refutations, p. 256.

be verified conclusively by any number of observations, however large, and that the question of falsifiability is in many ways more relevant than that of verifiability."[40]

Popper's requirement of a scientific system is not for it to be singled out, once and for all in a positive sense but that its logical form shall be such that it can be singled out by means of empirical tests. This is because as pointed out by Bernays " ... all the rationality contained in the development of science is to be found in testing and criticism."[41]

This criterion of demarcation as enunciated by Popper will no doubt generate a lot of reactions and oppositions. One of such likely reactions is the reasoning that the criterion might turn out to be a misplaced arrangement that will make science that is meant to give us positive information, presented as satisfying negative requirement such as refutability. This objection lacks foundation, because the range of positive information about the world that is conveyed by a scientific statement is much, the more likely it is to clash, because of its logical character with possible singular statements.[42] The 'Laws' of nature are called laws for obvious reasons - the more they prohibit the more they say. It is possible too that Popper's criticism of the inductivist criterion could be turned against him; that is his objection

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40. David Bottm "On the Problem of Truth and Understanding in Science" in the Critical Approach to Science and Philosophy, p. 213.
41. Paul Bernays "Reflections on Karl Popper's Epistemology", p. 40.
42. Popper, The Logic of Scientific Discovery, p. 41.

against verification can be used against his falsification. Popper is not bothered by this kind of objection for his proposal is founded on assymetry between verifiability and falsifiability; an assymetry which results from the logical form of universal statements. They cannot be derived from singular statements, but they can be contradicted by singular statements.

> Consequently, it is possible by means of purely deductive inference (with the help of themodus tollens of classical logic) to argue from the truth of singular statements to the falsity of universal statements. 43

This particular argument from singular to the falsity of universal statements is the only kind of deductive inference that proceeds in the 'inductive direction' - from singular to universal statements.

Popper believes his demarcation principle which presents empirical science as a system of propositions that could be refuted,

> had the great merit of showing what is wrong with the work of pseudo-scientists who claim to find empirical confirmation of their theories but refuse at the same time to indicate any conceivable circumstances in which those theories would be refuted. 44

He believes too that this is far better than the positivists' principle of verifiability as it did not make any attempt to condemn non-scientific theories as meaningless.

43. Popper, The Logic of Scientific Discovery, p. 41.

44. William C. Kneale "The demarcation of science" in the Philosophy of Karl Popper, Book I, p. 206.

This (seeing non-scientific theories as meaningless) cannot be, as theories of considerable merit such as the atomic theory of matter, emanated from pre-scientific speculations.

The cardinal fact of empirical science, Popper argues, is its readiness to be exposed to falsification in every way possible. The essence of this is to select the system that is more fitted than the other; and to acquire as many new ideas as possible, and as Feyerabend pointed out, "New ideas ... go far beyond the available evidence and must go beyond it in order to be of value."[45]

If it is accepted that falsifiability is our criterion of demarcation, it then will follow that singular statements have to be available to serve as premises in falsifying inferences. We are by so doing moved by our criterion to a new problem. We are moved from the issue of the empirical nature of theories to the question of the empirical character of singular statements. Inspite of this, however, we have gained something:

> For in the practice of scientific research, demarcation is sometimes of immediate urgency in connection with theoretical systems, whereas in connection with singular statements, doubt as to their empirical character rarely arises.[46]

There could be errors of observation, and when this takes place, it could give rise to false singular statements, but the scientist rarely has the chance to describe a singular

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45. Paul Feyerabend, Against Method (London, The Thetford Press Ltd., 1984,) p. 93.

46. Popper, The Logic of Scientific Discovery, p. 43

statement as non-empirical or metaphysical.

On the whole, it has to be noted that Popper's effort at demarcation between science and metaphysics centres on the need to know empirical theories as distinct from metaphysical ones, and that a theory is rational and acceptable. He remarks that it can be thoroughly examined:

> subject it to attempted refutations, including observational tests; and the fact that in certain cases, a theory may be able to withstand those criticisms and those tests - among them tests under which its predecessors broke down, and sometimes even further and more severe tests. It is in the rational choice of the new theory that rationality of science lies, rather than in the deductive development of the theory. 47

And for the fact that a scientific statement speaks about reality, Popper believes it must be falsifiable and, as Watkins remarks, "... in so far it is not falsifiable, it does not speak about reality."[48] And empirical learning, he believes, is not exhausted by learning facts and techniques but includes a development of our methods of thinking and talking concerning nature, with the aim of a better appreciation of the processes in nature.

The issue of observation is another crucial area as far as the methodology of science is concerned. The topical

47. Popper, Conjectures and Refutations, p. 221

48. J. W. N. Watkins "Metaphysics and the advancement of science" in the British Journal for the Philosophy of Science, Vol. 28, 1977, p. 106

matter here is whether science starts with observation or idea. Popper answers this question in the proceeding sub-section.

4. POPPER ON SCIENCE AND OBSERVATION

A major function of a philosopher which could be seen as a spectacular achievement is to see a riddle, a problem or a paradox which hitherto has not been seen by anyone else. Such stands is a greater achievement than the resolution of the riddle. Any philosopher that discovers a new problem excites a lot of interest among his contemporaries and in fact, shakes them up from their intellectual laziness. Such a philosopher does to us what Hume did to Kant: we are shaken up to action, to thoughtful endeavour.

It is believed that Kant was actually the first philosopher to appreciate the riddle of natural science. Whenever Kant talked about natural science what he had in mind was Newton's celestial mechanics. Kant is seen as the greatest cosmologist of all time. He accepts the veracity of Newton's celestial mechanics. As he puts it,

> Newton's theory not only accurately predicted the orbits of all the planets, including their derivations from Kepler's ellipses, but also the orbits of all their satellites. Moreover, its few simple principles supplied at the same time a celestial mechanic and a terrestrial mechanics. 49

Here was a valid system of the world that described the laws of cosmic motion in the simplest and most direct way possible

49. Popper, Conjectures and Refutations, p.185

and with absolute accuracy. The principles were clear enough as as simple and direct as geometry itself. In actual fact, Newton had a kind of cosmic geometry that consists of euclid's supplemented by a theory (represented geometrically) of the motion of mass-points under the influence of forces. It added, in addition to time, two essentially new concepts to euclidian geometry: the concept of mass or of a material mass-point, and the even more important one of a directed force. [3]

We note here a science of the cosmos, of nature and, as claimed, a science based upon experience. It was a deductive science just like geometry. Newton still asserted that he had wrested its functional principles from experience by induction. Thus, what Newton wants use to believe is that the veracity of his theory is logically derivable from the truth of certain observation statements.

There are prominent physicists that insist that Newton's laws can be derived inductively from observation statements. Kant recognises the absurdity in believing that Newton's theory could be derived from observations. Popper also rejects the view that Newton's theory was derived from observation.

Foremost, we need to appreciate how entirely a Newtonian theory differs from any observation-statement. It should be noted that observations are always inexact, whereas the theory on its part makes absolutely exact statements. It is unacceptable that more precise statements, especially absolutely precise ones of the theory itself, could be derived logically from less exact or inexact ones.

Even if the issue of precision is set aside, it is imperative for us to note that we can only carry out observation under special conditions. The claim of the theory is that it applies in all possible circumstances - not only to the planets, mars or jupiter, or even to the satellites in the solar system, but to all planetary motion and to all solar systems.[50] As a matter of fact, its claims transcend all these. For instance, the theory makes postulations about gravitational pressure inside the stars, claims that are yet to be tested by observation. Besides, it is a known fact that while theory is abstract, observation is concrete. What we observe, for instance, is extended planets, not mass points. This might not be important, but the crucial point is that we can never, under any circumstances, observe Newtonian forces.'

Force is defined such that we measure it by measuring acceleration. We can, at times, too, measure force not by measuring an acceleration but, for instance, with the help of a spring balance. In any of these measurements, the truth of Newtonian dynamics is always pre-supposed.

In the absence of a prior assumption of a dynamical theory, it is simply impracticable to measure forces. As a matter of fact,

> ... forces and changes of forces are among the most important things of which the theory treats.

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50. Ibid. p. 186.

> Thus we may assert that at least some of the objects of which the theory treats are abstract and unobservable objects. For all these reasons, it is intuitively not credible that the theory should be logically derivable from observations.51

We can only observe things that are concrete. Theories and, in particular, Newton's forces are abstract.

It is also historically false to assert that Copernicus' idea of placing the sun rather than the earth in the centre of the universe was an outcome of observation. It was, argues Popper, a new interpretation of new facts which centred on semi-religious platonic and neo-platonic ideas. Copernicus, it should be recalled, was tutored in Bologna under the Platonist Novara. The origin of this idea is linked to the sixth book of Plato's Republic where it is stated that the sun plays similar role in the realm of visible things as does the idea of the good in the realm of ideas. And as it is known in the Platonic hierarchy of ideas the idea of the good is the highest. Thus "the sun which endows visible things with their visibility, vitality, growth and progress, is the highest in the hierarchy of the visible things in nature",52 if it is to enjoy a pride of place, and retain divine status.

Kant reasons that we confront nature with hypotheses and demand a reply to our questions, and that without such hypotheses, what we can only make are haphazard observations that follow no plan and which can, therefore, never lead us to natural law. On space and time, he maintains, as Broad remarks,

51. Ibid. p. 186.

52. Ibid. p. 187.

that,

> they are in some sense subjective, i.e. that spatial and temporal characteristics are in some sense imposed by human percipients on the objects which they perceive. That our cognition of space and time is not merely conceptual or discursive, but is intuitive ... 53

Kant debunked the Baconian myth that we must begin with observation so as to derive our theories from the same. He believes equally that it is logically impossible to derive theories from observation.

He avers that,

> a science cannot be formed technically, that is from observation of the similarity existing between different objects, and the purely contingent use we make of our knowledge in concrete with reference to all kinds of arbitrary external aims; its constitution must be framed on architectural principles, that is, its part must be shown to possess an essential affinity, and be capable of being deduced from one supreme and internal aim or end, which forms the condition of the possibility of the scientific whole. 54

He believes that it is impossible for anyone to fashion out a science in the absence of some ideas as his basis of operation.

Kant reasons too, that our intellect does not derive its laws from nature, but it rather imposes same on nature. Popper considers this view a bit extreme, and as such modifies it. His own belief is that "our intellect does not draw its laws from nature, but tries with varying degrees of success - to impose upon nature laws which it freely invents."[55] The difference

53. C. D. Broad Kant, An Introduction, London, Cambridge University Press, 1978. p. 16
54. Kant, p. 472
55. Ibid. p. 191

here is obvious. Kant's line of reasoning is that our reason attempt not only to impose laws upon nature, but that it actually succeeds in doing this; from this assumption, Kant believes that Newton's laws were successfully imposed upon nature by us: that we were bound to interpret nature by means of these laws. He concludes from this that they must be true a priori. This is the way Kant sees the situation.

However since the time of Einstein, it has been noted that very different theories and very different interpretations are possible which may even be superior to Newton's. Thus, reasons is capable of several interpretations. Its interpretation is incapable of being imposed upon nature once and for all time.

> Reason works by trial and error. We invent our myths and our theories and we try them out. We try to see how far they take us. And we improve our theories if we can. The better theory is the one that has the greater explanatory power: that explains more; that explains with greater precision, and that allows us to make better predictions. 56

Popper does not believe that there is ultimacy in any theory. He believes that scientific endeavours do not actually need that. The only way we make progress is by trial and errors, substituting one theory that is weak with a stronger one that can withstand vigorous tests. He too believes as Giedymin points out that

> scientific hypotheses and theories though not verifiable by observation, can clash with reality,

56. Ibid. p. 192.

> provided they are falsifiable, experiments and observations though unable to establish any theory can eliminate some as false. 57.

The issue of the ultimate source of our knowledge, Popper sees as misplaced. Such issues are not relevant in the quest for knowledge. Rather than bothering about the sources of our knowledge, we should strive to detect and eliminate errors from our system.

However,

> we can ... say that a statement 'a' gets nearer to the truth than a statement 'b' if and only if its truth content has increased without an increase in its falsity content. 58

Kant is carried away by what he thought was the uniqueness of Newton's theory which we have to explain. Moreso, he was convinced that this theory followed inescapably and with logical necessity from the laws of our understanding. Popper's modification of Kant's solution in compliance with Einsteinian revolution unchains us from this compulsion. In line with this are seen to be,

> the free creations of our minds, the result of an almost poetic intuition, of an attempt to understand intuitively the laws of nature. On the contrary, we question nature as Kant taught us to do and we try to elicit from her negative answers concerning the truth of our theories. 59

In all these procedures, there is no attempt on our part to prove or verify our theories. What we do in actual fact is to carry out tests aimed at refuting our theories. By so doing, we are then showing critical approach to the

57. Jercy Gledynim "Antipositivism in Contemporary Philosophy Social Science and Humanities" in British Journal for The Philosophy of Science, 1977, Vol. 28, p. 376
58. Popper. The Open Society And Its Enemies. Vol. 4, p. 376.
59. Popper, Conjectures and Refutations, p. 192

study of science. It is by so doing that we will be on the way to achieving our set goal. We will then be able to establish that theories cannot be logically derived from observations, but they can clash with them (observations) and possibly contradict them. Through this, we shall be able to know and derive from observations, that a theory is false. Thus "in Popper's methodology the emphasis is on corroboration - appraisals of theories," says Watkins[60]. He sees the possibility of refuting theories as a necessary factor in empirical tests. That is in our tests of theories we should endeavour as much as possible to show that the assertion we are testing is falsifiable and must therefore be refuted. He dismisses as misconceived the attempts to qualify our theories with high degree of probability.

A theory can only explain this or that, we can see it as having been severely tested and that it has stood up to our tests. Two theories can, in this vein, be compared to find out which of the two can be corroborated by the outcome of the tests we embarked upon. On experience, Popper posits that by logical analysis, it could be shown that "it does not consist in the mechanical accumulation of observations."[61] Experience can be seen as something that is creative. It is a product of free, bold and creative interpretations that is characterised by severe criticisms,

60. J.W.N. Watkins, Metaphysics and The Advancement of Science " British Journal For The Philosophy of Science, 1977, Vol. 28, p. 107.

61. Popper, Conjectures and Refutations, p. 193.

as well as severe tests. We can see so far as stressed by Bunge that

> philosophically, Popper's view is the doctrine of learning from experience as a special case of learning from mistakes, of the critical method. ... Historically, it opens wide vistas of new studies of the history of science uncharted by the modern text-book. 62

Observation in most significant rules in our scientific activities is to enable us in the critical examination of our bold conjectures, by which we are able to probe into the unknown. Intuition and reasoning play similar role. They help us to solve whatever scientific problems we are faced with. And Chalmers emphasizes this point when he says that "... we learn from our mistakes by proposing bold conjectures or guesses as solutions to our problems ... testing and eventually refuting them."[63] Any system that does not obey this, Popper believes, is unscientific.

Part of the rational image of scientific research is objectivity. Our personal feelings and prejudice should not affect our research on scientific findings. These personal feelings of ours will render our efforts useless. This leads us to a full discussion of the objectivity of Science.

5. POPPER ON SCIENTIFIC OBJECTIVITY

Objectivity in science is an issue that requires a thorough attention. The term has suffered many contradictory

62. Joseph Agassi "The nature of scientific problems and their roots in metaphysics" in The critical approach to science and philosophy, Edited by E. Bunge, p. 198
63. A. F. Chalmers "On learning from our mistakes" The British Journal For the Philosophy of Science, 1973. Vol. 24, p. 164

usages to the extent that the purpose it is meant to serve becomes not very clear. Popper however uses the term almost in the same sense Kant used it; and which:

> indicate that scientific knowledge should be justifiable, independently of anybody's whim: a justification is 'objective' if in principle it can be tested and understood by anybody. 'if something is valid' ... then its grounds are objective and sufficient. 64.

Popper believes, however, that scientific theories are never fully justifiable or verifiable but are testable. It was in line with this belief that he views the objectivity of scientific statements in the light of the fact that they can be inter-subjectively tested.

Inter-subjective testing is an aspect of all the all-embracing idea of inter-subjective criticism or what may be called 'mutual rational control by critical discussions.[65] Kant's conception of subjective has common features to our feelings of conviction. That is the state of one weighing his ideas and accepting them. It is nonetheless within the field of psychology to examine how these feelings came about. "They may arise, for example, in accordance with the laws of association."[66] Objective reasons may also play the role of 'subjective causes of judging' in as much as we can have a reflection upon these reasons and become convinced of their authenticity. Kant, Popper remarks, seems among the very first to notice that the objectivity

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64. Popper, The Logic of Scientific Discovery, p. 44.

65. Ibid, p. 44.

66. Ibid, p. 45.

of scientific statements has a close affinity with the construction of theories - with the use of hypotheses and universal statements. When there is a repeated occurrence of events in obedience to rules and regulations, it creates the chance for our observation to be tested in principle, by anyone, just as we have in repeatable experiments. That we even take our own observation seriously is simply because there is the possibility for a repetition. It is only through this kind of repetition that we will be able to convince ourselves that we are not dealing with a mere isolated 'coincidence' but with events which on account of their regularity and reproducibility are in principle inter-subjectively testable.

A researcher or an experimenter in the field of physics will once in a while be exposed to some surprising and inexplicable 'effects' which in his laboratory can perhaps be reproduced for sometime but which ultimately disappear without trace. Given this kind of situation, no physicist can say he has made a new scientific discovery (though he still has the chance to rearrange his experiments so as to make the effect reproducible).
What could be properly seen as scientifically significant effect is that which can be regularly reproduced by anyone that carries out the right experiment in the way prescribed. That is there should be laid down rules and regulations through which a given theory or discovery is to be tested before such is to be taken as genuine. There is no physicist who deserves to be called that, who can offer for publication

as a scientific discovery any 'occult effect' - one that he cannot give reasonable guidelines as to how to reproduce them. Such a 'discovery' can only be dismissed as chimerical, unscientific as nothing other than negative results should be derived from its tests. It is for metaphysics and not science to decide whether a unique event that cannot be repeated can actually occur.

Our feelings or convictions are just not enough to justify a statement, no matter its degree of intensity. Scientific statements are not accepted simply on the basis of feelings or personal convictions. Feelings or convictions are at variance with scientific objectivity.

> Even the fact, for me so firmly established that I am experiencing this feeling of conviction, cannot appear within the field of objective science except in the form of a psychological hypothesis which, of course, calls for inter-subjective testing: from the conjecture that I have this feeling of conviction the psychologist may deduce, with the help of psychological and other theories, certain prediction about my behaviour, and these may be confirmed or refuted in the course of experimental tests. 67

One's feelings may be too strong about a statement, such could emanate from an irresistible impression of indubitable certainty (or self-evidence) yet such cannot constitute basis for the justification of scientific statements. Relying on such feelings or convictions cannot provide us any solution to empirical scientific problems. They lack merits in scientific methodology of acceptance or rejection of statements.

It has to be noted however, that the insistence on

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67. Popper, The Logic of Scientific Discovery, p. 46.

our requirements that scientific statements must have to be objective implies that those statements which belong to the empirical basis of science must also be objective - that is being able to pass through inter-subjective testing. And the implication of intersubjective testability is that testing in science is unending. From the statements that have been tested, other testable statements can be deduced. This means that science is bereft of ultimate statement, and there is no statement which cannot in principle be refuted. On this ground, Popper asserts as Stove points out that "the history of science is a succession of 'problems' conjectures and refutations." 68 The fact that there is no end to problems approachable through scientific methodology means that there is no end in the evaluation of solutions provided. As a matter of fact, one problem takes us to another, just as one solution becomes less valuable at the passage of time. Asike stresses this point thus "our beliefs are replaced by competing theories by competing conjectures. And through the critical discussion of these theories, we can progress." 69

Popper's emphasis on testing is meant to highlight the need for every statement of science to be capable of being tested; that we do not have such statements in science that we have entirely accepted as true simply because it does not seem

68. David Stove, Popper and after, four modern Irrationalists, (London, Pergaman Press, 1982) p. 13

69. J. I. Asike, "Scientific facts and their Theoretical Frameworks" in Uche, Journal of the Department of Philosophy 1984, Vol. 8, p. 40.

possible for logical reasons to test them.[70] The same situation applies to theory, no theoretical system is capable of enjoying an absolute acceptance beyond refutation. And he, in fact, believes that it would be a sad ending of scientific endeavour, as Kneale stresses, "if mankind ever came to permanent acceptance of a single theoretical framework"[71] The issue of demarcation between science and non-science takes us to the problem of method of science. If science is demarcated from non-science it follows that its method must be different.

6. POPPER ON THE QUESTION OF METHOD OF SCIENCE

Science as an empirical discipline has a method no doubt. Its approach to issues differs from what obtains in the liberal Arts and in metaphysics. It is the need to emphasize the methodology of science that informed its demarcation from metaphysics. It is imperative that the method of scientific discovery be stated as clearly as possible. With such clarity, the knowledge of a scientist should not be a closed affair, but instead open up for whoever wants to evaluate its veracity for possible use in tackling a given problem. Thus, whatever rules are adopted, whatever principle is applied in scientific discovery, must be in principle testable. It is only obedience to this that bestows scientific status on such research and discovery.

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70. Popper, The logic of Scientific Discovery, p. 48

71. William Kneale, "Scientific Revolution for Ever?" in the British Journal for the Philosophy of Science, 1969, Vol. 19, p. 27.

We shall be concerned in this sub-section about what the rules of scientific method are, why they are strongly needed by the authentic researcher. The approach to this issue lies essentially on one's conception of what science is. Those that see empirical science, like the positivists, as a system of statements that satisfy certain criteria such as meaningfulness or verification will offer one answer. And their answer will revolve around the fact that a scientific system must not have attributes of metaphysics, in addition to passing the tests of verification. A different answer will be offered by the set who along with Popper see the fundamental feature of empirical statements

> in their susceptibility to revision - in the fact that they can be criticised, and superseded by better ones; and who regard it as their task to analyse the characteristic ability of science to advance, and the characteristic manner in which a choice is made in crucial cases, between conflicting system of theories. 72

Popper harps on the need for a purely logical analysis of theories - an analysis which takes no account of how they change and develop. The fact remains, however, that this kind of analysis does not reveal that aspect of empirical science that rates so highly. Classical mechanics, for instance, is scientific to a great extent, but this does not preclude the system from being criticised. It is to be remarked too that no conclusive proof of a theory can be produced. To insist on strict proof in the empirical sciences denies one the benefit of experience, and precludes one from

72. Popper, the Logic of Scientific Discovery, p. 50.

learning from it how wrong one is. In fact, to Popper, one of

> the things a philosopher may do,and one of those that may rank among his highest achievements, is to see a riddle, a problem, or a paradox not previously seen by anyone else. This is an even greater achievement than resolving the riddle. 73

Besides advocating that we should not insist on strict proof in the empirical sciences, Popper adds that if one uses formal or logical structure of empirical science, to characterize it, one will end up being unable to isolate from it that existing form of metaphysics that results from elevating an outdated scientific theory into an indisputable truth. It is partly for this reason that there should be insistence that empirical science must have to be characterized by its method. It is the duty of a philosopher to examine the norms that guide a scientist when he is engaged in research works. When his procedures are noted, then it will be possible to state in very clear terms whether or not his research is in conformity with scientific methodology. Popper essentially believes that it is from problems that science and scientific research emanate. At any point in time, we are disappointed in our expectations and hope, we are submerged into more and more crucial problems. Our theories could lead us to much difficulties, they could create contradictions resulting from clash - either within a theory or two different theories, or between our theory and observation. The whole or either of the above issues take us deep and deep into scientific works and research. If we persist

73. Popper, Conjectures Refutations, p. 184.

in sourcing solutions for them, we create more opportunities for more awareness, for new experiences and invariably achieve an advancement in scientific knowledge.

7. THE NATURALISTS AND THE ISSUE OF METHOD

The positivists, as earlier on noted, were hostile to any knowledge that possesses metaphysical feature. By such stance, they were opposed to anything philosophical. They were consumed by the view that philosophical theory is incapable of tackling issues within the field of 'positive' empirical science. In the same reasoning, a positivist does not believe that there is a genuine theory of knowledge, an epistemology or a methodology. He sees all the so-called philosophical problems as pseudo problems or puzzles. Anything outside the empirical science the positivist believes, is mere nonsense. He sees the development of modern logic as instrument to tackle the question of metaphysics once and for all. And in the view of Rudolf Carnap "... logical analysis reveals the alleged statements of metaphysics to be pseudo-statements."[74] The positivist is in agreement with Kraft's contention that "no meaning can be given to that which is not reducible to experience..."[75]

It has already been stated that the positivists stance on meaningfulness and meaninglessness lacks basis. If meaningful-

74. Rudolf Carnap, "The Elimination of Metaphysics Through Logical Analysis of Language," p. 60.
75. Victor Kraft, The Vienna Circle, p. 33.

ness is restricted only to Natural Science's problems, then any debate about the concept of meaning will also turn to be meaningless. It is the view of Popper that if we enthrone the dogma of meaning as a criterion of evaluating science such unwarranted elevation will forever be problematic. Such is capable of creating a nuisance of values to the extent that the rational discussion in ways of acquiring knowledge will become difficult.

Popper believes that the existence of adhoc movements such as that of the Positivists' have no doubt threatened and questioned the existence of genuine philosophical problems. They have often confronted its study with such terms as "meaningful", "positive", "empirical" and "science". To the positivists in particular, whatever does not belong to the empirical science has no sense at all. Their conception of experience is not as a problem but as a programme. They believe that only two kinds of statements exist - logical tautologies and empirical statements. Where a methodology fails to be logical, their conclusion is that it must be a branch of the empirical science - science, say of the behaviour of scientists at work.

> This view, according to which methodology is an empirical science in its turn - a study of the actual behaviour of scientists, or of the actual procedure of 'science' - may be described as naturalistic. 76

It is worth remarking that the Naturalistic Methodology that is often called 'inductive' theory is not without some values, according to Popper. A student of logic no doubt could learn

76. Popper, The Logic of Scientific Discovery, p. 52.

quite a lot from it. He (Popper) nonetheless made it abundantly clear that what he perceives as methodology should not be taken for an empirical science. He does not believe that the application of empirical science methodology can solve such controversy as to whether science actually uses a principle of induction or not. His doubt on this is strengthened by the fact that what is to be seen as science and who is to be called a scientist will always remain a matter of convention or decision.

A question of this nature is to be approached differently. For instance, we could juxtapose two different systems of methodological rules, one with and one without a principle of induction. Having done that, we then evaluate whether the introduction of such principle can be applied without giving rise to inconsistencies, and see the much help we get from it and be able to know how needful it is.

The opinion of Popper as far as inductive principle is concerned is that it is needless, helpless, in addition to its leading to inconsistencies. He, in line with this stance, rejects the naturalistic methodology as it favours dogmatism. What its advocates are blind to, is that "whenever they believe themselves to have discovered a fact, they have only proposed a convention. Hence, the convention is liable to turn into a dogma." [77]
Thus, Popper rejects its criterion of meaning, its idea of science as well as its conception of empirical method which,

77. Ibid, p. 53.

Mcevoy says is "simultaneously 'naturalistic', 'psychologistic', 'inductive' and 'dogmatic'"[78] In its place, Popper advocates for a methodology that combines the insights of the philosophy of conventionalism that is followed by criticism.

He sees the game of science as continuous. At any point in time that a scientist sees a scientific statement as final and absolute (that cannot be challenged) is the time he retires from the game of science. But if a hypothesis proved its mettle after being proposed and tested, it may not be allowed to drop out without a good reason. A good reason here implies the emergence of a better hypothesis to replace the earlier one proposed. Another good reason is the falsification of one of the consequences of the hypothesis. The above points are what methodological rules are all about.

Methodological rules have close affinity with our criterion of demarcation. The connection is, however, not a purely deductive or logical one.

> It results rather from the fact that the rules are constructed with the aim of ensuring the applicability of our criterion of demarcation; thus their formulation and acceptance proceeds according to a practical rule of a higher type. 79

We cannot find a profound truth in methodology. This is, however, not to say that we should not endeavour to clarify the logical situation and attempt to solve far-reaching problems we are confronted with in our research. There should

78. John G. Mcevoy "Discussion, A Revolutionary Philosophy of Science. Fayerbend and the Degeneration of Critical Rationalism into Sceptical Fallibilism" in The British Journal of the Philosophy of Science, Vol. 42, No. 1, March, 1975, p. 52.

79. Popper, The Logic of Scientific Discovery, p. 54.

always be an aim and effort to reach our set goal no matter how difficult doing so may be. With our demarcation principle, We are sufficiently equipped to place problems where they rightly belong and prepare to offer solutions. He agrees that "definitions are dogmas; only the conclusions drawn from them can afford us any new insight." [80] And as far as scientific concepts are concerned, the above assertions are just real. What we are interested in mostly are the consequences of definitions. They are of immense assistance to us to detect inadequacies in older theories of knowledge, and to trace them back to the fundamental assumptions and conventions from which they spring. This method through which we are able to both detect and resolve contradictions, though of particular importance to the theory of knowledge, is equally very relevant to the field of science. A theory that is beset with contradiction lacks information to give in the quest for knowledge. And immediately a theory shows signs of contradiction, a researcher has no better option than to abandon it as unfit in scientific system. And because of the uncertainty surrounding our concepts of the world, the fact that none is so valid as to be beyond refutation, Popper urges for continuous tests of our ideas as he believes as Asike points out that "Our view of the world ... is at any moment necessarily theory impregnated." [81]

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80. Ibid, p. 55.

81. J.I. Asike P.40 [illegible]

Such views are tested by exposing them to critical evaluation.

Simplicity is another issue that is readily mentioned in scientific research. A simple and precise theory seems more preferable to complex ones. What are the issues involved in simplicity as a concept in science? We should highlight this carefully in the next sub-section.

8. SIMPLICITY

Simplicity, as an important terminology in the discipline of science, especially philosophy of science, has not really received enough attention it deserves which will no doubt put its uses into correct perspective. Its uses in the qualification of theories cannot afford to be overlooked. And theories constitute the bedrock for the approach of the discipline of science. And our preference of theories for the description of the world to singular statements is for the presupposition that theories are simpler than singular statements. Ironically, there have not been genuine efforts by anybody to find out where this simplicity lies and why. If it is a truism that theories are to be made use of for the fact that they are simpler, it follows then that we have to use the simplest theories.

As a concept simplicity no doubt, could be made use of in different ways. For instance, a given theory could be said to be of great simplicity in a methodological sense, but when it is looked at from a different perspective, it could become complex. In the same veins, it could be said that the solution to a given problem is not simple but difficult; or an exposition of presentation could be seen as not simple. However, Popper "excludes the uses of simplicity to anything like a presentation or an exposition."[82] In Mathematics, for instance, in the exposition of two Mathematical proofs, one could be seen to be simpler than the other, or more elegant than the other. In the field of theory of knowledge such distinction has minimal significance. Also, it does not fall within the province of logic. What it demonstrates is simply a preference of an aesthetic or pragmatic character. A similar situation manifests when people say that one task is carried out by simpler means than the other. What could be understood from this assertion is that there is a simpler way of doing it, or in order to do it, less training or less knowledge is needed. In all such cases, the word 'simple' can be easily eliminated: its use is extralogical.

8.1 THE METHODOLOGICAL PROBLEM OF SIMPLICITY:

What do we have after the elimination of the aesthetic and the pragmatic ideas of simplicity? Does a logician have any interest in any concept of simplicity? Can we possibly distinguish theories that are logically not equivalent according to their degrees of simplicity? Getting at the roots of these problem is not quite easy. Schlick gave a negative answer to the problem. He conceives simplicity as

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82. Popper, The logic of Scientific Discovery, p. 137.

"concept that demonstrates preferences that are partly practical and partly aesthetic in character."[83] His answer was obviously in response to what Popper calls 'the epistemological concept of simplicity'. He further stated that even in the event of our not being fully aware of what simplicity is all about, we still have to note that any scientist who succeeded in representing a series of observations by means of a very simple formular (e.g. by a linear quadratic or exponential function) is immediately convinced that he has discovered a law.

Schlick explores the possibility of making a distinction between law and chance using the concept of simplicity.

He dismisses this with the remarks that

> simplicity is obviously a wholly relative and vague concept, no strict definition of causality can be obtained with its help; nor can law and chance be precisely distinguished. From this passage, it becomes clear what the concept of simplicity is actually expected to achieve: it is to provide a measure of the degree of lawlikeness or regularity of events. 84

As an epistemological concept, simplicity plays a significant role in the theories of inductive logic. The proponents of inductive logic posit that we arrive at natural laws by generalization from particular observations. And according to modern usage as Pap points out

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83. Ibid. p. 137

84. Ibid. p. 138.

> ... inductive inference ... is an inference whose conclusion is not claimed to follow necessarily but only with some degree of probability; hence inductive inference is commonly used inter-changeably with 'probable inference'. 85

When confronted with series of problems, it is common to make attempt to choose the one that could bring out the quickest answer; that is choosing the simplest problem which we could find immediate solution to. For instance, Wittgenstein believes that "the process of induction entails an assumption of the simplest law that can be made to harmonize with our experience." 86

As a matter of fact, induction involves movement from one stage to another stage. And the various stages involved entails observations of facts over a period of time from where theories are promulgated based on our observations. The Inductivists generally believe, as Kemeny remarks, that one

> ... cannot check a general law directly; you must first ask what it tells you about particular facts. You cannot observe that the sun rises everyday throughout eternity; what you can observe is that it rises today and that it rises tomorrow, and the next day, etc. 87

In physics, for instance, it is most often the case that a general law is established on the basis of some single events. If a physicist, for example, can determine a certain physical constant like the heat-conductivity of

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85. Arthur Pap An Introduction to The Philosophy of Science (New York, The Free Press of Glencoe, 1962) p. 14
86. Popper The Logic of Scientific Discovery, p. 138.
87. John G. Kemeny A Philosopher look at science (New York, D. Van Nostrand Company, 1959) p. 86.

a sample of some pure metal, in a single experiment, he will believe that on other occasions, that object similar to the first sample will very likely be characterizable by the same constant. It is the principle of induction that is at work here. Carnap made this point thus:

> As a result of many previous observations the physicist is in possession of a universal sentence of a high order which enables him in this case to follow an abbreviated method. 88

In all scientific investigations, there is always the tendency to choose simplest law, it is usually tacitly assumed that a linear function, for instance, is simpler, than a quadratic one, a circle simpler than an ellipse. Most times reasons are hardly given for the choice of simplest laws over the others.

Popper, as a matter of fact, does not attach the slightest interest to the word 'simplicity'. He did not introduce the concept nor was he unaware of its shortcomings All that he did in the issue of simplicity was to reason that the concept of simplicity, if clarified, assist to answer those very questions that have so often been raised by Philosophers of science in issues of choice of theories.

8.2 SIMPLICITY AND DEGREE OF FALSIFIABILITY

There are a number of epistemological questions emanating from the concept of simplicity. If this concept is equated with the degree of falsifiability, it will be

88. Rudolf Carnap, "Psychology in Physical Language In Logical Positivism", p. 69

possible to attend to all such questions. It has already been noted that theories of a lower dimension are more easily falsifiable than those of a higher dimension. A law that has the form of a function of the first degree, for instance, is more easily falsifiable than that expressible by means of a function of the second degree. We can yet classify the latter as belonging to the best falsifiable ones among the laws that have Mathematical form of an algebraic function.

As earlier remarked, the degree of universality and precision of a theory increases with its degree of falsifiability. The more precise and universal a theory becomes, the more falsifiable it is.

> Thus we may perhaps identify the degree of strictness of a theory - the degree as it were to which a theory imposes the rigour of law upon nature - with its degree of falsifiability, which shows that the latter does just what Schlick ... expected the concept of simplicity to do.89

The examples earlier examined in respect of the degree of testability can be applied to the problem of simplicity. This is more real for the degree of universality of a theory: a more universal statement can take the place of many less universal ones. Because of this, it has often been seen as simpler.

Simplicity, as a concept, is in actual fact highly desirable. When one is really after the acquisition of knowledge, it is expected that simple statements should be

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89. Popper, *The Logic of Scientific Discovery,* p. 141

more highly sought after, than less simple ones. This is because we are informed more from it which is basically due to the fact that its empirical content is greater and they are better testable. And this is a good riddance evaluation of theories. For, as Radnitzky emphasizes, "... the more 'daring' the theory, the greater its potential for corroboration by tests."[90]

8.3 CONVENTIONALISM AND THE CONCEPT OF SIMPLICITY

Popper and the conventionalists view simplicity from diverse angles. There is no agreement of idea between them (Popper and the conventionalists) as far as the issue of simplicity is concerned. The crucial point for the conventionalist and invariably, their basic assumption, is that no theory is unambiguously determined by experience. Popper did not raise any objection to this. The conventionalist reasons further that the simplest theory must be chosen. To a conventionalist, the idea of simplicity differs in a very significant way from the issue of degree of falsifiability, since his theory is not treated as a falsifiable system. To him, as Lakatos says, "... false assumptions may have true consequences: therefore false theories may have great predictive power."[91]

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90. Gerard Radnitzky "Popperian Philosophy of Science as an antidote against relativism" in essays in memory of Imre Lakatos, Edited by R. S. Cohen, P. K. Feyerabend and M. W. Wartofsky, p. 527

91. Imre Lakatos "History of Science and Its Rational Reconstructions" in method and appraisal in the physical sciences, the critical background to modern science, 1800 - 1905, Edited by Colin Howson, p. 5

On the simplicity issue, the conventionalist concept of it turns out to be partly aesthetic and partly practical. Thus the comments of Schlick that "the concept of simplicity can only be defined by convention that is arbitrary"[92] applies only to the conventionalists and not to Popper. Popper believes it was regretable that the conventionalists did not demonstrate much interest in the issue of simplicity, and instead choose the way of arbitrary convention. He reasons further that

> a system must be described as complex in the highest degree, if in accordance with conventionalist practice, one holds fast to it as a system established forever which one is determined to rescue, whenever it is in danger, by the introduction of auxillary hypotheses, for the degree of falsifiability of a system thus protected is equal to zero.[93]

Popper is in essence advocating against the methodology that makes use of adhoc and auxillary hypotheses which conventionalists advocate to save the falsification of a theory. Such methodology is clumsy and unscientific, and these are at variance with Popper's methodology that is clear-cut. And as Lakatos remarks "the great attraction of Popperian methodology lies in its clarity and force."[94] It is for this and other reasons that Popper rejects, in addition to induction, probability which he does not see as a method of science. Scientists he argues look for bold conjectures and not probabilistic statements as we shall see presently.

92. Popper, The Logic of Scientific Discovery, p. 145

93. Ibid. p. 145.

9. PROBABILITY

We should be here concerned with the probability of events and the problems associated with it. We shall witness in the course of the study the theory of games of chance as well as the probability laws of physics. As it is well known, ideas that have to do with probability are very crucial in the modern theory of physics. It is also a fact that we are yet to possess satisfactory axiomatic system for the calculus of physics. We also require clarification in the issue of probability and experience. We will notice as we progress in this section, what at first encounter seems a very strong objection to Popper's methodological views. We will see too that in spite of the significant role that probability plays in empirical science, it turns out to be in principle impervious to strict falsification. As a matter of fact, probability is identified says Settle, "... with the key criterion of rational betting or of decision making under uncertainty,"[95] The significance of probability in scientific system is due to shaken faith in science resulting from the overthrow of Newtonianism. As a result, instead of seeing a theory (scientific knowledge) as provable inductively beyond any reasonable doubt, there came the view that a theory was more or less acceptable as it was rendered more or less probable by the evidence that is available.

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94. Imre Lakatos "History of science and its rational reconstruction" in method and appraisal in the physical sciences: the critical background of modern science, 1800-1905 p. 7.

95. Tom Settle "Induction and probability unfused" in The Philosophy of Karl Popper Book 2, Ed. By Paul Arthur Schlip, p. 697.

This view, the equating of degree of acceptability with degree of confirmation and degrees of confirmation with degrees of probability was very seductive. 96

Popper was not impressed by this methodology and in fact, refuses to accept both induction and probability as scientific method. Harre, for instance, echoes Popper's view that " . . . The degree of corroboration cannot be equated with probability just as we cannot see 'The calculus of corroboration as one of the possible interpretations of the probability calculus." 97

9.1 INTERPRETING PROBABILITY STATEMENTS

By way of a preamble, two kinds of probability are to be evaluated. The first is that which sees probability in terms of numbers. And this we shall call numerical probability statements. The second one are those that do not see probability in terms of numbers. For instance, the assertion: 'The probability of throwing eleven with two (true) dice is 'I8' would be an example of a numerical probability statement. On the other hand, 'non-numerical' probability statements can assume diverse forms' such as, 'it is very probable that' we shall obtain a homogenous mixture by mixing water and alcohol' gives an insight into a kind of statement which suitably interpreted might perhaps be changed into a numerical probability statement (for example, 'The probability of obtaining ... is very near to 1'). In every numerical probability statement, the question

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96. Ibid. p. 69 7.

97. R. Harrē The Principles of Scientific Thinking, p. 175.

that could arise is how we can interpret a statement of this kind and in particular, the numerical assertion it makes'.

9.2 DECIDABILITY

Probability statement, as we have noted so far, can hardly be falsified, due to the wild assumption it makes and chances given for its validity. Equally, probability hypotheses do not rule out anything observable: probability estimates cannot contradict or be contradicted by a basic statement, nor can they be contradicted by a conjunction of any finite number of basic statements, and not by any finite number of observations either.

The infinite dimension of probability hypotheses is a very strong factor against its falsification. The tendency here is then to see probability hypotheses as empirically uninformative - void of empirical content, but certainly not void of logical content. Can this view be accepted in the light of all the landmark that physics has achieved with predictions obtained from hypothetical estimates of probabilities? Many of such estimates cannot be certainly seen as inferior in scientific significance to any other physical hypothesis - for example, to one of a determinist character.

Besides, a physicist is often quite well able to decide whether he may for the time being accept some particular probability hypothesis as 'empirically confirmed' or if he ought to reject it as practically falsified', that is, as useless for purpose of prediction. There is no ambiguity that this 'practical falsification' can be obtained through a methodological decision to regard highly improbable events as ruled out - as prohibited. The crucial questions, however, are: with what right can they be so regarded? Where are we to draw the line? Where does this 'high improbability begin?

The obvious point is that from a purely logical point of view probability statements cannot be falsified. That probability can be used empirically seems a big upset to Popper's basic idea of demarcation criterion between metaphysics and the empirical studies.

Popper insists however that probability cannot be used empirically. He sees it (probability) in the same fashion as speculative metaphysics, which discussion follows immediately.

9.(a) PROBABILISTIC SYSTEM OF SPECULATIVE METAPHYSICS

A significant use of a probability statement in physics is that certain physical regularities or observable effects are interpreted as 'Macro Laws' that is, they are interpreted or perceived as main phenomena, or as the observable results of hypothetical and not directly observable micro events. The macro laws are deduced by showing that an observation that agrees with the observed regularity in question are to be expected with a probability very close to '1' that is with a probability which deviates from '1' by an amount which can be made as small as we choose. Having shown this, we can then say that by our probability estimate, we have 'explained' the observable effect in question as macro effect.

If, on the other hand, we make use of probability estimates in this way for the explanation of observable regularities without introducing special precaution, then we may immediately become involved in speculation which in accordance with general usage can well be described as typical of speculative metaphysics. Just as probability statements are not falsifiable, it must always be possible in this way to explain by probability estimates any regularity we please. For instance, events of some kinds are selected to serve as elementary or atomic events; for instance, the movement of a small particle. What is to be as a primary property of these events are also selected; for instance, the direction and velocity of the movement of a particle.

Then there should be an assumption that these events show a chance - like distribution. At last, the probability is calculated that all the particles within a certain finite spatial region, and during a certain finite period of time, a certain cosmic period - will with a specified accuracy move accidentally in the way required by the law of gravity. It is expected that the probability calculated here will be very small, negligibly small, but yet should not be equal to zero. Then a question of how long an 'n'- segment of the sequence would have to be or the duration to be assumed for the whole process so as we may expect, with a probability close to 1, "the occurrence of one such cosmic period in which, as a result of an accumulation of accidents, our observations will all agree with the law of gravity will be raised."[98]

98. Ibid. p. 197.

. For any value as close to '1' as we choose, we obtain a definite though extremely large finite number.

> We can then say: if we assume that the segment of the sequence has this very great length - or in other words that the 'world' lasts long enough - then our assumption of randomness entitles us to expect ~~that~~ the occurrence of a cosmic period in which the law of gravity will seem to hold good, although 'in reality nothing ever occurs but random ~~a~~ scattering. 99

Explanation of this kind by means of an assumption of randomness is applicable to any regularity we choose. The totality of the cosmos could be explained this way "with all its observed regularities as a phase in a random chaos - as an accumulation of purely accidental coincidences." 100

Speculation of this kind, Popper sees simply as purely metaphysical. As a result of this, he feels they are of no significance for science. They cannot be falsified, and equally we can always and in all circumstances indulge in them. Thus, Popper posits that

> theories involving probability ... if they are applied without special precaution, are not to be regarded as scientific. We must rule out their metaphysical use if they are to have any use in the practice of empirical science. 101

Our question from the discussion so far is how probability hypotheses that have been seen to be non-falsifiable play the part of natural laws in empirical science? Popper's response to this question is that probability statements

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99. Ibid. p. 197

100. Ibid. p. 197

101. Ibid. p. 198.

so far as they are non-falsifiable are metaphysical, and without physical and without empirical significance and in so far as they are used as empirical statements they are used as falsifiable statements. From the above answer, another question can emanate which is: how is it possible that probability statements which are not falsifiable be used as falsifiable statement?

There is no doubt that they can be used in this way: the physicist knows well enough when to regard a probability assumption as falsified. There are two aspects to this question: foremost, we must make the possibility of using probability statements understandable in terms of their logical form. Secondly, we have to analyse the rules governing their use as falsifiable statements.

Besides examining the position of probability as far as empirical science is concerned, law and chance were the next issues that interested Popper as to their relationship in scientific research. What divides the two is very minute as we shall see in a moment.

9.4 LAW AND CHANCE

It is generally believed that the movement of the planets obeys strict laws, whereas the fall of a die is subject to chance. The true position really is that we have so far been able to predict the movement of the planets successfully. As to the individual result of throwing dice, we have not succeeded in doing the same thing.

To be able to deduce prediction, one needs laws and initial conditions. If there are no suitable laws available;

and if the initial condition cannot be ascertained, the scientific way of predicting breaks down. In throwing dice, what we lack is clear knowledge of initial condition. If it is possible for us to afford a precise measurement of initial condition it would be possible to make prediction in this case too. But the rules for correct dicing (shaking the dice box) are so chosen as to prevent us from measuring initial condition.

'Frame conditions' are what to be called the rules of play and other rules determining the conditions under which the various events of a random sequence are to take place. Among the requirements are that the dice shall be 'true' (made from homogenous material) and that they shall be well shaken, etc. There are several cases where prediction may be unsuccessful.

> Perhaps it has not so far been possible to formulate suitable laws, perhaps all attempts to find a law have failed, and all predictions have been falsified. In such cases we may despair of even finding a satisfactory law.[102]

It is only when the problem has not arrested our attention that we give up.

There are no such cases in which we can say that there are no laws in a particular field (this is a consequence of the impossibility of verification). The implication of this is that my view makes the concept of chance subjective, (Popper did not by this make any concession to a subjective interpretation of probability, or of disorder or randomness).

102. Ibid. p. 205.

We speak of 'chance' any way when our knowledge is not sufficient for prediction: that is we lack sufficient knowledge of initial condition. A physicist that is properly equipped with good instruments can afford to predict things that other set of people not similarly equipped cannot do.

We speak of laws when we are successful with our prediction. Such laws become a kind of guides for future action as far as the issue already predicted is concerned. The existence of laws make it possible for us to speak about regularities and irregularities. We encounter 'chance' in the objective sense, it could be said, when our probability estimate are corroborated, just as we encounter causal regularities when our predictions deduced from laws are corroborated.

> Popper called probability sequences, chance-like. In general, a sequence of experimental results will be chance-like if the frame condition which defined the sequence differ from the initial conditions; when the individual experiments carried out under identical condition will proceed under different initial condition and so yield different results. 103

Popper was in doubt if there are chance-like sequences whose elements are in no way predictable.

That a sequence is chance-like is not enough reason to say its elements are not predictable, or that they are 'due to chance' in the subjective sense of insufficient knowledge. The least we cannot derive from this is the 'objective' fact that there are no laws. We can never repeat an experiment precisely - all we can do is to keep certain

103. Ibid. p. 206.

conditions constant within certain limits. It is in essence no argument for objective fortuity or chance or absence of law if certain aspects of our results repeat themselves, while others vary irregularly especially if the condition of the experiment is designed with a view to making condition vary.

> Not only is it impossible to infer from the chance-like character of the sequence anything about the conformity of law, or otherwise of the individual events: it is not even possible to infer from the corroboration of probability estimates that the sequence itself is completely irregular; for we know that chance-like sequence exist which are constructed according to Mathematical rule. 104

In the success of probability prediction we must see no more than a symptom of the absence of simple laws in the structure of the sequence as opposed to the events constituting it.

We have earlier on noted that a statement that is falsifiable to a higher degree than another statement can be described as the one which is logically more improbable; and the less falsifiable statement as the one which is logically more probable. The logically less probable statements entail the logically more probable one.

Popper emphasizes that it is the lot of science to aim at logically less probable statement. The more highly probabilistic a statement is the more it distances itself from science and tends towards metaphysics. Popper also emphasizes as Kneale points out that

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104. Ibid. p. 206.

> the degree of confirmatione or corroboration attained by a scientific hypothesis should always be distinguished snarply from probability discussed in Mathematics textbooks, i.e. from probability in that sense in which we may be said to give the probability of a proposition when we state what chance there is of its being true. 105

This idea agrees with the view that it is more appropriate to speak of the acceptability of scientific hypotheses rather than of their probability. And a scientific hypotheses that had survived rigorous test according to Popper is that with a low level of probability. Thus a scientist always aims at ensuring that the probability of his hypotheses is, if possible, brought down to zero level.

It is seen in essence that Popper links "the high acceptability of a successful scientific hypothesis with its low absolute logical probability."[106] In maintaining this position, he believes he has noted what emphasizes a remarkable difference between laws of nature and laws of logic. In his view,

> laws of logic,,unlike supposed laws of nature can be taken as certain precisely because they have maximum logical probability and null content, i.e. are not open to refutation by experience. 107

He believes the less a statement says the greater its probability. "Which is to say that every interesting and forceful statement must have a low probability."[108]

105. William Kneale, "On Popper's Use of the Notion of Absolute Logical Probability" In the Critical Approach to Science and Philosophy in Honor of Karl Popper, p. 145
106. Ibid. p. 145
107. Ibid, p. 145
108. [illegible] Popper, –Conjectures and Refutations, p. 58

He argues further that every hypothesis of natural laws possesses an absolute logical probability of '0'. And capacity for corroboration remarks Kneale, is the same as falsifiability and "falsifiability is at its maximum when absolute probability is at its minimum.[109] Scientists, Popper says, are after high degree of corroboration and not highly probable theories. Highly probable theories say little as they are empty of concrete facts significant for scientific growth. It is the verificationists who champion the view of high probability for scientific theories.

> If you find that you cannot verify a theory, or make it certain by induction you may turn to probability as a kind of 'Ersatz for certainty in the hope that induction may yield at least that much. 110

A theory can be said to have explained this or that with a target it aims to tackle. We can also say that such a theory has been tested and that it withstood all the tests it has passed through. Two theories can also be compared so as to establish which of the two has withstood more of the tests they have passed through - that is to ascertain which of the two theories is better corroborated than the other bearing in mind the results of our tests.

> But it can be shown by purely Mathematical means that degree of corroboration can never be equated with Mathematical probability. It can even be shown that all theories, including

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109. William Kneale, "On Popper's use of the notion of Absolute Logical Probability" in The Critical Approach to Science and Philosophy in Honour of Karl Popper, p. 146.

110. Popper K., Conjectures and Refutations, The Growth of Scientific Knowledge, p. 58.

> the best have the same probability, namely zero. But the degree to which they are corroborated (which, in theory at least, can be found out with the help of the calculus of probability) may approach very closely to unity, i.e. its maximum, through the probability of the theory is zero. 111

Popper uses the thesis of 'content of a theory' or 'the informative content' to show that science aims at low if possible zero probability. He sees the informative content of the conjuction "ab", of any two statements "a", and "b", as being always greater than, or at least equal to that of any of its components. To illustrate this we can assume 'a' to be the statement, 'It will rain on Monday', and 'b' the statement 'There will be a fine weather on Tuesday', and then 'a.b' will be seen as the statement 'It will rain on Monday and there will be a fine weather on Tuesday. We will then see that of the last statement, the conjunction 'a.b' will be greater than 'a' as well as 'b'. It will then be beyond doubt that the probability of 'a' (that is the probability that 'a.b' will be true) will be smaller in comparison with either of its components.

This can be represented thus:-

ct (a) = "the content of the statement a"

ct (a.b) = "the content of the conjunction 'a' and 'b'"

Then we have

(1) ct (a) $\leq$ ct (a.b) $\geq$ ct (b)

The above is in contrast with the corresponding law of the calculus of probability.

(111) Ibid. pp. 192 - 193.

(2) p (a) $\geq$ p (ab) $\leq$ p (b) in which the inequality signs of (1) are inverted. These two laws (1) and (2) "state that with increasing content, probability decreases, and vice versa; or in other words, that content increases with increasing improbability."[112] The above analysis is completely in consonance with the general idea of the logical content of a statement as the class of all those statements that are logically entailed by it. It could be said too that a statement 'a' is logically stronger than a statement 'b' if its content is greater than that of 'b' - that is, if it entails more than ('b'.)

The consequences deducible from this is that if the advancement of knowledge entails our operating with theories of increasing content, it then means too that we have to operate with theories of decreasing probability. If it is accepted that our aim is the advancement of knowledge, then a high probability (in the sense of the calculus of probability) cannot just be our aim, the two (high probability and the advancement of knowledge) are not in agreement.

> ... the absolute probability of a statement 'a' is simply the degree of its logical weakness or lack of informative content... the relative probability of a statement 'a'. given a statement 'b' is simply the degree of the relative weakness, or the relative lack of new informative content in statement 'a' assuming that we are already in possession of the information 'b'. 113

Testability remains for Popper the only criterion for potential satisfactoriness or improbability. The theory that is worth testing is only highly testable or improbable ones.

112. Ibid. p. 218

113. Ibid. p. 219

And as Chalmers says

> A theory which is not refuted by testing new and bold and improbable predictions to which it gives rise can be said to be corroborated by these severe tests...114

The subjectivist theory of probability, Popper sees as a product of subjectivist approach to truth. A subjective theory obviously cannot fit into scientific structure since it cannot be refuted. There is always the tendency for us to uphold the view that whatever we say or print should be replaced by a 'belief-statement'. For instance, we replace the statement 'sugar is sweet' by 'I believe that sugar is sweet' or at times by,

> In view of the evidence available I believe it is rational to believe that sugar is sweet'. To replace assertions about the objective world by any of these circumlocutions is trivial according to Popper. He posits that ... the subjective interpretation of logical probability links these subjectivist replacements, exactly as in the case of the coherence theory of truth... 115.

The more a statement asserts, argues Popper, the less probable it is. This could be expressed thus: the logical probability of a sentence 'x' on a given evidence 'y' decreases when the informative content of 'x' increases. We can deduce from the above that a high probability can really not be one of the aims of science. The interest of a scientist is really on a theory that has high content. If scientists are to be interested in high probability,

114. A. F. Chalmers "On Learning from our Mistakes" In the British Journal for the Philosophy of Science Vol. 24, 1973, p. 166.

115. Popper Conjectures and Refutations, The Growth of Scientific Knowledge, p. 227.

Popper believes that they will say as little as possible and preferably, assert tautologies only. In contrast to this, a scientist aims to advance science, to add more contents to his theory, which is the same as lowering its probability. "And in view of the high content of universal laws, it is neither surprising to find that their probability is zero ..."[116] Thus science aims at lower probability according to Popper.

116 Ibid. P. 286

10. CRITICAL SUMMARY

Popper is opposed to induction as a method of science. He believes that science does not operate by inductive method which is a process of generalisation from particulars.

On the basis of this rejection he querries the rationale behind prediction, as it is very possible that the future may be quite different from the past. He does not see the basis for induction; even when experiments are carried out, there is no way we can ensure a repetition of the same process. He sees the belief in induction as an outcome of confusion between psychological and epistemological problems.

He sees deduction rather as an appropriate method of science. In deduction we proceed by carrying out various tests, from which conclusions emerge. After this we are in a good position to compare our theory with others. Induction, argues Popper, is riddled with contradiction.

On demarcation he attacks the views of the positivists who used the criterion of sense and senselessness to demarcate science and non-science. He does not see verification as a criterion of demarcation as advocated by the Positivists too. Science he argues, is a dynamic discipline which progresses by conjectures and refutations. Falsification, he says remains the criterion of demarcation.

Objectivity, states Popper, is a strong factor in science. A scientific statement should be free from personal feelings or conviction, no matter the degree of intensity. A scientific experience must be repeatable which makes such knowledge accessible to others.

Popper has a negative posture as far as probability is concerned, whose statements, are metaphysical since they cannot be falsified. For him Probability statements are empirically void and uninformative.

Popper's position on induction and generalisation is no doubt controversial. It is controversial because there seems to be a favourable view in the science community on induction and generalisation. Besides, the method of science follows inductive method more than anything else. It is really doubtful whether science would have attained the extent of progress it has attained if knowledge in science could not be generalised through induction. By this (generalisation by induction) willing individuals are able to have access to scientific knowledge.

Popper's position on induction is definitely not based on what the scientists do, but may be on what they ought to do. The discoveries made in science have been possible through the process of induction. It is through the examination of particular specimens that a generalisation is made. A scientist is not bothered to take and examine all the specimens of the objects of his enquiries before he could come up with a theory. Induction is in fact the most feasible way of procedure in scientific research. This method through which generalisation is made, make research in science to be cumulative, less cumbersome and unified. It also makes for easy reference and the avoidance of starting from the scratch in research.

For instance in the field of Biology we have Mendel's law on plants species. He made observation over a period of years on the variety of plants he grew in his small garden. From his long and meticulous observation he made generalisation concerning the proportion of various traits in the offspring as determined by the traits of the parents. Farmers all over the world have for years benefited from this experiment that centres on breeding. In the field of Physics, the principle of buoyancy' was established by Archimedes' particular observation that the amount of water that overflow when he stepped into his full bath was equal in volume to that portion of his body which was inserted into the bath. These two discoveries from which generalisations were made obviously stemmed from the principle of induction - from particular to general, generalisation thus become a special attribute of science. As Cohen rightly points out,

> where science goes further than technology is in generalization, beyond any particular or any 'typical' concrete local reality. Science taken as a whole, is distinguished from technology as from philosophy, by virtue of this universalizing understanding[117]

Bertrand Russell obviously taking side with this generalisation notion, posits that the business of science is to find uniformities, such as the laws of motion and the law of gravitation."[118]

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117. Cohen, p. 81.

118. Bertrand Russell, The Problems of Philosophy (New York, Oxford University Press, 1980) p. 35.

Any knowledge that has a general appeal commands more acceptance and dignity. From the generalisation it will be easy enough to make prediction in regards to the future behaviour of a given object that has been studied. This is a very salient point in science - the ability to predict an event on the basis of its features that have been noted and mastered in the past. For instance that water could be formed by burning inflammable gas in air as discovered by Cavendish, was a theory he arrived at after carrying out a number of experiments with inflammable gas notably Hydrogen. This has since become a general law in science and it could be predicted that at any point in time that inflammable gas is burnt that water would be formed. Similarly it could be predicted that at any point in time, "We treat hydrochloric acid with sodium hydroxide in water, the product is simply a solution of common salt in water"[119] If the water is evaporated the salt will be left behind as a white solid. All these predictions are possible as a result of generalisation made on the basis of inductive principles. As Elgin points out, "... Comprehensiveness, simplicity, elegance and predictive power are features we want our scientific theories to have"[120]. A theory that lacks predictive power is certainly unfit in the scientific structure.

119. Graham et al, p. 94.

120. C. Z. Elgin "Elgin "lawlikeness and the end of science" in Philosophy of Science, Official Journal of the Philosophy of Science Association, Vol. 47, No. I. March, 1980 p. 66.

Popper raised doubt as to the possibility of the future being like the past. It is quite in order to entertain this kind of doubt. But science cannot progress without this kind of prediction. There could be an exception to a rule no doubt, but that does not go to render the rule completely impotent. Science cannot do without prediction in spite of some shortcomings. At any point in time a given theory is seen to be defective nothing definitely stops a change from being made. As Elgin equally points out "New theories are developed from old ones on the basis of changes we make in the face of recalcitrant experience".[121] It is by so doing that we are able to take care of future developments and changes.

Popper's conception of deduction as a method of science raises a number of issues. If by deduction he meant the general understanding of the laws of nature through which science and scientists proceed in their research, the problems will be considerable. However in real scientific research and enterprise induction from particulars to general is the practice. The view of Popper that induction is riddled with contradiction is unacceptable. At any point in time a researcher sees contradiction in his research works he should discontinue the experiments and attempt to rectify the problem.

121. Ibid. p. 66.

Popper's demarcation thesis is another area of his methodology that elicits controversy. His attack on the Positivists use of meaningfulness and meaninglessness to discredit Metaphysics is well taken. But his uses of falsification as a criterion of demarcation raises a number of problems. This falsification seems to perceive science only in the negative. **Besides an empirical experience** may not be falsified and this does not preclude it from being scientific.

Even the issue of demarcation between science and metaphysics may not be completely successful. This is because there are several assumptions that are made in science which cannot be explained empirically. Such assumptions are nothing short of metaphysics. There cannot be a complete successful demarcation between science and metaphysics since the latter is the basis of the former. As Kant points out,

> Metaphysics ... forms properly that department of knowledge which may be termed, in the truest sense of the World, Philosophy. The path which it pursues is that of science, which, when it has once been discovered, is never lost and never misleads. Mathematics, natural science, the common experience of men, have a high value as means, for the most part to accidental ends - but at least also, to those which are necessary and essential to the existence of humanity. But to guide them to this high goal, they require the aid of rational cognition on the basis of pure conceptions which, be it termed as it may, is properly nothing but metaphysics. 122.

122. Kant, pp. 480-481.

If it is believed that there is an affinity between science and metaphysics as the above position of Kant shows and as buttressed by Descartes who posits that Metaphysics stands as the root of physics,[123] then a complete demarcation between science and metaphysics as purportedly done by Popper cannot be successful.

On the objectivity of science Popper seems to overemphasize the issue of testing which he sees as unending. Testing in science, we should note is not an end but a means to an end. Besides at whatever time scientists test their theories, its essence is to escertain if it still lives up to its original expectation, such action is not really meant for refutation of the same theory.

Problems, as noted by Popper, could no doubt, warrant a scientific research but it is not all research programmes that are generated by problems. Research has different purposes and origin. A research could be just a continuation of a normal assignment of a scientist, it could be to achieve a particular purpose, it could be a fulfillment of one's sense of calling (the joy one derives from actualising oneself), or even be accidental. Francis Bacon, for instance, believes that,

> ... the purpose of scientific research was neither to acquire fame nor to produce

123. Copleston, p. 78.

> miracles, but to improve the conditions of human existence, and he believed this could only be achieved by collaboration, the founding of adequate institutions and the publication of results in plain exact terms. 124

Bacon's idea as to the essence of research in science seems to agree with that of Popper, but it is to be noted that a research in science in addition to solving human problems, could be directed towards the acquisition of fame.

Probability is another controversial area paid attention by Popper. His position on this is in consonance with his conception of science as a dynamic discipline. But in mathematics and science probability constitutes an important and unavoidable factor. Popper believes otherwise. He sees probability as lacking empirical character - to him, it gives no information. The more empirical content a theory has, he argues, the less probable it becomes.

This position cannot be so. In science and mathematics there is a reasonable basis for the application of probability, for instance, if at a given temperature a given metal 'A' melts - and a collection of such metals do the same at the same temperature - then a law could be formulated that given so and so temperature that the probability of so and so metal melting is very high. There are provisions here for a necessary adjustment given that the set target is not realised. It is possible that a material with the same components as the one

124. Paolo Rossi, Francis Bacon, From Magic to Science (London, Routledge and Kegan Paul, 1956) p. 36.

that melted say at 98°C, may not do the same. It cannot be deduced from such failure that the law governing the melting of that metal is completely faulty, since a reasonable number of such metals have already done the same. And to take care of this exception, the probability clause is added.

In the field of medical science probability is very relevant, for instance, it is one of the established theories in the medical science that when a couple that possesses genotype 'AS' and 'AS' copulate that there is a high probability of their having issues afflicted by sickle Cells anaemia. For this theory to be accepted a number of tests were carried out and corroborated. But there can be an exception where persons of alleged 'AS' genes could copulate and have issues that are free from this disease. If this happens, it does not mean that this law has been completely faulted and should as such be dispensed with. If in every 10 cases examined, 8 suffered the predicted consequences the theory is still tenable and the probability is thus said to be high. Probability as used here is to take care of any exception that might take place.

Thus in science we talk about high probability than about absolute truth and certainty. A medical doctor may predict that a patient who has undergone 'heart-transplant' will die in about ten years. This prediction is not an empty one. It is an outcome of experiments, experiences from similar cases gathered over a period of time. But such a patient may die earlier or live longer than has been predicted. In essence, the much he can say given the uncertainty surrounding his prediction is that, in view of my experiences in the past with patients, that have undergone 'heart-transplant' and for the fact that their situation are exactly the same with Patient 'A' and the fact that such previous patient died ten years after the transplant, the probability that patient 'A' will die after ten years is very high. This prediction certainly has a basis-the experiences of the medical doctor - that the prediction will be realised is another issue that is beyond him.

Even the meteorologists cannot dispense with probability. After observing the weather, they can predict that the probability that there will be rain is high. This may not be after all, but their forecast has a foundation.

In the field of Agriculture, probability is also very useful. A farmer can predict that given up to 'X' acres of land and 'hybrids crops' and with a reasonable degree of rainfall, that the probability that he will produce not less than 'y' bags of rice is very high. And if the conditions he stated are fulfilled, he might achieve the target he had set for himself.

Thus probability notions are indispensable in science. It has nothing to do with absolute truth. As Harre' points out, "To say that a certain event is probable, is to say, with an escape clause, that it might or will occur. To say that some event was probable is to say that it was likely that it would have occured."[125] And as a matter of fact the kind of complex materials that scientists deal with do not give room for much certainty. We can imagine, for instance, the difficulty that a physicist has on his hand when he calculates the trajectory of a bullet fired by a gun. He has a number of issues to contend with, such as the direction of the wind as well as the moisture of the air. As a result of all these factors he can only as Reichenbach points out "predict the point where the bullet will hit only with a certain probability."[126] This kind of uncertainty makes probability inevitable in the sciences.

It is amazing that Popper had to reject both induction and probability which are crucial in scientific research, and yet he (Popper) advocates for the growth of science. [illegible] His rejection of induction and probability seem to contradict this position. And as Newton-Smith points out,

> if Popper genuinely abandons induction, there is no way in which he can justify the claims that there is growth of scientific knowledge. and that science is a rational activity. 127

Science cannot definitely thrive in the absence of the application of induction and probability.

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125. R. Harre' The Principle of Scientific Thinking (London, The Macmillan Press Ltd, 1972) p. 160.
126. Reichenbach , p. 164
127. Newton-Smith, p. 52.

CHAPTER THREE

FALSIFIABILITY AND CORROBORATION IN POPPER

In this Chapter we shall discuss falsification and corroboration in Popper's philosophy of science. We shall in doing so delve into a number of sub-titles. This include Popper's conception of theories which he believes are inevitable in scientific investigation. A good theory he believes should have a good foundation and be able to withstand crucial tests. We shall also look into Popper's thesis of dynamism in science on the basis of which he rejects induction.

Falsification which he believes is a procedure in scientific research will also be examined. Also to be examined is Popper's insistence on objectivity.

1. THEORIES

In the empirical science, theories are common features. They are methodologically formulated to embody one's ideas with which one intends to approach a given problem. Theories that lack strong foundations, which are incapable of providing solutions to issues at hand are discarded in search of more suitable ones. Thus, the logic of scientific research is seen by Popper as a theory of theories, and scientific theories are perceived as universal statements, and as is the case in linguistic representation, they are systems of signs or symbols. "Theories are nets cast to catch what we call 'the world': to rationalise, to explain and to master it. We endeavour to make the mesh ever finer and finer."[1]

The formulation of theories requires a lot of plan and care so as to ensure that its goals are realised.

1. Popper, The Logic of Scientific Discovery, p. 59.

Besides, theories are in stiff competition with one another to the extent that it is the strongest that survives. A good theory that has passed through a crucial test is that which takes us closer to the truth. And as Newton-Smith puts it "truth is the aim of science ... we are never entitled to claim to know the truth of scientific theory or hypothesis."[2] There is, however, the awareness that there is the truth: all our efforts are just to approximate it.

Popper harps on the issue of when a theory merits to be ranked as scientific, when it is to be accep as a way of tackling a given problem. A theory, he believes, should be capable of opening up new ideas, be able to make novel predictions. For instance,

> Einstein's gravitational theory had led to the result that light must be attrac by heavy bodies (such as the sun) precisely as material bodies were attracted. As a consequence, it could be calculated that light from a distant fixed star whose apparent position was close to the sun would reach the earth from such a direction that the star would seem to be slightly shifted away from the sun; or, in other words, that stars close to the sun would look as if they had moved a little away from the sun, and from one another.[3]

Popper sees this as a case that cannot, under normal circumstances, be observed as such stars are rendered invisible in daytime by the sun's consuming brightness. But in the event of eclipse, the photographs of them can be taken. Granted that the same constellation is photographed at night, the distances on the two photographs could be measured

2. W. H. Newton-Smith, The Rationality of Science (London Routledge & Kegan Paul, 1981) p. 44.
3. Popper, Conjectures and Refutations, p. 36

which enhances the checking of the predicted effect.

The exciting thing in the above case centres on the risk inherent in a prediction of this nature. If through observations it is established that the predicted effect is completely absent, then the theory stands refuted. And in such a case, the theory is seen by Popper to be '... incompatible with certain possible results of observation - in fact with results which everybody before Einstein woul[illegible] ve expected."[4]

From the above analysis, Popper reaches a number of conclusions. One of which is the fact that to obtain confirmations or verifications for almost every theory is very easy, if we are after confirmations. The second one is that confirmations should come into play if they are the result of risky predictions, that is if unenlightened [illegible] the theory at stake, we are to have anticipated an event which was not compatible with the theory - an event that would have refuted the theory. He also sees every scientific theory that is worth its name as a prohibition; it prohibits certain things to happen. A theory is better for it the more it prohibits. He sees any authentic attempt to test a theory as attempts meant to refute it. Confirming evidence, he says, should only matter when such is the result of a genuine test of the theory, which is to say that it can be seen as a serious but non-successful attempt to falsify the theory. He remarks that it is the case that some real testable theories, when established to be false, are still preserved by their admirers; this is done by way of

4. Ibid, p. 36.

introducing some ad hoc auxillary assumptions, or by the re-interpretation of the theory in such a way that it escapes falsification. This kind of procedure can always be, but that the theory in question is rescued from refutation is achieved at the cost of destroying or at least lowering its scientific status.

Popper believes there should not be so much anxiety to protect a theory as to ensure that such a theory has a focus, an aim and be explicit enough as to how to achieve its purpose. He, as a result, posits that the main task of a philosopher should be the cultivation of critical habit, and as Bartley III points it out,

> philosophers, should not demand and search for infallible intellectual authorities, but should instead try to build a philosophical programme for counteracting irtellectual error. 5

He believes we should be bothered on how our intellectual life and institution should be arranged to the extent that it could open up our beliefs, conjectures, policies, sources of ideas, traditional practices, among others(whether justifiable or not) to optimum critical evaluation so as to counteract and possibly eliminate as much intellectual errors as possible. He does not see such things as 'clarity' and 'distinctiveness' as criteria of truth, but certainly when a theory suffers from obscurity and confusion there are obvious indications of errors.

Popper sees science in the light of constant changes. Nothing is ever static in science: hypotheses are tentative

5. William W. Bartley III "Rationality Vs. The Theory of Rationality" in The Critical Approach to Science and Philosophy in Honour of Karl Popper, p. 21.

subject to refutation at any point in time. Hence Dynamism is the term he uses to describe this progressive nature of science.

2. THE DYNAMISM OF SCIENCE

Popper rejects the psychological theory of induction which central thesis is that we wait passively for repetitions to impress or impose regularities on us. He believes instead that we actively attempt to impose regularities upon the world. We make attempt to note similarities in the world or nature, and when such attempts succeed we try to interpret it in terms of laws invented by us. A conclusion may be hastily reached without waiting for premises, but such could be dispensed with very much later if by observation such is shown to be wrong. He sees this theory as that of trial and error, of 'conjectures and refutations.'

By this, it could be understood why our attempts to force interpretations upon the world were logically prior to observation of similarities, The logical reason leading to this procedure can be established and for this reason Popper believes it could be applied in the field of science. Scientific theories, he argues, could not be seen as the digest of observations,

> but that, they were inventions - conjectures boldly put forward for trial, to be eliminated if they clashed with observations; with observations which were rarely accidental but as a rule undertaken with the definite intention of testing a theory by obtaining if possible, a decisive refutation. 6

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6. Popper, Conjectures and Refutations, p. 46.

He believes there is no doubt that it is a widely held notion that the procedure of science is such that we move from observation to theory, to the extent that it looks as if we were being insincere and awkward to say otherwise. He affirms strongly, however, that it looks rather ridiculous to believe that we can start with pure observations in the absence of anything in the nature of a theory. In an attempt to drive home his point, he remarked that some years ago he made attempts to make this issue more clear to a group of physics students in Vienna by starting a lecture with the following instructions: 'take pencil and paper; carefully observe, and write down what you have observed.' In reaction, the students asked him what he wanted them to observe. From this, Popper was convinced that the word 'observe' is absurd. He posits that,

> observation is always selective. It needs a chosen object, a definite task, an interest, presupposes a descriptive language, with property words; it presupposes similarity and classification, which in its turn presupposes interests, points of views and problems. 7

Objects are subject to classification: they can become similar or otherwise only when they are related to needs and interests. This rule, Popper believes, applies to scientists as well as to animals. In the case of animals, a point of view is provided by its needs, the present challenges, and its expectations; as for the scientists, by their theoretical interests, the special problem under investigation, their conjectures and expectations, in addition to the theories that they accept as a kind of background; their frame of reference -

7. Ibid, p. 46

their area of expectations.

He likens the problem of which comes first, the hypothesis (H) or the observations (O) to that of the Hen (H) and the egg (O). His answer to the latter is 'an earlier kind of egg' and to the former his answer was an earlier kind of hypothesis.' It is non-disputable, he asserts, that a given hypothesis chosen ought to have been preceded by observations - the observations, for instance, that it is designed to explain. Such

> observations in their turn presupposed the adoption of a frame of theories. If they were significant; if they created a need for explanation and thus gave rise to the invention of a hypothesis, it was because they could not be explained within the old theoretical framework, the old horizon of expectations. 8

He believes that there is no risk of infinite regress here. If we go back to more and more primitive theories, as well as myths, he argues, we shall ultimately discover unconscious-inborn expectations.

Every organism possesses inborn reactions or responses which include responses adopted to impending events. Expectations are what these responses may be called, which is not to say that the expectations are conscious. For instance, a new born baby's expectations include to be fed, and possibly, to be protected and loved. And the fact that there is closeness between expectations and knowledge, it can still be in order to speak of 'inborn knowledge.' Such knowledge nonetheless cannot be valid a priori just as an 'inborn expecta-

8. Ibid, p. 47.

tion' may be mistaken. A newborn baby's expectation to be fed, loved and protected may after all be illusory. In essence, it could be asserted that we are born with 'expectations', with 'knowledge' which nonetheless lacks validity a priori. The desire for regularity is one of the most important aspects of this expectation. It is an inborn desire for one to always look out for regularities, as could be noted from the pleasure of a child that satisfies this need.

Popper believes that this expectation of finding regularities is both psychologically and logically a priori. It is logically a priori to all observational experience, as it comes before any recognition of similarities, "though it is logically a priori, in this sense the expectation is not valid a priori."[9] It is capable of suffering failure: an environment could be constructed by us (it could be a lethal one) which in comparison with our ordinary environment is so disorderly to the extent that we cannot find regularities. He does not agree with the view that our search for knowledge must succeed, no matter what. It is not always the case that we are successful in imposing the laws of our intellect on nature. Nature has the capability to show all resistance to our attempts to impose our laws on it; but there is always the opportunity for us to try. Thus when a scientist has a purpose, a goal he wishes to attain, he is expected to approach that with all resources, with all the intellect he has. But it does not follow that he must succeed in attaining such goal. It is not

9. Ibid, p. 48.

enough, however, for him to give up after a first trial.

There is always the tendency for a serious-minded researcher and an adventurer to be dogged and ruthless in an attempt to achieve his purpose. Thus, there is a battle between man and nature in the attempt of the former to formulate its laws and impose the same on the latter. Popper argues that the tendency on the part of man to search for regularities and at the same time, impose laws upon nature leads to dogmatic thinking or behaviour. We are inclined to look for regularities everywhere and we dismiss as useless any event that is unable to meet up to this expectations.

The belief is that such events will take us no where as they are not in consonance with our procedure and manner of research. This dogmatism, he argues, is not entirely useless, as it is an outcome of a state of affairs that can only be handled by the enforcement of our conjectures upon the world. The other positive aspect of this dogmatism is that by it we can approach a good theory in stages, by way of approximations. The habit of succumbing to defeat very easily prevents us to notice that we are almost right.

Dogmatic attitude is most often the outcome of a strong belief; it ties us to our impression, and by all means resist all attempts for change. In contrast, critical attitude is a versatile and pragmatic posture. It creates the tendency to permit modification to original tenets. It admits doubt and sought for tests - this is an indication of weaker belief one may say.

Popper's position, however, is that

them to test, to refute them and if possible, to falsify them.

On the basis of the above fact, Popper asserts that critical attitude agrees with science culture while dogmatic one is a common phenomenon in pseudo - science. It could be inferred from this that the pseudo-science attitude is more primitive, and is in existence before that of science. Criticism, as a matter of fact, is directed to prevalent and influential beliefs which are in need of critical revision, which is to say that criticisms are most of times directed to dogmatic beliefs.

> Thus science must begin with myths, and with the criticisms of myths; neither with the collection of observations, nor with the invention of experiments but with the critcal discussion of myths, and of magical techniques and practices. 13

Scientific theories are promulgated and passed on not as a dogma, but as what should be subjected to discussion and a possible improvement. Popper sees this tradition as Hellenic; which could be traced back to Thales who is regarded as the founder of 'the first school' and the concern of this school was not with the preservation of dogma. A critical attitude to the acquisition of knowledge, he sees, as reasonable and rational. A theory is only a way of looking for a solution to a problem and each theory is subject to improvement. As Kneale puts it, there is "no final satisfactory theory

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13. Ibid. p. 50

> dogmatic thinking, an uncontrolled wish to impose regularities, a manifest pleasure in rites and in repetition as such are characteristic of primitives and children, and increasing experience and maturity sometimes create an attitude of caution and criticism rather than of dogmatism. 10

He is in agreement with an assertion of the psycoanalysts which maintains that neurotics and others give interpretation of the world in accordance with a personal pattern that cannot be given up easily, and this can often be traced back to childhood; a pattern or scheme that was adopted very easily in life is maintained throughout, and "whatever experience that is new is interpreted in terms of it; verifying it, as it were, and strengthening to its rigidity."[11] His picture of dogmatism agrees with the above description in contrast to critical attitude that is opened up and ready to welcome novel ideas. Most neuroses, he says, could be explanable in terms of a partially arrested development of the critical attitude

> to an arrested rather than a natural dogmatism; to resistance to demands for the modification and adjustment of certain schematic interpretations and responses. This resistance, in its turn, may perhaps be explained, in some cases, as due to an injury or shock, resulting in fear and in an increased need for assurance or certainty, analogous to the way in which an injury to a limb makes us afraid to move it, so that it becomes stiff. 12

It is his belief that the application of dogmatic attitude is closely related to the tendency to verify our laws and schematics by attempts to apply them and to confirm them even to the extent of neglecting refutations. In contrast, critical attitude involves the preparedness to change them - to subject

10. Ibid, p. 49
11. Ibid, p. 49
12. Ibid, p. 49.

because nature is infinitely complex..."[14]

He believes that it is an effort in futility to try to justify or prove anything (outside of mathematics or logic) and that it is unreasonable to demand for rational proofs in science. In spite of this, he still extols the role of logical argument of deductive logical reasoning for the critical approach, not because it permits us to prove our theories, or to infer them from observation statements, but because it is only by purely deductive reasoning that we will be able to discover what our theories imply, and then be in a position to criticise them.[15] And it is only by criticism that we are able to find the weak spot in a theory. If a theory is seen as consisting absolute knowledge which cannot be challenged, discussed or refuted, there will not be an opportunity to detect the loopholes in it which if detected could have led to a new information in scientific knowledge. As Bernays points out, the "... refutation of one theory is often the triumph of a new theory..."[16]

The advancements in science are achieved by the critical evaluation of theories and possible refutations. There could be new inventions and discoveries by so doing. And by so doing, too, the defectiveness and timid assumptions originally made in the acceptance of the theory will be noted and repudiated. If man, he says, had accepted to live in the world that was completely unknown to him, and made all the

14. William Kneale, "A Scientific Revolution for Ever" In The British Journal For The Philosophy of Science, Volume 19, 1969, p. 27.
15. Popper, Conjectures and Refutation, The Growth of Scientific Knowledge, p. 51.
16. Paul Bernays "Reflections on Karl Popper's Epistemology" in the Critical Approach to Science And Philosophy in Honour of Karl Popper, p. 42.

necessary adjustments that he could do, and have utilised the much opportunities available to him, and have tried enough to explain phenomena as much as possible with theories and laws, then it follows that the only rational way to approach issues should be by trial and errors. And this is the same as we propose conjecture and refutation theories boldly, and in turn put in our best to show that they are wrong: if our efforts failed we grant them acceptance tentatively.

On the basis of this argument, Popper goes to say that all laws, all theories are essentially conjectural or hypothetical, even if we are unable to raise doubt about them anymore. Prior to the refutation of a theory, we can hardly know in what way a modification can be carried out in it. The interest of the scientist should be to propound a bold theory and a theory is bold as Chalmers posits, if,

> it predicts 'new' kind of events (which the physicist calls 'new effects') such as the prediction which led to the discovery of wireless waves, or of zero point energy to the artificial building up of new elements not previously found in nature as opposed to 'events of a kind which is known such as eclipses, or thunderstorm. 18

Theories are meant to be in stiff competition with each other. The adoption of a critical attitude to them ensures that it is only the fittest that survives. There is no

17. Popper, Conjectures and Refutations, The Growth of Scientific knowledge, p. 51
18. A.F. Chalmers "On Learning from our Mistakes" British Journal For the Philosophy of Science, Vol. 24. 1973. p. 165.

chance for the emergence of fragile and weak-founded theories by the application of our critical method. There is a lot of sense and rationality in this procedure, Popper believes. The success of science depends upon this (critical argument) the application of deductive rules, luck and ingenuity.

The essence of observations and experiments in science is simply as tests of our conjectures or hypotheses, that is attempts to refute them. This is in conformity with the principle of empiricism which centres on the contention that in science it is only with the weapon of observation and experiment that the acceptability or the rejection of scientific statements, including laws and theories may be attained. And the acceptance of laws and theories as earlier on noted is tentative which Popper at times called hypotheticis

A theory that is capable of surviving the severest tests we can afford is at least accepted tentatively; if it cannot do this, it is rejected. And we are able to arrive at a theory from a problem-situation, and the theory is expected to enable us to elucidate the observations that created the problem. And the process of emerging to a good theory from observations involves the testing of a number of theories, eliminating bad ones and in the process be able to invent new ones.

And, as a matter of fact, the method of trial and error, involves the elimination of false theories by observation statements. That we prefer non-falsified theories to falsified ones is simply because science entails the search for truth (though we can never be sure we have found it) and moreso any theory that is falsified is seen to be false and those that are not falsified are seen to be truth or at least close to it. Also, that we prefer a non-falsified theory is because it proves to be more authentic than its competitors, and it is able to solve our problem at hand plus the fact that it has passed through many tests successfully and may still be able to do the same in future.

As a matter of fact, one of the reasons for Popper's rejection of induction centres on his fragile faith on laws, and the fact that the future may not be like the past. He cited the case of the appearance of sun at midnight, the fact that water may not quench thirst and air being able to choke those that breathe it. Our belief, he argues, that the future will be like the past has its foundation in the view that laws of nature are immutable. And the laws of nature, he argues further, can only be realiable if we are in possession of regularity that does not change, but once there is a change in regularity, then the foundation of the laws of nature has been rocked. That we search for natural law is because we have the belief that we can find it, and that it exists, but "our belief in a given natural law, Popper stated, cannot have a safer basis than our unsuccessful critical attempt to refute it."[19]

19. Popper, Conjectures and Refutations, The Growth of Scientific Knowledge, p. 57.

So long a proposition says something about the world that is concrete and which could be falsified, Popper believes that such a proposition is empirical. And it is by empirical evidence that the falsity of a proposition could be inferred. The falsity of a particular proposition could be established right-way, but a universal proposition falsity may not be established at once. And once such a proposition is not yet falsified, it is to be taken to be provisionally true till further notive. Munz puts it thus:

> No finite number of positive instances will ever conclusively verify a universal proposition; and similarly no finite number of observation will ever entitle us to use a general concept or a word which means more than one thing, only. But a single negative instance will conclusively refute a general law. 20

What determines the empirical nature of a proposition, Popper argues, is not its verifiability but rather if it can be falsified or not. He sees the study of empirical science as involving, among others, the development of our methods of thinking and talking about nature. It is only in the process of doing this that we are able to appreciate the complexity of nature, and this understanding enhances the uses we make of it.

He believes that our knowledge must have a foundation, as it is not possible that our knowledge should start from nothing, but certainly as earlier remarked, it is not from observation. That we are able to advance our knowledge is because there is always the possibility of modifying our earlier knowledge. And as of sources, most

20. Peter Munz, "Popper and Wittgenstein" in The Critical Approach to Science and Philosophy in Honour of Karl Popper. p. 90.

of the sources of our knowledge are traditional, but any aspect of our traditional knowledge, even those that are seen to be inborn are subject to critical evaluation and possible falsification. The significant role that observation and reasoning - intuition and reasoning inclusive - pplay is in enabling us in the critical evaluation of those bold conjectures with which we examine the unknown forces of nature, which we want to know for the service of mankind.

It is wrong to see any source of knowledge as ultimate. He welcomes any source, any suggestion, so far as such will be exposed to critical examination. It is only by critical appraisal and rigorous tests that we know the authenticity or otherwise of a source of knowledge or theory, for as Grunbaum posits "... attempted refutation or severe ('crucial') tests are conducive to the fruition of the scientific quest for truth."[21]

Simpler theories, he argues, have higher degree of testability than complicated ones. Popper strongly believes in the utility of scientific research. A research should have a direction and a purpose. It is only the cultivation of this virtue that will bring out the best out of science. It is not very much in the interest of scientific growth, he argues, for a scientist or a philosopher at that to be pre-occupied with talking about what he or others are doing or might do.

What contributes to scientific progress, he believes, is for a scientist to tackle and possibly solve scientific problems. He says that,

21. Adolf Grunbaum "Is the Method of Bold Conjectures and Attempted Refutations Justifiably the Method of Science" in the British Journal for the Philosophy of Science. Vol. 27, 1976, p. 105.

> Any unsuccessful attempt to solve a scientific or philosophical problem, if it is an honest and devoted attempt, appears to me more significant than a discussion of such a question as 'what is science?' or 'What is philosophy?' 22

Similarly, he does not place so much importance on the distinction of disciplines. He reasons that we believe that we have such a thing as physics, or biology or archaeology, and that such 'studies' or 'disciplines' could be distinguished by the subject matter which they investigate, centres on the conviction that a theory necessarily follows from a definition of its own subject matter. He does not, however, accept the fact that subject-matter should be the basis for placing areas of studies into disciplines.

> Disciplines are distinguished partly for historical reasons and reasons of administrative convenience (such as the organisation of teaching and of appointments), partly because the theories which we construct to solve our problems have a tendency to grow into unified system. But all this classification and distinction is a comparatively unimportant and superficial affair. We are not students of some subject matter but students of problems. And problems may cut right across the borders of any subject matter or discipline. 23

As an illustration of the above assertion, we could give an instance with the case of the activities of a geologist: his problem such as evaluating the chances of discovering deposits of oil or uranium in a given place can be effectively tackled with the theories and techniques seen usually as Mathematical, physical and chemical. Also, in the area of atomic physics, geological survey plays a significant

22. ____. Popper, Conjectures and Refutations, The Growth of Scientific Knowledge, p. 66.

23. Ibid, P.67

role in handling one of its most abstract and fundamental theories, like testing predictions about the relative stability or instability of atoms of an even or odd atomic number. It is geological techniques and theories that are applied to solve this problem.

Despite the above points, Popper still admits that some problems, which could be tackled by making references to different disciplines, can still be identified with specific disciplines. For instance, two problems stated above rightly 'belong' to geology and physics respectively: at least the major issues involved in them are common features of those disciplines.

> It arises out of the discussion of some theories, or out of empirical tests bearing upon a theory; and theories, as opposed to subject matter, may constitute a discipline (which might be described as a somewhat loose cluster of theories undergoing challenge, change and growth). 24

The making of the above point, he reasons, does not affect his position that classification into disciplines plays minute roles in the acquisition and advancement of knowledge. Our interest should be on how to tackle problems and not how to belong to some disciplines. It is the ability to formulate theories, through which a problem could be tackled that makes one an authentic scientist. He (Popper) remarks, however, that theories, no matter how solid their background, may not enjoy any permanent status.

It is the fact that no theory has absolute claim to truth

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24. Ibid. p. 67

than the need arises for every theory to be tested as rigorously as possible with the aim of falsifying it. In the process of doing this, new information about the theory could be found which assist generally in the acquisition and growth of scientific knowledge. This is what makes research programmes in science to be dynamic.

In line with his dynamic outlook to science, Popper rejects the metaphysical idea of determinism. He believes the theory is hardly justifiable for various reasons. It does not agree, according to him, with the rationalist tradition to which he subscribes. It is also not in agreement with rational critical attitude which is the hallmark of science.

If there was not this critical attitude, it is highly doubtful if we can have science as we do today for "'science is differentiated from the older myths not by being something distinct from a myth, but by being accompanied by a second-order tradition - that of critically discussing the myth."[25] What we had previously was the first order tradition, when there was just a story handed down. But now, story is still handed down, but there is the provision for rational discussion for critical reflection and a follow-up acceptance, modification or complete rejection. The second order tradition (critical attitude) is fundamentally one of the greatest assets of science for he believes "that science is essentially critical; that it consists of bold conjectures, controlled by

25. . Popper, Conjectures and Refutations, The Growth of Scientific Knowledge, p. 127.

criticism, and that it may, therefore, be described as revolutionary."[26]

In a sense, Popper sees science as myth-making in comparison with religion. There is a difference, however, between the myths of science and that of religion. This difference centres on the critical attitude of the former in contrast to the dogmatic attitude of the latter. When a scientific myth is presented, there is an open-mindedness about it. Anybody can use it to face a given problem it is meant for. After attempts to do this fails or succeeds, it could then be said, this myth is suitable or not for the handling of this kind of problem. This finding will form the basis for its rejection or acceptance. By so doing, the growth of scientific knowledge will be enhanced.

> Thus it is the myth or the theory which leads to and guides our systematic observation - observations undertaken with the intention of probing into the truth of the theory or the myth. From this point of view, the growth of the theories of science should not be considered as the result of the collection, or accumulation of observations; on the contrary, the observations and accumulation should be considered as the result of the growth of scientific theories. 27

Popper calls this the 'searchlight' theory of science; which is that it is the duty of science to throw new light on things; that science in the process of solving problems and creating new ones, profits from observations. When myths, for instance, are mounted with enough pressure of criticism they (myths)

26. Popper, "Normal Science and its Dangers", in Criticism and the Growth of Knowledge, Proceedings of the International Colloquium in the Philosophy of Science London, 1965, Volume 4, edited by Imre Lakatos Et al. p. 55.
27. Popper, Conjectures and Refutations, The Growth of Scientific Knowledge, p. 127.

are forced to condition themselves to the challenges of providing us an adequate and a more detailed picture of the world in which we find ourselves. This is basically why scientific myths, when exposed to the pressure of criticism, are quite distinct from religious ones. In their origin, however, they are properly seen as myths or inventions.

He emphasizes that it is a grievous error to see scientific theories as just the results of observation, they are instead the products of myth-making and of tests.

> Tests proceed partly by way of observation, and observation is thus very important; but its function is not that of producing theories. It plays its role in rejecting, eliminating and criticising theories; and it challenges us to produce new myths, new theories which may stand up to these observational tests. 28

If we are able to appreciate this, he says, we can as well note the essence of tradition in science.

There are two main ways through which we may explain the growth of science. The first of this is that which perceives science in terms of accumulation of knowledge, like a growing library, or a museum; where knowledge could be assessed by the extent of the accumulation of books. The second one explains growth by the intensity of criticism; its method of growth is more revolutionary than a simple accumulation of knowledge. This latter method involves the destruction, changes and alteration of everything, even its most vital instrument and the language in which our myths and theories are formulated. The first method Popper sees as less important and contrary to popular opinion. He believes

28. Ibid, p. 128.

that "the accumulation of knowledge in science is very much less when compared with the revolutionary changes of scientific theories."[29] If the growth of science is attained by the accumulation of knowledge, it makes no difference if scientific tradition is lost, for at any point in time, a fresh accumulation could start.

But if the advancement of science is by the tradition of changing its traditional myths, there is thus the need for something with which to start. In the absence of anything to change or alter one will remain static. And because science does not permit this, it possesses two major essential beginnings, namely, the emergence of myths and a subsequent new tradition of changing the same critically. The kind of tradition that needs to be preserved in science is, for instance, the user of descriptive language in science. Language, Popper argues, grows together with the growth of myth, and every language preserves countless myths and theories. There is the need to preserve this tradition, for granted they are destroyed, it will not even be possible to start accumulating any knowledge in the first instance. The possibility of this is not in doubt. For instance, in physics the theories are not completely axiomatized, yet the links between its various parts may be sufficiently clear to enhance our decidability on which some of its sub-systems are affected by particular falsifying observation.

Popper insists that every ... theory should be subjected to tests no matter the magnitude of confidence

29. Ibid, p. 129.

we have on it. He detests the practice of using some ad-hoc auxillary assumptions to rescue a theory from refutation. This goes to show how hollow the foundation of such a theory is. It is not every theory in science, he believes, as Lakatos points out, that we should take seriously. As Lakatos points out "... a theory should be taken seriously when a crucial experiment could and indeed has been deviced against it,'[32]

By the methodology of subjecting our theories to crucial tests, we are able to eliminate false theories. A theory that is able to withstand rigorous tests is to be preferred to one that is easily falsifiable. Thus Popper sees scientific practice essentially as consisting of ingenous attempts to falsify theories that have been put forward to tackle various problems in scientific circle.

Where a theory witstands a crucial test it is corroborated where otherwise it is refuted. Corroboration thus becomes to Popper a yardstick to measure the authenticity or otherwise of a theory. A detail discussion on this is given in the next sub-section.

3. CORROBORATION

Popper, as earlier on remarked, does not believe that there is any source of knowledge in the empirical science that cannot be falsified. In fact, falsification is an essential feature an empirical science should possess. He

32. Imre Lakatos, "Popper On Demarcation and Induction" in The Philosophy of Karl Popper, Vol. I. p. 242.

did not advocate, however, that all our theories must be falsified. A theory that passes our crucial test is qualified as an instrument to handle a given problem. Such theories that cannot be falsified by our sincere attempts are seen to be corroborated at least tentatively. Corroborating a theory, he argues, differs significantly from verifying it. A theory, according to him, cannot be verified but it can be corroborated. He debunks the attempts of the inductive logician to ascribe the values of 'true', 'false', 'probability' to statements. He believes that it is valueless in scientific development to talk about the probability of hypothesis. Instead of such trivial adventure, our interest on hypothesis should be to assess the tests, the trials, it has passed; that is, we should try to evaluate how far it has been able to show its fitness to be, by standing up to tests.

This is the same as assessing how far it has been corroborated. That a theory is corroborated is an evidence that it has an evidence that it has successfully passed through series of tests. We have to note, however as Feigle remarks that

> Popper allows for corroboration in the sense that we may say that a theory which has withstood very severe tests is, at least until further notice, acceptable as part of the justified corpus of scientific knowledge claims. 33

It is to be noted that the view of Popper on the suitability of scientific theories differs from those of the positivists in

32. Imre Lakatos, "Popper on Demarcation and Induction"
33. Herbert Feigl, "What Hume might have said to Kant" in the Critical Approach to Science and Philosophy in Honour of Karl Popper, p. 48.

diverse ways. While Popper believes that a theory can be corroborated the positivists (the Vienna circle members) believe that it can rather be verified.

It is, however, remarkable that Popper, **did not make use of verification in determining the status of theories.** [illegible] that [illegible]. He dismisses the belief that there can be a complete verification of empirical statements. What is only possible is a preliminary corroboration. And this corroboration as Kraft said does not mean more than: "not yet falsified."[34]

It is erroneous, Popper argues, for some people to believe that theories are verified immediately some of the predictions derived from them have been verified. It is absurd, too, he asserts, to hold the belief that natural regularities do not change. A law that is already corroborated can be falsified, according to him. Hypotheses are to be seen just as conjectures and not as true statements. Traces of incompatibility are indications that a hypotheses is riddled with intellectual errors and as a result, stands falsified. Compatibility is a positive indication to a hypothesis but it is not enough to guarantee its corroboration. In addition to a given theoretical system being compatible with basic statements, a non-empty sub-class of these basic statements should be derivable from the theory in conjunction with the other accepted basic statements.

Also, the simplicity of a theory contributes to the strength of its testability. Simple and strong hypotheses

34. Victor Kraft, "Popper and the Vienna Circle" In the Philosophy of Karl Popper, Vol. I, p. 196.

are preferable to complex and weak ones, for the former are both rich in content and make severe testing possible. It has to be noted, too, that the severity of testing in a given instance depends not only on the strength of the hypothesis but equally on other factors such as the ingenuity of the experimenter, his sincerity of purpose, his technical capabilities and the ability to reach evidence. As Giedymin shows,

> Popper emphasizes that
> to make a choice of a hypothesis in accordance with the testability - corroboration criterion it is not sufficient to know that the hypotheses under consideration are compatible with the evidence in the logical sense. It is necessary to know how the evidence was obtained in order to appraise the severity of testing. 35

We are only right to say that a theory is corroborated accordingly if we have made sincere and desperate predictions from it. Corroboration represents a form of validity capable of degrees, and it can be predicted on a proposition only relatively, not absolutely until further notice. We can thus see theories as being better or less well corroborated. And the number of corroborating instances does not show degree of corroboration as the severity of the various tests to which the hypothesis in question can be and has been subjected to. "But the severity of the tests in its turn depends upon the degree of testability and thus upon the simplicity of the hypothesis."[36]

It is not possible for us to compare the degree of corroboration of two statements, any more than the degree of falsifibility. Equally, we cannot define a numerically calculable

35. Jerezy Giedymin, "Strength, Confirmation, Compatibility" in The Critical Approach to Science and Philosophy in Honour of Karl Popper, p. 37.

36. Popper, The Logic of Scientific Discovery, p. 267.

degree of corroboration. The much we can do is only to speak roughly in terms of positive degree of corroboration, negative degree of corroboration and so on. We can state clearly rules much as the one that can say we shall discontinue to accord a positive degree of corroboration to a theory that has been falsified by an inter-subjectively testable experiment centred on a falsifying hypothesis. Inter-subjectively testable falsification so long as the test is carried out thoroughly is seen as final. The need for this thoroughfulness as Stove points out made Popper to "... enjoin our utmost efforts to establish empirically the falsity of any proposed law or theory."[37]

As we have noted that Popper's belief as far as the methodology of science is concerned is that it is always the theory and not the experiment, always the idea and not the observation which open the way to new knowledge, he also accepts that it is always the experiment which enables us to follow only the rational part through which new information about certain ideas are gathered, which inform us to corroborate or falsify them.

A theory, generally speaking, has a higher degree of corroboration if it has a higher degree of universality. Equally, precision is another factor that count positively for theories: a theory that is precise stands a better chance of being corroborated than that which is vague. That a test could be severely carried out depends to a large extent on the precision of its assertions and its predictive content increases with these two factors. Thus the content, the much information a statement can

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37. David Stove, Popper and After, Four Modern Irrationalists, p. 6.

give goes a long way to determine to what extent it could be tested and possibly corroborated. A scientist should thus be out to seek for more and more content, more information about a statement before he goes ahead to carry out his tests if he is interested in carrying out a thorough scientific research.

A scientist is out for bold conjectures; conjectures that could be severely and independently tested. It is such theories that stand good chances of being corroborated as against conjectures that are tested with laxity, without any sense of purpose. Prophecies of palmists and sooth-sayers cannot be positively tested or corroborated for their predictions are so cautious and imprecise that the logical probability of their being correct is very high.

> And if we are told that more precise and thus logically less probable predictions of this kind have been successful, then it is not, as a rule, their success that we are inclined to doubt so much as their alleged logical improbability since we intend to believe that such prophecies are non-corroborable, we also tend to argue in such cases from their low degree of corroborability to their low degree of testability. 38

The corroborability of a theory stands in inverse ratio to its logical probability, he says. His concern for a strong foundation for scientific theories urges him to advocate that auxillary hypotheses should be used as sparingly as possible, and to that the number of axioms that we make use of should be minimal. Statements of a high level of universality should be preferred always.

38. Popper, The Logic of Scientific Discovery, p. 270.

Corroboration, it should be noted, has not the same value as 'true' or false'. Truth and false are free from temporal subscripts. Corroboration has a tentative value. That a theory is corroborated today does not mean that it has gone to a stage of finality that its falsification cannot be. The word 'corroboration' is used to show how genuine a scientific theory is, it is not a note of finality or an end to research as far as the issues the theory takes care of are concerned. Also, that a theory is yet to be corroborated does not mean that it cannot be successfully corroborated.

There could be new information, more equipment and more devotedness that can achieve this feat (corroboration) at a later date. But when it is said that something is true or false, it is seen as such. If a statement we appraise as false yesterday is appraised as true today, it follows that we were mistaken in our appraisal of the statement yesterday, and that the statement was even true that very yesterday we appraised it false.

Corroboration is entirely a different thing, according to Popper. It is an evaluative report of past performance. Competing theories, 'A' and 'B' in the light of a critical discussion at the time 'T', and empirical evidence (test statements) at our disposal at the discussion, the theory 'A' is preferable, or differently put better corroborated than the theory 'B'. We have to note, however, that the degree of corroboration at a given time 'T' makes no assertion about the future, that is about the degree of corroboration at a time later than 'T'.

The report we examined is simply a report which centres on the state of discussion at the time 'T' on the logical and empirical preferability of the competing theories. Popper's conception of corroboration is not forward looking - it takes care of the immediate problem in respect of immediate evidence available to us. It is possible that the evidence that may be available in future may favour the corroboration of the theory 'A' or the evidence immediately available may not be tenable for the corroboration of the same theory in the immediate future - for the corroboration of theory in Popper's sense carries no future implications.

Where a theory is not corroborated it is falsified. That it is falsified means that it was not able to pass through crucial Tests. Falsification which is fully discussed below is thus the fate that awaits weak theories that could not be corroborated by tests.

4. FALSIFICATION:

Falsifiability is introduced primarily as a criterion for the empirical character of a system of statements. A theory is seen as falsified if we have accepted basic statements that contradict it. In as much as this condition is necessary, it is not sufficient, as non-reproducible single occurrences have been seen to be of no significant to science. In essence, a few stray basic statements contradicting a theory is not enough to make us to reject it as falsified. The discovery of a reproducible effect is the only condition that will make us to see it as falsified. "In other words, we only accept the falsification if a low-level empirical hypothesis which describes such an effect is proposed and corroborated. This kind of hypothesis may be called a falsifying hypothesis."[39] The falsifying hypothesis must be empirical and so fasifiable. This means that it must stand in a certain logical relationship to possible basic statements. This requirement applies only to the logical form of the hypothesis. That the hypothesis should be corroborated refers to tests that it ought to have passed - tests which confront it with accepted basic statements.

We see basic statements in dual roles here. Foremost, we have used the system of all logically possible basic statements in order to obtain with their help the logical characterization for which we were looking (that of the form of empirical statements). From the other perspective, the accepted basic statements are the basis for the corroboration of hypothesis. Once a basic statement contradicts a theory, enough ground for its falsification has been provided. Equally a hypothesis that is beset with contradiction ceases to be a source of knowledge. A hypothesis that is worthy of scientific use, argues Popper, must be seen to be consistent. Other essences of consistency in science in general and falsification in particular are given below.

5. CONSISTENCY:

Consistency is very paramount as far as any theoretical system is concerned. Any theory that is worth its name

39. Ibid. p. 87.

in the scientific structure must have to satisfy this requirement. A self-contradictory system actually does not belong to any worthy field of study, be it scientific or not. This is so because in addition to other shortcomings, a self-contradictory system is false, and there is no field of study [illegible] y that romances with false ideology. It can, in essence, be seen that a self-contradictory system is more useless than a false one. It can happen at times that we may work with a false system that even has the capability of yielding results; such a system has a purpose it serves.

In contrast, a self-contradictory system has no information to give. This is because any conclusion whatsoever that pleases us can be drawn from it. Which means that no statement is singled out, either as incompatible or as derivable since all are derivable. A consistent statement, on the other hand, divides the set of all possible statements into two: those which it contradicts and those with which it is compatible. (Among the latter are the conclusions which can be derived from it). It is for this reason that consistency constitutes a general requirement for a system, be it empirical or non-empirical, if any use is to be made of it at all. Besides consistency, an empirical system must be falsifiable, insists Popper. for

[illegible]

him for a theory to be scientific it has to be falsifiable. And consistency and falsifiability to Popper seem analogous.

> Statements which do not satisfy the condition of consistency fail to differentiate between any two statements within the totality of all possible statements. Statements which do not satisfy the condition of falsifiability fail to differentiate between any two statements within the totality of all possible empirical basic statements. 40

Besides consistency, objectivity is another crucial factor in science. If science is not objective, then it lacks a strong basis to be differentiated from non-science. Hence objectivity, as is clearly shown in our preceeding sub-section, is indispensable for any theory that is scientific.

6. EMPIRICAL BASIS AND OBJECTIVITY

Popper looks at science from an objective perspective. He made a distinction between objective science, on the one hand, and our knowledge, on the other hand. He did not dispute the contention that it is only observation that can give us knowledge of facts and that we can (as Halin says) become aware of facts only by observation. The point he emphasizes is, however, that this knowledge of ours does not justify or establish the truth of any statement. In view of this fact, it is inappropriate for the epistemologists to question the basis of our knowledge, or asking how I can justify my experience against doubt.

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40. Popper, The logic of science discovery, p. 92

The right question for Epistemologists to ask, according to Popper, is how we can test scientific statements by their deductive consequences, and secondly, the kind of consequences we can select for this purpose if they, in their turn, are to be inter-subjectively testable. The belief as far as the empirical statements of science are concerned is that they are grounded on experiences such as perception or 'protocol sentences'. It will be seen by most, however, that any attempt to base logical staements on protocol sentences is tending towards psychologism. The situation is the same as far as empirical statements are concerned, too. But whether it is the question of logic or statement of empirical science, Popper sees the answer as the same,

> Our knowledge, which may be described vaguely as a system of disposition, and which may be of concern to psychology may be in both cases linked with feelings of belief or of conviction in the one case perhaps, with the feeling of being compelled to think in a certain way, in the other with that of perceptual assurance.41

All these interest only the psychologists. The only way to ensure the validity of a chain of logical reasoning is to put it in form in which it is most easily testable. They are to be broken up into many small steps, each easy to check by whoever that has learnt the mathematical or logical technique of transforming sentences. We will ask anyone that still raises doubt to point out whichever error he had noticed, or alternatively to think the matter

41. Popper, The Logic of Scientific Discovery, p. 99

all over again. We have the same picture in the case of empirical sciences. Empirical scientific statements are presented (by describing experimental arrangements) in a pattern that whoever that has learnt the relevant technique can test it. In case of a rejection of our statement whoever does that must give an alternative view that counter ours and provide us with instruction for testing it. Where he is unable to do this, we only have no option than to tell him to have a careful look at our own experiment.

In a situation where the logical form of an assertion renders it untestable, it can, at best, operate within science as a stimulus. It can suggest a problems. In the field of logic and mathematics, this may be exemplified by format's problem, and in the field of natural history, say by reports about sea serpents. Given any of the above situation, science does not say that the reports are unfounded, that that format was in error, or that all the records of observed sea serpents are lies; instead it suspends judgement.

Science is not viewed only from the point of view of the epistemologists. It could be looked upon as a biological or a sociological phenomenon. It can be viewed as a tool or an instrument that could be compared to some of our industrial machinery. It could be perceived as a means of production - as the last word in round-about production. From this standpoint, science cannot be seen as very much connected with our experience than other instruments or means of production. Popper admits that it is not

> incorrect to say that science is ' ... an instrument' whose purpose is '... to predict from immediate or given experiences to later experiences, and even as far as possible to control them. 42

He is, however, convinced that all the talk about experiences do not contribute to clarity.

> It has hardly more point than, say, the not incorrect characterization of an oil derrick by the assertion that its purpose is to give us certain experiences; not oil, but rather the sight and smell of oil, not money, but rather the feeling of having money. 43

Testing to Popper is what determine the suitability of a theory. And testability goes with experimentation. And the essence of testing is to ascertain the suitability of a theory. We shall see more of this theory and experiment in the next subsection. We shall also see the role that basic statement plays in experiences, theory and experiment.

6. <u>THEORY AND EXPERIMENT</u>

Basic statements are statements that assert that an observation event is taking place in a particular place at a particular time. They are about experience, and are required to establish if a theory is falsifiable - that is, if it is empirical.

There are rules governing the acceptance of basic statements. One of the crucial rules is that which tells us not to accept logically disconnected statements. It is

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42. Ibid. p. 100

43. Ibid, p. 100.

in the process of testing theories that we accept basic statements. In such tests, we must maintain a very critical attitude.

The procedure of the Empiricists or the Inductivists is quite different. They believe our procedure is such that we collect and arrange experiences and by so doing, be able to climb the ladder of science. Which is the same as saying that any desire to attain scientific knowledge requires foremost the collection of protocol sentences.

The crucial question here, however, is how possible it is for me to carry out orders in regards to the assemblage of my experiences. Am I to record that I am writing, that I see a boy running, that I hear a bell ringing or a newsboy shouting; or am I to report that I am being irritated by all the noises emanating from all these events around me? Granted that I can carry out this order, and however rich the assemblage of my experiences might be, they could never add up to a science. There are a number of serious issues involved in science, among which are the need for points of view and theoretical problems.

> Agreement upon the acceptance or rejection of basic statements is reached as a result, on the occasion of applying a theory, the agreement in fact is part of an application which puts the theory to the test. Coming to an agreement upon basic statements is like other kinds of applications to perform a purposeful action guided by various theoretical considerations. 44

44. Popper, The Logic of Scientific Discovery, p. 106

With the above, it will be possible for us to explain regular coincidences, unlike the inductive logician that cannot explain regularity by theories. This is because of his belief that all science start from stray elementary perception; besides, he is committed to the view that theories are nothing but statements of regular coincidences. Our position, however, reveals that the link between our various experiences are explicable and deducible in terms of theories that we are engaged in testing. A falsifiable theory cannot answer such questions as to why we are lucky often in the theories that we construct or why there are natural laws. Questions of this sort belong to the discipline of metaphysics.

A definite number of questions face an experimenter, and he (experimenter) tries to find answers to such questions; and in doing so, he tries as much as possible to eschew every other question that is not related to the ones relevant to him. His sensitivity only to the questions facing him enables him to detect all possible sources of error, which are to be avoided by him. To do all these is obviously not meant to ease the task of the theoretician (who presented definite questions to the experimenter), and it is neither to provide a standpoint for the theoretician for inductive generalization. The theoretician is expected to have done at least the most crucial aspect of his work like a clear and sharp formulation of his question. The experimenter is able to proceed by the way the theoretician has already shown. Popper sees it as equally wrong to suppose that the experimenter makes exact observation in the main, as his work predominantly constitutes theoretically the initial planning to the finishing touches in the laboratory.

> This is well illustrated by cases in which the theoretician succeeded in predicting an observable effect which was later experimentally produced! Perhaps the most beautiful instance is the Broghe's prediction of the wave-character of matter, first confirmed experimentally by Davidson and Germer. 45.

A theorist is pushed to look for a better theory as a result of the experimental falsification of a theory, so far accepted and corroborated. And, in fact, as Chalmers put it,

> tests of a bold, highly refutable theory that result in a confirmation of the theory are informative because of the unlikelihood of the result as assessed prior to the tests. 46.

We do obviously accept the theory instead of the other. The question is why do we choose one theory instead of the other. That we have chosen a theory cannot be as a result of the experimental justification of statements composing the theory, and it is neither not as a result of a logical reduction of that theory to experience. That we choose a theory is obviously because it surpasses the other competing ones in competition. The one we select is usually that that excels the others, that prove to be the fittest in natural selection which has the greatest potential for survival. Such a theory should be capable

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45. Ibid. p. 108.

46. A. F. Chalmers "On learning from our mistakes" in <u>the British Journal for the philosophy of science</u> 1973, vol. 24.

of withstanding rigorous tests. Popper believes as Maxwell remarks, that

> ... At any stage, the best theory is the theory of highest empirical content which has stood up best to all our attempts at experimental refutation. 47.

Logically the testing of a theory is based on basic statements whose acceptance or rejection lies on our decisions. We can see in actual fact that it is decision that settles the fate of theories. The difference between Popper's method of deciding on theories with those of the conventionalists is that, with Popper, the guiding principle is the severity of tests to be carried out. To the conventionalists the principle is simplicity - they are inclined to the system that is the simplest.

At long last what decides the facts of a theory is the result of a test carried out - an agreement about basic statements. Popper reasons along with the conventionalists that the choice of any particular theory is an act, a practical matter. He differs from the conventionalists in that his choice is decisively influenced by the application of the theory and the acceptance of the basic statement in connection with this application: to the conventionalists aesthetic motives are decisive. He also differs from the conventionalists for his stance that the statements decided by agreement are not universal but singular. From the positivists, he differs too by his position that basic,

47. Nicholas Maxwell "A critique of Popper's view on scientific method" Philosophy of science: Official Journal of the philosophy of science association 1972, vol. 39, No. 1, p. 132.

statements are not justifiable by our immediate experiences, but are from the logical point of view accepted by an act, by a free decision.

The acceptance of a basic statement is part of the application of a theoretical system. This application alone actualises any further application of the theoretical system. As of the empirical basis of empirical science, there is nothing absolute about it. It is wishful thinking to see science as resting upon solid bedrock. Nonetheless, the bold structure of its theories rises, as it were, above a swamp. And as Asike points out

> ... Our beliefs are replaced by competing theories, by competing conjectures. And through the critical discussion of these theories we can progress. 48

Popper compares science to a building erected on piles,

> the piles are driven down from above into the swamp but not down to any natural or given base and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being. 49

Thus, in our methodology, we give preference to theories that are severely testable, and those with the highest possible empirical content. As Ayer puts it,

> an empirical statement which did not belong to the class of basic statements was to be accounted false if it was contradicted by a set of accepted basic statements, so long as our attempts to rest it did not result

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48. J. T. Asike "Scientific Facts and Their Theoretical Frame Works" Uche: Journal of the Department of Philosophy, University of Nigeria, Nsukka, 1984, Vol. 8, p. 40
49. Popper, The logic of scientific discovery, p. 111

> in such a contradiction we might provisionally hold it to be true. 50

Basic statements, on their part, are singular existential statements

> to the effect that an event of such and such a kind occurred at such and such a particular place and time, the only restriction placed upon the range of these events was that their presence or absence should be publicly observable. 51

Popper reasons that no justification could be given for ascribing truth or falsehood to a basic statement. To see a basic statement as true implies accepting it, and to see it as false on the other hand, comes down to rejecting it. The acceptance or rejection remains a matter of decision. Such decision can, however, not be seen to be entirely arbitrary as we are motivated to them by our experiences, though they are not substantiated by them.

As Popper equally noted, this treatment of truth draws him closer to pragmatism. He, however, claimed to differ from the pragmatists for failing to identify truth along with verification,

> His reason was that truth, if one was going to speak of it at all, could not be regarded as admitting of degrees or as capable of change. A statement can be strongly or weakly corroborated, but it cannot be strongly or weakly true. It can be accepted at one time and not at another; but it is true or false without any temporal qualification. 52

It is worth noting that Popper later on changed his stance on the issue of the usage of truth and falsehood. He believes that

50. A. J. Ayer "Truth Verification and Verisimilitude" in The Philosophy of Karl Popper Book II, p. 684.

51. ibid. P. 684.

52. ibid. p. 684.

his earlier stance not to accept it cannot be justified. He points out that since the time of Tarski, he had been convinced of the need to use the concepts, and that its uses need not lead to antimomies. For instance the proposition 'snow is white' is true if and only if snow is white; and this achieves the purpose of the usage which is truth corresponding with fact. He (Popper) says

> ... if we wish to elucidate the difference between pure and applied science between the search for knowledge and the search for power or notfor powerful instruments then we cannot do without it. For the difference is that in the search for knowledge, we are out to find true theories, or at least theories which are nearer than others to the truth - which correspond better to the facts; whereas in the search for powweful instruments we are in many cases quite well served by theories which are known to be false.53

Popper sees one big advantage in the theory of objective or absolute truth. It is that it allows us to assert

> ... that we search for truth, but may not know when we have found it; that we have no criterion of truth, but are nevertheless guided by the idea of truth as a regulative principle and that, though there are no general criteria by which we can recognise truth - except perhaps tautological truth - there are criteria of progress towards the truth.54

Popper compares the status of truth in objective sense as correspondence to the facts and its role as a regulative principle to that of a mountain peak wrapped in clouds. A

53. Popper, Conjectures and Refutations, p. 226

54. Ibid. p. 226

climber has both difficulties in getting there as well as ascertaining when he has got to the peak. Even if he gets to the summit he will be faced with the difficulty of identifying the main summit and a subsidiary peak. This situation does not alter the objective existence of the summit of the mountain, and if the climber says he doubts if he gets to the summit of the mountain, this implies that such a summit does actually exist, but that he gets there or not is entirely a different thing. Thus, that we can be wrong or that there is an error implies the idea of an objective truth which we may not be able to realise. The climber, for instance, may not be able to ascertain that he has reached the summit of the mountain, but it is much more easier for him to precognise that he has not reached there. In spite of this limitation, Popper says Kelly, sees some positive elements in the belief for truth as "all growth is brought about by ... subjective hope in truth ..."[55] In fact, the view says Ayer, which Popper shares with Pierce" that all our beliefs are fallible seem to imply that there is a standard for truth of which they may fall short."[56] It can also be assumed that in scientific enquiry there is the possibility of attaining truth. Be that as it may, the point of interest here is, it is by testing that we are able to assess the strength of a theory.

55. Delly A. Kelly "Popper's ontology: an exposition and critique" Southern Journal of Philosophy, vol. 13. 1976, p. 81.

56. A. J Ayer Truth, verification and verisimilitude", p. 685.

But for a proper and thorough experiment to be carried out in science to ascertain the suitability of a theory, there is the need for precision, and this is the essence of measurement.

7. MEASUREMENT

If due to a high level of universality or precision a statement 'p' is easily falsifiable than a statement 'q', then the class of the basic statements permitted by 'p' is a proper subclass of the class of the basic statements permitted by 'q'.

> The subclass relationship holding between classes of permitted statements is the opposite of that holding between classes of forbidden statements (potential falsifiers), the two relationships may be said to be inverse (or perhaps complementary). 57

'Range' is the name given to the class of basic statements permitted by a statement. The 'range' permitted by a statement to reality is the amount of freedom that it allows to reality. 'RANGE' and empirical content are interchangeable concepts: they are complementary. And the ranges of two statements are related to each other just as are their logical probabilities. Range is made use of to clarify issues that have to do with precision in measurement.

Measurements play significant roles if we must talk of precision. They help to speak with some measure of certainty even when we meant to make a comparison between theories or statement. The technique of measurement is very needful to detect differences between two theories moreso if the consequences

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57. Popper, The Logic of Scientific Discovery, p. 124

of the two theories are very minimal to be detected, maybe due to the fact that our degree of precision is not very high. Armed with the technique of measurement, a certain range - a region within which discrepancies between the observations are permitted by the theory are detected. And the frequent needs for testability of theories "entails the demand that the degree of precision in measurement should be raised as much as possible."[58]

The common belief is that measurement consists in the determination of coincidences of points. The reality of this belief is only to a limited extent. We cannot unequivocally state that there are coincidences of points. It has to be noted that the issue at stake really is measurement and not counting. And strictly speaking two points cannot coincide. However

> two physical 'points' - a mark say on a measuring-body to be measured - can be brought into close proximity, they cannot coincide, that is, coalesce into the one point. 59

This remark may look common in another context, it has to be noted that it is useful in the issue of precision in measurement. And it will be seen that the point of the body to be measured lies between two gradations or marks on the measuring rod, that is that the pointer of our measuring apparatus lies between two gradations on the scale.

> We can then either regard these gradations or marks as our optimal limits of error, or proceed to estimate the position of (say) the pointer within the interval of the gradation, and so obtain a more accurate result. 60

58. Ibid. p. 124
59. Ibid. p. 125
60. Ibid. p. 125

What we can say in this case is that the pointer lies between two imaginary gradation marks. And we have a remnant of interval, a range. In physics, it is a common practice to estimate this interval for every measurement. "(Thus following MILIKAN they give, for instance, the elementary change of the electron measured in electrostatic units as $e = 4.774.10^{-10}$, adding that the range of imprecision is $\neq 0.0005.10^{-10}$." 61

An obvious problem here really is what the purpose of replacing, as it were, one mark on a scale by two - to wit, the two bounds of the interval - when for each of these two bounds there must again arise the same question: What are the limits of accuracy for the bounds of the interval? There is no positive impact in giving the bound of the intervals, excepting these two bounds in turn can be fixed with a degree of precision far exceeding our hopeful attainment. For the original measurement, fixed within their own intervals of imprecision that should be smaller by several orders of magnitude than the interval they determine for the value of the original measurement. This implies that

> ... the bounds of the interval are not sharp bounds but are really very small intervals, the bounds of which are in their turn still much smaller intervals and so on. In this way we arrive at the idea of what may be called the 'unsharp bounds' or condensation bounds' of the interval.62

61. Ibid. p. 125

62. Ibid. p. 125.

There is no pre-supposition of the mathematical theory of errors in all the consideration, nor the theory of probability. It is rather the other way round: by the analysis of the idea of a measuring interval they furnish a background in the absence of which the statistical theory of errors makes very little sense. The repeated measurement of magnitude will enable us obtain values distributable with different densities over an interval - the interval of precision depending upon the prevailing measuring technique. The knowledge of condensation bounds of this interval, that we are in search of is only what we can use to apply to these values the theory of errors, and determine the bounds of the interval.

What we should deduce from all these is the fact of the superiority of methods that make use of measurements over purely qualitative methods. Even in the case of qualitative estimate for instance, the estimate of the pitch of a musical sounds, the interval of accuracy for the estimates may be given, but without measurements there can be no clarity for such interval and in such cases, the concept of condensation bounds cannot be applied. This concept can only be applied when we speak of orders of magnitude, and thus only where methods of measurements are defined.

Theories are constantly in competition. Those that are able to pass through crucial experiments are preferred to those that fall on the wayside, according to Popper. By so doing, the degree of testability is compared. Dimension is also made use to examine the degree of testability. This constitutes the next topic of discussion

8. THE USE OF DIMENSION TO EXAMINE THE DEGREE OF TESTABILITY

The degree of testability of theories can be evaluated by ascertaining the minimum degree of composition which a basic statement must have if it is to be able to contradict the theory. And this is on the condition that we can establish a way to compare basic statements so as to note whether they are more (or less) composite, that is, compounds of a greater (or a smaller) number of basic statements of a simpler kind. We can make a comparison of basic statements and by so doing, those of other statements. To accomplish this, we require to select at random a class of relatively atomic statements that we choose as a basis for comparison. Its definition can be carried out by means of a generating 'schema' or 'matrix' (for instance, 'There is a measuring apparatus for "x" at the place "y" the pointer of which lies between the gradation marks "0" and "p"). It could then be seen as relatively atomic, and thus as equi-composite, the class of all statements obtained from this kind of matrix (or statement function) by the substitution of definite values. A field can be used to describe the class of these statements in addition to all the conjunctions that can be formed from them. We can call a conjunction of different relatively atomic statements of a field an 'n-tuple of the field', and it could be taken that the degree of its composition is equal to the number 'n'.

If there is for a theory 't' a field of singular (but not necessarily basic) statements such that, for some number "d", the theory "t" cannot be falsified by any 'd-tuple' of the field, although it can be falsified by certain d+1-tuples, then we call "d" the characteristic number of the theory with respect to that field. All statements of the field whose degree of composition is less than 'd' or equal to 'd' are then compatible with the theory and permitted by it irrespective of their content.

Thus, the comparison of the degree of testability of theories could be based upon this characteristic number 'd'. To avoid any inconsistency that is likely to arise through the use of different fields, it is essential to make use of somewhat narrower concept than that of a field - that is, of a field of application.

> If a theory 't' is given we say that a field is a field of application of the theory 't' if there exists a characteristic number 'd' of the theory 't' with respect to this field, and if in addition it satisfies certain further conditions.[63]

The characteristic number 'd' of a theory 't' in respect to a field of application, Popper sees as the dimension of 't' with respect of this field of application. The expression 'dimension' suggests itself because we can think of all possible n-tuples of the field as a spatially arranged (in a configuration space of infinite dimensions).

63, Ibid. p. 129.

If, for instance, d=3, then statements that are admissible because their composition is too low, form a three-dimensional sub-space of this configuration, transition from d = 3, to d = 2 corresponds to the transition from a solid to a surface.

> The smaller the dimension d, the more severely restricted is the class of those permitted statements which, regardless of their content, cannot contradict the theory owing to their low degree of composition and the higher will be the degree of falsifiability of the theory.[64]

The field of application as a concept is not restricted to basic statements. Singular statements of all kinds are allowed to be statements belonging to a field of application. There could be, however, an estimate of degree of composition of the basic statements by comparing their dimensions with the help of the field. (There is an assumption that to a highly composite singular statement, there corresponds highly composite basic statement). It can thus be assumed that to a theory of higher dimension, there is a correspondence of a class of basic statements of higher dimension, such that all statements of this class are permitted by the theory in spite of what they assert. The question is then answered of how the two methods of comparing degrees of testability are related - the one by means of the dimension of a theory, and the other by means of the subclass relation. There are cases in which none of the methods will be applicable. In such situation, there is no room for conflict between the two methods. If both methods are applicable in a particular case, then it will be taken that two theories of equal dimensions may yet have different degrees of falsifiability if assessed by the method

64. Ibid. p. 129.

based upon the sub-class relation. In such cases, we have to accept the verdict of the latter method as it would prove to be the more sensitive method. In all other cases in which both methods are applicable, they must lead to the same result, as it can be shown, as it could be seen with the assistance of a simple theorem of the theory of dimension, that the dimension of a class must be much higher, or equal to, that of its sub-class.

The application of dimension assist us to make a comparison of theories that previously we were unable to do. For instance, we can afford to compare a circle hypothesis with a parabola hypothesis (which is four dimensional). Each of the words 'circle', 'ellipse', 'parabola' denotes a class or set of curves, and each of these sets has the dimension 'd' if 'd' points are necessary and sufficient to single out or characterize one particular curve of the set. In the representation of algebraic system, the dimension of the set of curves depends upon the number of parameters whose values we may freely choose. It could thus be said that the number of freely determinable parameters of a set of curves by which a theory is represented is characteristic for the degree of falsifiability (or testability) of that theory.

Popper posits that Kepler owes his success partly to the fact that the circle hypothesis with which he started was relatively easy to falsify. If Kepler had started with a hypothesis that cannot be tested easily as the circle hypothesis, he might not have got any result in view of the difficulties of calculations whose basic was in the air - a drift in the skies, as it were, and moving in a way unknown. The unequivocal negative result that Kepler reached by the falsification of his circle hypothesis was in fact his first real success, avers Popper. There was a sufficient positive impact in his method, especially when his first attempt had already yielded certain approximation.

Kepler's might have been found in entirely different way. Popper sees it as no mere accident that it was by this way that success in his law was attained. It is in consonance to the method of elimination that is applicable only if the theory is sufficiently easy to falsify - sufficiently precise to be capable of clashing with observational experience.

If the degree of falsifiability of two theories is compared by the consideration of their dimension, we need to take into account their generality, that is, their invariance with respect to co-ordinate transformation along with their transformations.

In a continuous attempt to situate observation, experiment and theories where they rightly belong Popper made a comparison of laws and conventions. The two terminologies (laws and conventions) constitute the title of our next subsection.

9. LAWS AND CONVENTIONS

Popper does not believe that theories or laws have any logical status: Davies point out that,

> by this approach, Popper eliminates drastically but effectively all the difficulties of the

> general deriving from the particular. According to this view, a theory may be an inspiration, a leap of the imagination, or perhaps merely an analogy or a dream. But wherever the idea originates, whether it seems obvious or trivial, brilliant or stupid, it is a scientific theory if, and only if, it is an attempt to correlate experimental or observational quantities.65

The situation will be such that appropriate experiments could conceivably be used to disprove the theory. Scientific experiments or observations are provoked by the latter, and Popper harps on this leading role of theory. He posits that our experiments or observations are always made with some preconceived ideas (i.e. theory) of the likely nature or behaviour of the system.

A scientific theory, Popper argues, should be testable experimentally, but more so there should be some measure of simplicity or easiness in this test.

> With the possibility that many theories are silly and trivial, experiments that are carried out as easily as possible ared desirable, and even more important, as few experiments as possible should be necessary in each check of the theory. 66.

If we are to prove a theory wrong only after millions of measurements, then the scientific literature will be burdened with wild ideas meant to be tested with the hope of disproving them at a later date. This is, however, not the case mainly as a result of our desire for simple theory (which is called law often). It is always the case that simple theories are preferred if the condition for such exists. The preposition

65. J.T. Davies "The simple laws of science and History" in critical Approach to Science and Philosophy p. 255.

66. Ibid. p. 256.

of complex theory only comes about when there is no access to simple theory:

> further we usually choose relatively simple systems to investigate in detail, particularly those in which it is possible to keep all but one of the possible variables constant during the test. 67.

Analogy is an aspect of convention that Popper talks about. There seems to be a general agreement that analogy plays significant role in our theorizing. Popper points out that "arguing by analogy is bereft of any logical basis, simple analogies, however, most of the times correlate physical data."[68] Popper's principle, however, which he insists on, is that all theories should be testable with data chosen in a bid to disprove them. It is only by so doing that we are applying scientific methodology. By testing as, Kelly says, we are aiming at truth as:

> the aim of philosophy, science and other theorizing activities is to arrive at the truth, or at any rate to search for the statement which correspond with reality. 69.

Popper's emphasis is that there must be a sincere testing of our theories at least to ensure their suitability into the structure of scientific system. And as Maxwell points out too "So long as these attempts at falsification fail, we are justified in tentatively retaining and using the theory and always continuing if possible, the attempts at falsification."[70]

What Kuhn believes in is that a given dogma should dominate over a considerable period: he does not see the method of science

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67. Ibid. p. 259.
68. Ibid. p. 259.
69. Derek. A. Kelly "Popper's ontology: an exposition and critique" in The Southern Journal of Philosophy, vol. 13.1975, p. 80.
70. Grover Maxwell "Corroboration without Demarcation" in the Philosophy of Karl Popper, I ed. p. 4. Schilpp.

as consisting of bold conjectures and criticism. To Kuhn

> ... '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. 71

Besides, he believes that rationality lies on something such as a common language and a common set of assumptions. From this basis, he argues that rational discussion and criticism can only be possible if we have agreed on fundamentals.

Popper sees this thesis as false. He admits alright that at any moment we may be prisoners caught in the framework of our theories, our expectations, our past experiences, our language.

> But we are prisoners in a ... sense: if we try, we can break out our framework at any time. Admitedly, we shall find ourselves again in a framework, but it will be a better and roomier one; and we can at any moment break out of it again. 72

The issue here is really that Popper is of the view that there is always the possibility of a critical discussion and a comparison of the various frameworks. He sees it as a dangerous dogma that the different frameworks are like mutually untranslatable languages. The belief in the myth of framework, he posits, will simply exaggerate a difficulty into an impossibility. There could be some problems quite alright in discu-

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71. Thomas S. Kuhn, The Structure of Scientific Revolutions, CHICAGO, The University of Chicago Press, 1962, p. 10.

72. Popper, "Normal Science and its dangers" in Criticism and the Growth of Knowledge, Proceedings of International Colloguium in the Philosophy of Science, London 1965 Vol. 4, Ed Imre Lakatos, p. 56.

ssion among people brought up in diverse frameworks, yet, to generate discussion under such an atmosphere could be quite fruitful as well as exciting.

An intellectual revolution, Popper admits, can quite be like a religious conversion.

> A new insight may strike us like a flash of lightning . But this does not mean that we cannot evaluate, critically and rationally, our former views, in the light of new ones. 373

Which is to say that in science, in contrast to theology, there is always the possibility of a critical comparison of the competing theories. Scientific knowledge can thus be seen as a system of theories on which we carry on works just as masons do on gigantic structure like the cathedral. The main goal is to source theories which in view of critical and rational discussion, get closer to the truth. And this is the same as saying that we aim always to increase the truth content of our theories.

When theories are subjected to critical evaluation, the main aim is to have them refuted; and spurious ones suffer this fate. By a critical outlook we are led into further experiments and observations of a kind which nobody would ever have dreamt of without the stimulus and guidance both of our theories and of our criticism of them.

A scientist, notes Popper, should bear in mind while researching that science is one of the few human activities where errors are systematically criticised very often, which means

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373 Ibid. p. 57.

that we learn by trial and errors. It is such outlook to science that ensures the much cherished progress in the dis-cipline. As to the nature of progress in science, there is no complexity in assessing this. Once a theory is able to pass through specified tests, such a theory will be seen as having progressed more than the others that fell on the way side.

> ...we know what a good scientific theory should be like, and - even before it has been tested - what kind of theory would be better still, provided it passes certain crucial tests. And it is this (meta-scientific) knowledge which makes it possible to speak of progress in science, and of a rational choice between theories. 74.

Popper reasons that it is not virtuous to be cautious in avoiding errors but to be ruthless in aliminating them. There should be boldness in conjectures on the one hand and austerity in refutations, on the other. As Lakatos points out,

> intellectual honesty does not consist in trying to entrench, or establish one's position by proving (or probabilifying') it - intellectual honesty consists rather in specifying precisely the conditions under which one is willing to give up one's position. 75.

This makes for progress in science, reason Popper

CRITICAL SUMMARY

Theories, their corroboration and falsification are the main issues of Popper's philosophy of science which we examined in this chapter. Popper believes that as a dynamic enterprise scientific theories are subject to constant changes. There is no absolute theory in science.

74. Popper Conjectures and Refutations, p. 217.

75. Imre Lakatos "Methodology of Scientific Research Programmes" in Criticism and The Growth of Knowledge, Proceedings of The International Colloquim in the Philosophy of Science London, 1965, vol. 4 Ed. Imre Lakatos p. 92.

Scientists are continuously attempting to falsify existing theories through the process of testability. Theories that could not be falsified after crucial tests are deemed **corroborated**.

Science, argues Popper, progresses by trials and errors. And it is by constant and severe criticism that we get closer to scientific truth. Nonetheless all laws and theories he sees, as hypothetical, as there is always the possibility of having an existing theory falsified.

Most of the ideas of Popper highlighted in this chapter are, no doubt, controversial. His conception of dynamism in science is very puzzling. If he sees dynamism in terms of constant changes of theories which might still be very fruitful, then such dynamism will be awkward as it will rather be antithetical to the development of science. Dynamism should be perceived in the positive sense, that is, allowing only ideas that are fruitful and workable to stay, irrespective of the number of years such have endured. Science, in actual fact, does not really undergo the kind of changes or revolution that Popper talked about. Theories in science take time to evolve. Scientists on their own part keep on revering a prevailing theory, maximising its uses until such a time such a theory becomes absolete. Revolution in science as Popper harps takes place once in a while like the Copernican revolution he often cites as an example.

But in other instances we will discover that a given discovery lasts for centuries, without a blemish found in it. In

In this respect we can recall the break-through that Lavoisier made in the field of Chemistry in combustion which has lasted for centuries, We can also recall the break-through made by Faraday in the field of electricity and molecular chemistry. As far back as 1832 Faraday was able to reduce the matter of electrolysis to quantitative terms by "announcing what are now called Faraday's laws of electrolysis."[76] This discovery is valid even up till today after more than a century. In the field of Physics Dalton's atomic theory held sway for centuries before atom could be split in the early part of this century. Also the discovery of Sabriel Fanrenheit in respect of the freezing point of water which he puts at 32°f and the boiling point which he puts at 212°f since the 17th century has remained valid in the field of Physics, Chemistry and Biology. Thus the kind of revolution that Popper talked about in terms of constant changes is not a common feature in science. There could be changes in theories or better put modifications based on new findings, just like we had in Stahl Phlogiston thesis but one point that is obvious is that theories in science endure for quite sometime.

It is surprising too that Popper failed to see scientific knowledge in terms of cummulative laws. As Zahar points out,

> Popper offers a Darwinian account of the progress of knowledge. Progress is suppose to result negatively from the elimination by

76. Asimov, P. 281.

natural selection of defective alternative.[77] This is unfortunately not the case. Scientific research is a continuous and co-operative event, one theory leads to the other, one idea illuminates the other. For instance Galileo discovered the principle of Pendulum. But he could not solve the problem of correct timing. This problem could not be solved until the time of Huygens, Christian who used Galileo's principle of pendulum as a means to regulate the clock. In the same vein, it is known that Dalton's atomic theory was a welding of Democritus' thesis on atom which was purely speculative and Lavoisier's quantitative method in chemistry for he brought in the ideas of mass and measurement in atomic theory. In the same vein, Einstein's work climaxed the famous experiment of Michelson and Morley who had been a unable to detect any difference in the velocity of light with changes in its direction through other. Einstein pursuing the same research was able to establish that light travelled in quanta, and therefore had particle-like properties and was not merely a wave that required some material for its propagation. Einstein also points out that without the other there was certainly nothing in the universe that could be viewed as at absolute rest' nor could any motion be considered as absolute motion. All motion, he argues, "was relative to some frame of reference chosen."[78]

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77. Eli Zahar "Logic of Discovery or Psychology of Invention" in the British Journal for the Philosophy of Science, Vol. 34, No. 3, September 1983

78. Asimov - p. 871.

In essence scientists do not work merely to upset the existing ideas, but rather to make use of the later to achieve a more noble goal. The advice of Francis Bacon many years ago agrees so much with what scientists do in their research efforts. He advises thus. "When a person prepared himself for discovery he first obtains a full account of all that has been said on the subject by others"79

In this regard, an American inventor is reported by Graham as saying that,

> When I want to discover something I begin by reading everything that has been done along that line in the past. That's what all those books in the library are for. 80

The essence of making this reference is not to refute the existing idea but to use it as a guide for further discoveries.

Where a change, in idea becomes most necessary is where there are some unsettling facts or where the ultimate aim of a research is most unlikely to be reached by the existing idea. Lavoisier's thesis on combustion came up to rectify the basic problems that Stahl Phlogiston thesis had. Einstein's theory of relativity was to rectify the absolute thesis of Newton's laws of motion.

Popper's view on the essence of evidence is also intriguing. He sees evidence not in the positive light that is, not in terms of supporting a theory but rather as a means to refute it. This, is really not the case in actual scientific research. There are in actual fact more efforts to support than to refute

79. Graham, etal, vol. 3, p. 407
80. Ibid. p. 407.

In fact, more often than not researchers are continuously making effort to break new grounds so that there will be increase in laws in science, not really making efforts to refute existing theories. As Quine rightly points out"... a scientific theory consists of laws in conjunction."[87] And there is a kind of continuity in research programme. It is difficult for us to accept that science grows by revolutionary overthrow of theories as Popper argues, but in a continuous accumulation of theories and improvement on them. The aim of a researcher is to solve a puzzle not to refute an existing theory. It looks a negative effort if the whole times and resources of a researcher are wasted in refuting an existing theory.

It is difficult too to posit, that science progresses by trial and error. This is not acceptable. A serious research in science is purposeful, it has an aim, a focus. This is part of what informs the rationality of science. It is amazing that Popper sees rationality as an attribute of science and yet argues that progress in science is by trial and error. It was not by trial and error that Faraday invented the first ever electric transformer, and generator. It was not by error that Huygens found a method of regulating the clock. It was not by error that nuclear bomb was discovered. Nuclear bomb which was a joint effort of Hahn, Meitstetner Szillard and Einstein was seen as a project to be embarked on and possibly get executed. In fact Einstein had to write

87/ W. V. Quine, "Popper's Negative methodology" in the Philosophy of Karl Popper, Book I, p. 220.

"a letter to President Franklin Roosevelt urging him to put into effect a gigantic research program designed to develop a nuclear bomb."[82] Most discoveries in science take this pattern. Once in a while, however, a discovery could be by accident. For instance, it was by accident that Sir Humphy Davy discovered Nitrous oxide which is often called laughing gas (it was used as the first chemical anaesthetic). It was in the course of inhaling the gases he was producing in his experiments that he noted the peculiar character of Nitrous oxide. But this kind of accidental discovery takes place once in a while. It is not enough to generalise as Popper did that science progresses by trial and error.

Also it is difficult to agree with Popper that all science theories are to be seen as hypotheses or conjectures (as he, at times, calls them). Conjectures (he calls them guesses too) as they are understood are like propositions that are yet to be tested and corroborated. In science, hypotheses are ideas that are proposed which are yet to be tested and corroborated. But once a hypothesis has been tested and corroborated it becomes a law or a theory, and no longer a hypothesis as Popper wants us to believe. A proposition in science does not remain as a hypothesis for ever as Popper argues. This does not conform with the purpose of science which as Mackinnon points out

> aims at producing claims that are objectively true and then using these as stepping stones to further claims. 83

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82. Asimov. p. 871
83. Edward Mackinnon "The truth of scientific claims" in the <u>Philosophy of Science, official Journal of Philosophy</u> of Science Association, Volume 49, No. 3, June, 1982, p. 438

Ideas are obviously guides to action. If all laws and theories are conjectures they cannot be relied upon. All knowledge cannot be seen as provisional. There should be a distinction between knowledge and conjectures. As Putmam rightly points out,

> a theory, as the term is actually used is a set of laws. Laws are statements that we hope to be true, they are supposed to be true by the nature of things and not just by accident. 84

It is quite wrong for Popper to equate laws with hypotheses. If his proposal is accepted then it becomes difficult to differentiate a proposition that has been tested and corroborated and those yet to be tested. As Putnam points out again,

> scientific theories are shown to be correct by their successes just as all human ideas are shown to be correct, to the extent that they are by their success in practice. 85

It can only be seen as a wrong use of terminologies for Popper to equate hypotheses with laws and theories.

It is not quite correct for Popper to insist that truth could not be reached. What can be true anyway is that no truth can be seen to be absolute. What is true today may not be the case tomorrow. But as long as there is no evidence to see it as nothing but the truth, we can take it that we have reached the truth at that point in time. That we rely on a piece of knowledge as a guide to actions is because we

84 Hilary Putnam, "The corroboration of theories" in the Philosophy of Karl Popper, p. 226.

85. Ibid., p. 229.

see it as truth. However as Mackinnon points out "future break-throughs and conceptual revolution may undermine claims now held to be true"[86] But until this happens we continue to see a true knowledge as nothing but the truth.

Also we cannot see why Popper takes falsification as a special attribute of empirical science. Falsification is rather to be seen as a negative factor in science. Irrefutability is to be thus seen as an asset instead of as a liability in science. It is rather difficult for one to see why Popper decided to use falsification as a criterion of demarcation between science and metaphysics. Once a theory is falsified it has ceased to be a guide to any scientific investigation: One then wonders why this should be a valuable factor to evaluate the scientific status of a given system of knowledge.

Testability to Popper is the same as falsifiability. When we are testing a theory we are at the same time attempting to refute it. This thesis is not appealing to us at all. A scientist may have diverse reasons for embarking on testing. It could be to ascertain the ingenuity of an existing theory, or to evaluate theory in view of recent findings. Also when a research is going on the researcher constantly carries out tests to corroborate the propositions he made prior to the research. As Bernays points out "testing does not generally have the character of searching for refutations".[87] If a scientist

86. Mackinnon, p. 3 438.

87. Paul Bernays "Reflections on Karl Popper's Epistemology" in the Critical Approach to Science and Philosophy p. 41.

searches always for refutations whenever he tests, it means testing has negative attribute.

Be that as it may our attention shall now be focused on a critique of Popper's methodology of science and alternative methods of science suggested. Kuhn, Feyerabend and Lakatos are the Philosophers of science to be examined in this respect.

CHAPTER FOUR

REACTIONS TO CRITICS

Popper's thesis on the methodology of science is no doubt controversial. It has, as a result, generated a lot of interest from different schools of thought that are interested in the development of scientific knowledge. An interesting thing about the reaction to Popper's work is that most of his critics are those that knew him intimately either as a colleague or a teacher. Others are the set of people that were attracted to his work because of the intellectual reflection it generates. In spite of whatever misgivings most of Popper's critics may have about his thesis they still acknowledge where there is agreement between their line of thought and Popper's. In this section of the research, we shall be pre-occupied with the critical appraisal of Popper's work as witnessed in the works of Thomas Kuhn, Paul Feyerabend and Imre Lakatos. We shall also evaluate critically the scientific methodology, they advocated. We shall for the sake of clarity start with the methods they favour in this order: Thomas Kuhn and his conception of normal science, Feyerabend and his notion of scientific anarchism and Lakatos Hard-Core notion of science.

(1) THOMAS KUHN AND HIS CONCEPTION OF NORMAL SCIENCE

As we shall see later Kuhn is convinced that the central point in Popper's thesis - falsification - is a misrepresentation of what scientists do in their day to day activities. What a scientist does, according to Kuhn, in his daily enterprise amounts to what he calls 'normal science.' Any scientist that does not pre-occupy himself with spurious scientific research must meet up to the requirements of 'normal science.' To Kuhn, 'normal science' means the research that is firmly rooted upon one or more past scientific achievements. Such achievements must be acknowledged by a given scientific community for a given point in time and it is such that provides the basis for further scientific enterprise. This kind of achievements, he argues, are today recounted, though not often in their original form, by science textbooks - elementary and advanced. Such books expound the body of accepted theory, illustrate many or all of its successful applications and compare these applications with exemplary observations and experiments.

Some accepted instances of actual scientific practice, according to Kuhn, which include law, theory, application and instrumentation give models from which emanate particular coherent tradition of scientific research. A scientist possesses two essential attributes." Their achievement was sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. Simultaneously, it was sufficiently open-ended to have all sorts of problems for the refined group of practitioners to resolve."[1]

1. Thomas S. Kuhn, The Structure of Scientific Revolution, p. 10.

Achievements that share these characteristics, Kuhn calls, paradigms' a term which he says relates closely to 'normal science.'- These constitute the tradition that the historian describes under such rubrics as 'Ptolemanic astronomy' or 'Copernican' or 'Aristotelian dynamics' or 'Newtonian Corpuscular optics' or 'wave optics.'

The study of paradigms including many that are for more specialized than those already stated above is what fundamentally prepares the students for membership in a given scientific community in which he will later practice.

> "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 prerequisite for normal science i.e. for the genesis and continuation of a particular tradition." [2]

The ability to be acquainted with a paradigm in addition to the more esoteric type of research it permits signify maturity and advancement in the development of any scientific field. For instance the wave theory is embraced by almost all the practitioners of optical science. In the 18th century, however, the paradigm for this field was provided by Newton's opticks which taught that light was material corpuscles. "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 development pattern of mature science." [3] In biology, for instance, part of its study centres on the study of heredity which is a most universally received paradigm. The essence of having paradigms in the view of Kuhn is essentially because history reveals that the

2. Ibid, p. 11

3. Ibid, p. 12

road to a firm research consensus is extraordinarily arduous. Where there are no paradigms all the facts that relate to the development of a given science may seem equally relevant.

We cannot interpret any natural history in the absence of at least some implicit body of intertwined theoretical and methodological belief that permits selection, evaluation and criticism. If it is the case that the body of such belief is not already implicit in the collection of facts — in which case more than 'mere facts' are at hand — it must be externally supplied perhaps by a current metaphysics, by another science, or by personal and historical accident. It is basically for this reason that in the early stages of the development of any science different men confronting the same range of phenomena (but not usually all the same particular phenomena) describe and interpret them in different ways. Before a theory is to be accepted as a paradigm it simply has to be seen to be better than its rivals; but it does not and in fact never does it explain all the facts with which it can be confronted.

In the development of a natural science, once a new school of thought emerges which is strong enough to attract members of the scientific community the older schools gradually fizzle out. This is partly as a result of the conversion of the members to the new paradigm. This does not mean that there are no such men that stubbornly stick to the older methodology. But the outcome of such strong-headedness is that such men are no longer reckoned with in the profession as they have lost touch with the present realities and the new trends.

This view is informed by Kuhn's belief, as Keita points out, that "progress in science is often punctuated by revolutions in the theoretical assumptions of the particular sciences themselves."[4] The emergence of a new paradigm serves as a new and more rigid definition of the field. Whoever that is not prepared to accommodate his work to it must proceed in isolation or alternatively attach himself to some other group. Kuhn asserts that going by the record of history that this set of people have often stayed in the departments of philosophy from which so many of the special sciences have been spawned. A group that is merely interested previously in the study of nature are suddenly transformed into a profession or Discipline by their reception of paradigm.

> In the sciences (though not in fields like medicine, technology, and law of which the principal raison d'etre is an external social need) the formation of speciqlized 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. 5

Kuhn argues that this was the state of affairs between a century and a half ago, when the institutional pattern of scientific specialization first developed and the very recent time when the paraphernalia of specialization acquired a prestige of their own.

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4. L. Keita "Progress in Science: A Critique of Kuhn's "Incompatibility Thesis" of Scientific Change, In the Nigerian Journal of Philosophy, Vol. 4, Nos. 1 & 2, Vol. 5, Nos. 1 & 2, 1984, 1985, p. 26

5. Kuhn, The Structure of Scientific Revolution, p. 19

There are other consequences of a rigid definition of the scientific group. It makes an individual scientist to take a paradigm for granted. As a result, he has no further needs in his major works to attempt to build his field from the scratch, starting from the first principles and justifying the use of each concept introduced. Works of this nature could be left to the writers of textbooks according to Kuhn. With the availability of such textbooks, a creative scientist can begin his research where it leaves off and thus concentrate exclusively upon the subtles and most esoteric aspects of the natural phenomena that concerns his group. In the process of this, his research communiques will begin to change in ways whose evolution has been too little studied but whose modern end products are obvious to all and oppressive to many. His research will henceforth appear as brief articles addressed only to professional colleagues, the men whose knowledge of a shared paradigm can be assumed and who prove to be the only ones able to read the papers addressed to them.

Kuhn says that in science today, books are usually either tests or retrospective reflections upon one aspect or another of the scientific life. Only in those fields that still retain the book with or without the article as a vehicle for research communication are the lines of professionalisation still so loosely drawn that the layman may hope to follow progress, by reading the practitioners original report. Both in the field of mathematics and astronomy research reports had ceased already in antiquity to be intelligible to a generally educated audience.

Kuhn does not believe that there can be a progressively, and in fact, successful practice of a given science in the absence of a paradigm. As a matter of fact, as Newton-Smith points out "Kuhn's talk of a paradigm is meant to direct our attention to those common factors, reference to which is required in explaining the behaviour of the scientists." [7] By the ability to focus attention upon a small range of relatively esoteric problems the paradigm forces scientists to investigate some part of nature in a detailed form which otherwise would have been unthinkable. And normal science possesses a built-in mechanism but that ensures the relaxation of the restriction that bound researc whenever the paradigm from which they derive 'ceases to function effectively.' Given this situation scientists begin to behave differently and the nature of their research problems changes. In the interim, however, during the period when the paradigm is successful the profession would have solved the problems that its members could scarcely have imagined and would never have undertaken without commitment to the paradigm. At least part of that achievement always proves to be permanent.

As regards scientific revolutions, Kuhn sees it in terms of a widening sense but often restricted to a narrow subdivision of the scientific community. And a sense of malfunction that can lead to crisis is necessary for any such revolution in scientific development. Thus, to Kuhn, as Feyerabend points out

> ... a revolution occurs whenever a new research programme has accumulated a sufficient number of successes and the orthodox programme suffered

7. W. H. Newton-Smith, *The Rationality of Science*, p. 164.

> a sufficient number of failures for both to be considered as serious rivals, and when the protagonists of the new programme proclaim the demise of the orthodox view. 8

Besides, scientific revolution seem revolutionary only to those whose paradigms are effected by them. To outsiders, they may seem normal parts of the developmental process: astronomers, for instance, could accept X-rays as a mere addition to knowledge, for their paradigms were unaffected by the existence of the new radiation. But for men like Kelvin, Crookes and Roentgen, whose research dealt with radiation theory or with cathode raytubes, the emergence of x-rays necessarily violated one paradigm as it created another: which explains why these rays could be discovered only through something first going wrong with normal research.

In Kuhn's methodology, as we have seen, paradigm plays a key role in scientific research. For a discipline to be worthy of being classified as a science, it must have a paradigm as Tornebohm, points out, that are "... factors which direct and control work done within a field of research."

1(1) ON POPPER'S METHODOLOGY

Thomas Kuhn's opinion about Popper's thesis is very straight forward. Besides making his stance clear in his work entitled The Structure of Scientific Revolution, as we have already seen he contributed an Essay in a collection of Essays on Popper's Philosophy edited by Paul Schilpp entitled "Logic of Discovery or Psychology of Research." In this paper he confesses his admiration for Popper's work, points out areas where he shares his ideas; and moreover stresses where he differs from him. He believes ...there is the

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8. Paul Feyerabend, Against Method, p. 198.

9. Hakan Tornebohm "Inquiring Systems and Paradigms" In Boston Studies In The Philosophy of Science, Essays In Memory of Imre Lakatos, Volume 39, p. 635.

need for him to have this kind of critical outlook for Popper's thesis so that scholars and Popperians that have hitherto tried to pigeonhole his ideas into Popper's will be able to have a clearer picture of what his stance on the methodology of science is all about. He did not fail to remark, however, that there are substantial areas of agreement between the two of them. He writes:

> We are both concerned with the dynamic process by which scientific knowledge is acquired rather than the Logical structure of the products of scientific research. Given that concern both of us emphasize as legitimate data facts and also the spirit of actual scientific life, and both of us turn often to history to find them. 10

He remarks also that Popper and himself were similarly opposed to the classical thesis of the positivists which centres on their (positivists) principle of verification. He points out too that he shares with Popper "the intimate and inevitable entanglement of scientific observation with scientific theory."[11] The two too, Kuhn stresses, were skeptical of efforts to produce any neutral observation language; in that scientists should carefully aim to invent theories that explain phenomena. The most fundamental agreement, says Kuhn, which he shares with Popper is "their insistence that an analysis of the development of scientific knowledge must take account of the way science has actually been practiced."[12]

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10 Thomas S. Kuhn "Logic of Discovery Or Psychology of Research" in the Philosophy of Karl Popper, Book II, p. 798.

11 Ibid, p. 798

12 Ibid. p. 800

Outside the above areas of agreement, Kuhn raises objections in other areas of Popper's works. He does not see himself agreeing with Popper that,

> a scientist whether theorist or experimenter, puts forward statements or systems of statements and tests them step by step. In the field of the empirical sciences, more particularly, he constructs hypotheses or systems of theories and tests them against experience by observation and experiment. 13

Kuhn sees this assertion by Popper as ambiguous. He sees it this was in so far as there is no clarity as to which of the two sorts of 'statements' (guesses) or theories are being tested. Kuhn argues further that the generalisation inherent in the statement is historically faulty. He reasons too that the assertion lacks the essential feature of scientific practice that almost make a world of difference between the sciences and other creative practice. He believes that there is just one sort of statement or hypothesis that scientists do really and repeatedly subject to systematic test. These are "statements of an individual's best guesses about the proper way to connect his own research problem with the corpus of accepted scientific knowledge."[14]

A scientist, according to Kuhn, may, for instance, conjecture that a given chemical unknown, contains the salt of a rare earth, that the rats he uses for experiment are fat due to a specified component of their diet, or that a newly

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13. Popper, The Logic of Scientific Discovery, p. 27

14. Kuhn "Logic of Discovery or Psychology of Research" in the Philosophy of Karl Popper, Book II, pp. 800 - 801.

found spectral pattern is to be understood as an effect of nuclear spin. In each of the stated instances the next stage of his research is specifically to test the conjecture or the hypothesis. If there is a positive outcome from the tests - that is, if his conjecture survives reasonable tests - it will then be deduced that he had made a discovery or at least resolved a puzzle. If in contrast the outcome of the test is negative he can then abandon the whole thing or try another hypothesis in his resolute attempt to resolve the puzzles. Kuhn is not insisting by this that all research procedures take this form, but that tests of this nature is what he calls 'normal science' or 'normal research', an enterprise that accounts for the substantial work done in the basic sciences.

> In no usual sense, however, are such tests directed to current theory. On the contrary when engaged with a normal research problem the scientist must premise current theory as the rules of his game. His object is to solve a puzzle preferably one at which others have failed, and current theory is required to define that puzzle and to guarantee that given sufficient brilliance it can be solved. 15

Whoever that is involved with this enterprise has to test often the conjectural puzzle-solution that his ingenuity suggests. Whatever is tested represents only his personal conjecture. If there is a failure, it is only his ability that has failed and not the 'corpus' of current science.

It is the belief of Kuhn that the above procedural test identifiable with his thesis is quite different from the kind of test that Popper advocates. Popper, he argues,

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15. Ibid. p. 801

was more interested in the procedure that enhances the growth of science and such growth in Popper's view is achieved not by accretion but by "the repeated overthrow of scientific theories and their replacement by better or more satisfactory ones."[16] From this Popper's assertion Kuhn concludes that by tests he (Popper) had in mind those performed for the purpose of carrying out an exploration of the limitations of accepted theory or to subject a current theory to maximum strain.

Popper also, according to Kuhn, tends to characterise the totality of scientific enterprise in respect of its occasional revolutionary parts, like the eclipse expedition of 1919. Kuhn sees such episodes as very rare in the development of science. At any point in time they occur they are generally called forth either by a priori crisis in the relevant field or by the existence of a theory that competes with the existing canons of research. Instances include the exploits of Copernicus or Einstein's theory of relativity. Kuhn sees this as 'extraordinary' research, an enterprise where scientists do display many of the features Popper talks about. "Normal Science does not aim at novelties of fact or theory and when successful finds none".[17] Kuhn reasons further that neither science nor the development of knowledge is likely to be comprehended if research is seen exclusively through the revolutions it occasionally produces. Testing

16. . Popper, Conjectures and Refutations, The Growth of Scientific Knowledge, p. 215

17. . Kuhn, The Structure of Scientific Revolution, p. 52.

of basic commitments, he argues, occurs only in extraordinary science: it is normal science that discloses both the points to test and the manner of testing. It is equally for the normal science to ensure that professionals are trained. This is not one of the features of extraordinary science. Kuhn believes that the kind of testing that Popper talks about does not exist in the field of normal science. Such can only be found in the extraordinary science which most nearly distinguishes science from other enterprises.

Kuhn also dissents against Popper's recommendation of a critical attitude to science. He (Kuhn) traces the origin of this ideas (critical attitude) to the Greek philosophers between Thales and Plato. He rejects this as he believes that by

> the Hellenistic period, mathematics, astronomy, statics and the geometric parts of optics had abandoned this mode of discourse in favour of puzzle solving. Other sciences in increasing numbers have undergone the same transition since. 18

He believes that it is the abandonment of critical attitude that have informed the path of science rather than its cultivation. And once there is this transition for a field of study,

> it is ... in periods of acknowledged crisis that scientists have turned to philosophical analysis as a device for unlocking the riddles of their field. Scientists have not generally needed or wanted to be philosophers. Indeed normal science usually holds creative philosophy at arms length. 19

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18 [illegible] Kuhn "Logic of Discovery or Psychology of Research" in the Philosophy of Karl Popper, Book II, p. 802

19 [illegible] Kuhn, The Structure of Scientific Revolution, p. 87.

Scientists only behave like philosophers when the need arises for them to choose between theories.

Popper's demarcation criterion by which he sees astrology as a non-science also attracted Kuhn's critical attention. He disagrees with Popper's assertion that astrologers by:

> making their interpretations and prophecies sufficiently vague they were able to explain away anything that might have been a refutation of the theory, had the theory and the prophecies been more precise. In order to escape falsification they destroyed the testability of their theory.[20]

In spite of whatever he thinks about Popper's demarcation criterion as it affects astrology, Kuhn still sees the above assertion of Popper as actually capturing something, of the spirit of enterprise of astrology;

> "but taken at all literally as they must be if they are to provide a demarcation criterion, they are impossible to support. The history of astrology during the centuries when it was intellectually reputable, records many predictions that categorically failed.[21]

Thus Kuhn does not see why astrology should be barred from the field of science basically for the form in which its prediction were cast. They (astrologers) cannot be barred too from scientific enterprise because of the way its practitioners explained their failures. They (Astrologers) point out, for instance, that to predict the future of any given individual is an uphill task which recommends care and extreme sensitiveness to minor errors in relevant data. The reasons for this are varied. One of which is the fact that there are

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20. K. R. Popper, Conjectures and Refutation, The Growth of Scientific Knowledge, p. 37

21. T. S. Kuhn "Logic of Discovery or Psychology of Research" in the Philosophy of Karl Popper, Book II, p. 803.

always changes in configuration of stars as well as the eight planets. Besides, the astronomical tables that are used for the computation of individual births are quite imperfect. It is only very few men that are aware of the exact date of their birth. It is partly for this reason that forcast failed. Similar arguments, according to Kuhn, are still used today to explain failures even in medicine or meteorology. In periods of upheaval they are also deployed in the exact sciences such as physics and chemistry. It is thus the stance of Kuhn that there is nothing unscientific about the astrologers explanation of failure.

Despite the position already taken by Kuhn, he still goes on to reason along with Popper that astrology is not a science. He sees it rather as "... a craft of one of the practical arts, with close resemblances to engineering, meteorology and medicine, as these fields were practiced until little more than a century ago."[22]

Astrology can thus be likened very intimately to the old medicine or psycho-analysis. An Astrologer is incapable of carrying out a research: in spite of the fact that he has rules to apply, he lacks puzzles to solve and invariably no science to practice. And in the absence of puzzles sufficient enough to challenge and test the ingenuity of the individual practitioner, astrology could not have become a science even if the stars are in fact in full control of human destiny. Thus,

> though astrologers made testable predictions and recognized that these predictions sometimes

22. Ibid. p. 804

> failed, they did not and could not engage in the sorts of activities that normally characterise all recognised sciences. Sir Karl is right to exclude astrology from the sciences; but his over-concentration on sciences occasional revolutions prevents his seeing the surest reason for doing so.[23]

Another area of Popper's thesis where Kuhn joins issues with him was on his position that tests precede the replacement of a theory. Kuhn contends that in some instances tests are not prerequisite to the revolution through which science advances. The situation differs, however, when puzzles are involved. In this, "with or without tests a puzzle-solving tradition can prepare the way for its displacement.. To rely on testing as the mark of science is to miss what scientists do and with it, the most characteristic feature of their enterprise."[24]

Kuhn also opposes Popper's thesis that we can learn from our mistakes. His rejection of this hinges on his reasoning that the kind of mistake that Popper's imperative most obviously applies are individual's failures of understanding or of recognition within an activity governed by pre-established rules. In the sciences such mistakes take place most frequently and perhaps exclusively within the practice of normal puzzle-solving research. Kuhn however believes that it is not in this direction that Popper seeks the mistakes,

> for his concept of science obscures even the existence of normal research. Instead, he

23. Ibid. p. 805
24. Ibid. p. 805

> looks to the extraordinary or revolutionary episodes in scientific development. 25

Kuhn also accuses Popper of often citing obsolete theories such as the Ptolemnic astronomy, and the Phlogiston theory (as examples of mistakes) and by learning from our mistakes he meant what takes place when a scientific community rejects one of these theories and replaces it with another. Kuhn dismisses this as old usage: he believes that no mistake was made in arriving at the Ptolemnic system, which goes to confuse him on what he (Popper) meant when be calls that system or any other outdated system a mistake. Granted there were mistakes they are the normal ones which a Ptolemnic (or a Copernican) astronomer makes within his system, perhaps in observation, calculations or the analysis of data. Such mistakes could be isolated and corrected immediately, and by so doing leaving the original systems intact.

> In Sir Karl's sense, on the other hand, a mistake infects an entire system and can be corrected only by replacing the system as a whole. 26

Kuhn believes this is not the case in scientific investigation.

Popper's falsification theory was also negatively received by Kuhn. He sees it as antonyms of proofs. His objection was not with the fact that all experiments can be challenged, either to ascertain their relevance or their accuracy. His contention is that there is the possibility of modifying any theory by a variety of ad hoc adjustments without ceasing to be in their main lines the same theories. There is the need

25. Ibid. p. 806

26. Ibid, p. 807

for the situation to be this way, as it is often by challenging observation or adjusting theories that the growth of scientific knowledge can be achieved. Kuhn sees challenges and adjustments as standard part of normal research in empirical science. Even ininformal mathematics, he believes, adjustments as standard part of normal research in empirical science. Even in informal mathematics, he believes, adjustments play a significant role. Kuhn accusses Popper of barring conclusive disproof, and yet failed to provide substitute for it. The much he could provide according to him (Kuhn) was a naive falsification. Kuhn thus asserts that "though he (Popper) is not a naive falsificationist, Sir Karl may, I suggest legitimately be treated as one." 27.

It is the view of Kuhn that the criterion applied by scientists to determine the validity of an articulation or an application of existing theory are not by themselves sufficient to determine the choice between competing theories. As a result he concludes that Popper,

> has erred by transferring selected characteristic of everyday research to the occasional revolutionary episodes in which scientific advance is most obvious and by thereafter ignoring the everyday enterprise entirely. In particular he has sought to solve the problem of theory choice during revolutions by logical criteria that are applicable in full only when a theory can already be presupposed. 28

Our methodology must agree with the routine activities

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27. Ibid. p. 809

28. Ibid. p. 813.

of scientists. Such activities must agree with what he called normal science.

2. FEYERABEND AND HIS NOTION OF ANARCHISM

Feyerabend was very vehement in his attack on method, through which he debunks the idea of rationality of science. He was similarly opposed to the tradition of searching for a system of rules which it is believed are meant to guide scientists in the choice of theory. There are no such rules according to him, and attempts to adopt any particular rule or system can only impede the advancement of science. He says

> ...the idea of a fixed method or of a fixed theory of rationality, rests on too naive of a view of man and his social surroundings. To those who look at the rich material provided by history, and who are not intent to improverishing it in order to please their own instincts, their craving for intellectual security in the form of clarity, precision, "objectivity, truth" it will become clear that there is only one principle that can be defended under all circumstances and in all stages of human development. It is the principle: anything goes. 29.

29. Feyerabend, Against Method, p. 28.

main, as his work predominantly constitutes theoretically the initial planning to the finishing touches in the laboratory.

> This is well illustrated by cases in which the theoretician succeeded in predicting an observable effect which was later experimentally produced: perhaps the most beautiful instance is the Broghels prediction of the wave-character of matter, first confirmed experimentally by Davidson and Germer. 45.

A theorist is pushed to look for a better theory as a result of the experimental falsification of a theory, so far accepted and corroborated. And in fact, as Chalmers put it,

> tests of a bold, highly refutable theory that result in a confirmation of the theory are informative because of the unlikelihood of the result as assessed prior to the tests. 46

We do obviously accept one theory instead of the other. The question is why do we choose one theory instead of the other. That we have chosen a theory cannot be as a result of the experimental justification of statements composing the theory, and it is neither not as a result of a logical reduction of that theory to experience. That we choose a theory is obviously because it surpasses the other competing ones in competition. The one we select is usually that that excells the others, that prove to be the fittest in natural selection which has the greatest potential for survival. Such a theory should be capable.

45. Ibid. p. 108.

46. A. F. Chalmers "On learning from our mistakes" in the British Journal for the philosophy science 1973, vol. 24.

A hypothesis that contradicts well-confirmed theories or well-established experimental results could be used. To examine the principle in concrete detail necessitates tracing the consequences of counter-rules' that oppose some familiar rules of the scientific enterprise. He advises further that if we are genuinely interested in the advancement of scientific knowledge there is the need for us to proceed counter - inductively. He believes that some of the most important formal properties of a theory are found by contrast and not by analysis. As Madan points out, Feyerabend) "stresses the approximate nature of all theory, thereby arguing in favour of the possibility of the existence of mutually inconsistent factually adequate theories."[30] And for a scientist to maximise the empirical content of the views he holds and for him to clearly appreciate them very well, he needs to introduce other views - and this he could achieve by the adoption of a pluralistic methodology. There is the need for him to make a comparison between ideas: he needs also to improve the views that have failed in the competition rather than throwing such away. By adopting this procedure, he will be able to retain the theories of man and cosmos as found in the genesis or in the Pimander. Such could be expanded and made use of to measure the success of evolution and other modern views. The point will then become clear to him that the thesis of evolution is not as good as is generally assumed, and the need for it to be supplemented or totally replaced by an improved version of genesis. The

30 Dilip B. Madan "Inconsistent theories as scientific objectives" in the Philosophy of Science, Official Journal of the Philosophy of Science Association, Vol. 45, No. 3, September, 1978, p. 453.

knowledge that is acquired by this procedure,

> is not a series of self-consistent theories that converges towards an ideal view, it is not a gradual approach to the truth. It is rather an over-increasing ocean of mutually incompatible (and perhaps even incommensurable) alternatives, each single theory, each fairy tales, each myth, that is part of the collection forcing the others into greater articulation and all of them contributing via this process of competition to the development of our consciousness. 31

Nothing is to be seen as capable of attaining final settlement, just as no view can ever be omitted from a comprehensive account. He (Feyerabend) was of the opinion that all views are worth serious consideration. As a result, the views of the experts, laymen, professionals, dilettanti, truth-freaks and even liars are all invited to the enrichment of our culture. 32

He abhors the view that the task of a scientist is to search for truth (as Popper advocates),

> or 'to praise god' or 'to systematise observation' or 'to improve predictions.' These are but side effects of an activity to which his attention is now mainly directed and which is 'to make the weaker case the stronger' and thereby to sustain the motion of the whole. 33

There are state of affairs when we can see things as they are and others when we are deceived. What can be deduced from this is that some of our sensory impressions are veridical while others are not. And prejudices can only be noted by contrast not by analysis. The material at the disposal of a scientist, his most sublime theories, in addition to his most sophisticated technique

31. Ibid, p. 30
32. Ibid, p. 30
33. Ibid, p. 30

are structured exactly in the same way. It embodies principles that are not known, which if known would be most difficult to test. What this goes to establish is the fact that a theory clashes with the evidence is not because it is incorrect, but because the evidence is contaminated.

Feyerabend believes that we cannot discover from inside what we have been using and the kind of world we presuppose. To be able to accomplish this, an external standard of criticism is required. There is similarly the need for a set of alternative assumptions, a dream-world so as to unveil the features of the real world. We need the invention of a novel conceptual system which suspends or clashes with the most carefully established observational results, confounds the most plausible theoretical principles and introduces perception that cannot form part of the existing perceptual world. This procedure is seen by Feyerabend as counter-inductive: though he remarks that by this he was not recommending any methodology but merely intends to harp on the fact that any method has its limit. This limit can be shown, even the irrationality of the rules that are taken to be basic. He sees an anarchist as an undercover agent that plays the game of reason so as to under-cut the authority of reason: truth, honesty, justice, among others.

There is no sense in the emphasis for consistency, which requires that new hypotheses have to agree with accepted theories. This insistence lacks merit for adhering to it is like preserving the old theories at the expense of better ones. But when a hypothesis contradicts well-confirmed theories, it gives us evidence that cannot be obtained in any other way. In line with the above reasoning, he fully supports the proliferation of theories: This enhances the advancement of science in contrast to uniformity that impairs its critical power. An individual can similarly be adversely affected by uniformity of ideas: such could endanger and dwarf his progress. Newton's theory, for instance, is inconsistent with Galileo's law of free fall and also with Kepler's laws: that statistical thermodynamics is inconsistent with the second law of the phenomenological theory, that wave optics is inconsistent with geometrical optics.

It has to be noted, however, according to Feyerabend, that what is being said here is logical inconsistency. We are not bothered with the inconsistency of Newton's theory and Galileo's law; we are instead interested in the inconsistency of some consequences of Newton's theory in the arena of validity of Galileo's law. The situation is especially clear in the last case. Galileo's law states that the acceleration of free fall is constant, while application of Newton's theory to the surface of the earth gives an acceleration that is not constant but decreases (although imperceptibly withnthe distance from the centre of the earth). Our procedure here must entail the confrontation of the accepted point of view with as many relevant facts as possible. The exclusion of alternatives is just a measure of expediency: their invention not only does not help, it even hinders progress by absorbing time and manpower that could be devoted to better things.

A uniform opinion was not one of the features Feyerabend advocates for the acquisition of scientific knowledge. A uniform opinion may be a virtue within a church or religious circle for greedy victims of some

> (ancient or modern) myth, or for the weak and followers of some tyrant. Variety of opinion is necessary for objective knowledge. And a method that encourages variety is also the only method that is compatible with a humanitarian outlook. To the extent to which the consistency condition delimits variety, it contains a theological element which lies of course in the worship of facts so characteristic of nearly all empiricism.[34]

All ideas, reasons Feyerbend, no matter how ancient are capable of improving our knowledge. Such ideas could be from ancient myth, modern prejudice, from the lubrications of experts or the fantasies of cranks. The utilization of the whole history of a subject is in the attempt to improve its most 'advanced' stage. By seeing the acquisition of knowledge in this totality,

> the separation between the history of science, its philosophy and the science itself dissolves into thin air and so does the separation between science and non-science.[35]

This idea is sharply opposed to the position of Popper on the need for a clear-cut demarcation between science and non-science.

Feyerabend sees a number of advantages in the proliferation of theories. Foremost, it leads to the emergence of alternative views. It does not also allow the elimination of older theories that have been refuted. Such refuted theories are assets to the content of their victorious rivals. While Popper reasons that a refuted theory should be done away with,

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34. Ibid, p. 40

35. Ibid, p. 48.

Feyerabend is of the view that even after a theory has been refuted, it still stands to perform positive function in the structure of scientific enterprise. Being rigid in issues of theories is unhelpful, he argues, as even the most advanced and the obviously most secured theory is not safe to the extent that it requires no modification or replacement with the assistance of views which the concert of ignorance has already thrown into the dustbin of history. It is specifically for this reason that the knowledge of today may turn the fairy-tale of tomorrow and now the most laughable myth may afterall turn into the most valuable piece of scientific knowledge.

It is this liberal view of Feyerabend that informed his pluralistic theory. Even metaphysics, he reasons, is important for methodology and constitutes an essential part of a humanitarian outlook. It is impossible, he argues, for a theory to agree with all the facts in its domain. And the blame is not to be placed on theories as it is to be noted that

> facts are constituted by older ideologies and a clash between facts and theories may be proof of progress. It is also a first step in our attempt to find the principles implicit in familiar observational notions. [36]

He noted that when the invention, elaboration and use of theories that are inconsistent (not just with other theories but even with experimental facts evaluated) we will discover that no single theory ever agrees with all the known facts in its domain. This is a creation of experiments and

36 Ibid, p. 55.

measurements of the highest precision and reliability and not because of sloppy procedure.

He querries the possibility of living with the rules of critical rationalism as championed by the Popperian school of thought. He doubts that we can know science as we do today if we had followed the rules this way. He believes this critical rationalism arose out of the need to understand Einsteinian revolution; and this was extended to even the conduct of one's private life. But when the interest of man is considered, (especially the issue of his freedom from hunger, despair, from the tyranny of constipated systems of thought and not the academic freedom of the will) then we are moving in the worst possible fashion. If science as we know it today constitutes essentially in the search for truth in the style of traditional philosophy, he believes this will create a monster. He asks if this has not the potency to harm man, turn him into a miserable, unfriendly self-righteous mechanism without charm or humour. He repeats the question posed by Kierkegard whether it is not possible that his activity as an objective (or a critico-rational) observer of nature weakens his strength as a human.[37] After reflecting on the above issues Feyerabend was convinced that a reform of the sciences that makes them more subjective (in Kierkegaard's sense) is urgently needed. He believes it is impossible to have both science as we know it and the rules of a critical rationalism as stated

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37. Ibid, p. 175.

above. He equally disagrees with Popper's view that the actual development of institutions, ideas, practices and so on does actually start from a problem; it rather starts from some insignificant activity like playing which as a side effect results in development which ultimately can be viewed as solution to unrealized problems. He sees also Popper's thesis of falsification as dangerous as it is capable of wiping out science, and would not have in the first place allow it to start.

Science, according to Feyerabend, does not develop by critical rationalism which Popper advocates. He resents such recommendations of Popper as falsifications, increase content, avoidance of adhoc hypothesis. He believes all these are unnecessary for, science is much more 'sloppy' and 'irrational' than its methodological image. He rather sees such things as 'deviations' 'errors' as pre-conditions of progress. Besides, the ideas that we perceive today as the bedrock of science are there just because such things as prejudice, conceit, passion, exist and because these things opposed reasons and because they were allowed to have their way. The deduction that could be made from all this is that even in the arena of science, reason cannot and should not be permitted to be comprehensive and that it must often be overruled or eliminated in favour of other agencies. It is difficult to find any rule that is valid in all circumstances, just as it is impossible to find any agency that could be appealed to in all times. By the nature of science, reason cannot be universal: and we cannot also exclude unreason. The ancient and the modern also are at interplay.

> There is no longer any antagonism between the most advanced parts of science and ancient points of view which have degenerated because of scientific warfare. Ancient myths are reconsidered, brought into testable form, examined. 38

Feyerabend believes that a fruitful exchange between science and non-science world views will be in the best interest of science development. And anarchistic approach is only what can ensure such realization. And anarchism, he believes, beside being necessary for the internal progress of science is also required for the development of our culture as a whole. He attacks Popper's view that scientific investigation starts with problems. His criticism rested on the fact that this kind of characterisation fails to put into consideration the fact that problem may be wrongly formulated, that one may be out to carry out enquiries about properties of things and processes that later views declare to be non-existent. A solution to such a problem cannot be found. The outcome in such state of affairs is that they are dissolved and removed from the domain of legitimate enquiry. He gives as an instance, the problem of the absolute velocity of the earth, which, in his view, was dissolved by the theory of relativity that denies the existence of absolute velocity.

2(1) ATTACK ON POPPER

Paul Feyerabend was no doubt one of the admirers of Popper's intellectual status. He respects Popper's personality as a scholar that was able to make his marks in his chosen area of interest - the methodology of science. In spite of this, Paul Feyerabend vehemently disagreed with

38. Feyerabend "On the Critique of Scientific Reason" in the Boston Studies in the Philosophy of Science, Essays in Memory of Imre Lakatos, p. 113.

the central issue in Popper's thesis as regards the methodology of science.

As a matter of fact, Paul Feyerbend was anti-methodology - was opposed to any kind of methodology in science and he believes he did not recommend any. Science as a versatile and dynamic enterprise does not need any methodology. A methodology for science is like a chain tied round it which in essence is only a disaster to its advancement. And progress, he argues, is very crucial as far as science is concerned, and

> the only principle that does not inhibit progress is anything goes. The idea of a methodology that contains firm, unchanging, and absolutely binding principles for conducting the business of science meets considerable difficulty when confronted with the results of historical research. 39

He believes that there is no single rule if we should go by historical research, however plausible and however firmly grounded in epistemology, that is not violated at some time or the other. Such violations certainly, he argues, cannot be attributed to accidental events, just as they cannot be seen as results of insufficient knowledge or of inattention that might have been avoided. Rather than to see it in any of the stated ways, they are to be accepted as imperative for progress. He emphasizes that one of the most salient points that is visible in the history of philosophy and science is the fact that

> events and developments such as the invention of atomism in antiquity, the Copernican Revolution, the rise of modern atomism (Kinetic theory, dispersion theory, stereochemistry..)

39. Paul Feyerabend, Against Method, p. 23.

> the gradual emergence of the wave theory of light occurred only because some thinkers either decided not to be bound by certain 'obvious' methodological rules or because they unwittingly broke them.[40]

The practice of science is in consonance with this pragmatic attitude and it is only when the practitioners of science conform to this requirement that progress in science could be assured. Feyerabend asserts that no matter how fundamental or crucial a rule in science may be, there are always circumstances when it is most advisable to ignore the rule and adopt the opposite.

He counters Popper's position that we should eschew adhoc hypothesis, and go straight rather for bold conjecture. Feyerabend feels rather that there are instances when it is advisable

> 'to introduce, elaborate and defend adhoc hypothesis or hypotheses which contradict well-established and generally accepted experimental results, or hypothesis whose content is smaller than the content of the existing and empirically adequate alternative or self-inconsistent hypotheses and so on.[41]

Feyerabend also disagrees with Popper that an investigation commences with the awareness of a problem. He believes rather that from an analysis of the relation between ideas and action it will be noticed that interests, forces, propaganda and brain-washing technique play a much greater role than is commonly believed in the growth of our knowledge as well as in the growth of scientific knowledge. It is taken for granted, he argues further, that action and distinct

40. Ibid, p. 23
41. Ibid, p. 24

understanding of new ideas precede, and should precede their formulation and their institutional expression. The whole idea of having an idea or a problem before we act, that is speak, build or destroy is obviously not the way development takes place in small children. Feyerabend makes the point thus:

> they (children) use words, they combine them, they play with them until they grasp a meaning that has so far been beyond their reach. And the initial playful activity is an essential prerequisite of the final act of understanding. There is no reason why this mechanism should cease to function in the adult. 42

For instance, it should be expected that the idea of liberty could be made lucid only by means of the very same actions that were supposed to create liberty. The creation of a thing and creation in addition to complete comprehension of a correct idea of the thing, are most often parts of one and the same individisible process and cannot be demarcated by bringing the process to a stop.[43] Well-defined programme guiding the process is not in existence, and such a programme cannot guide it, as it contains the conditions for the realisation of all possible programmes. It is rather guided by a vague urge, by a 'passion.' From the passion arises specific behaviour which in turn creates the circumstances and the ideas necessory for analysing and explaining the process for making it rational. He draws an instance from the development of the Copernican point of view, from Galileo to the 20th century.

42. Ibid, p. 26

43. Ibid, p. 26

> We start with a strong belief that runs counter to contemporary reason and contemporary experience. The belief spreads and finds support in other beliefs which are equally beliefs which are equally unreasonable, if not more so ... 44

There is now a new direction to research, new kinds of instruments are erected, 'evidence' 'is related to theories in new ways until an ideology emerges sufficiently rich to provide independent arguments for any particular part of it and mobile enough to find such arguments whenever they seem to be required. And " ... theories became clear and 'reasonable only after incoherent parts of them have been used for a long time. Such unreasonable, nonsensical, unmethodological foreplay thus turns out to be an unavoidable precondition of clarity and of empirical success."45

Feyerabend denied this frequent uses of the words such as 'progress' 'advance, 'improvement' as a claim to aspecial possession of knowledge on what is good or bad about sciences or an attempt to impose such on anybody. Everybody, he believes, can read and understand the terms in his own way and to a set of tradition he belongs. For instance, an empiricist will see progress as a transition to a theory which provides direct empirical tests for most of its basic assumptions. Others may, in contrast, view progress in terms of unification and harmony, at times at the expense of empirical adequacy. Einstein, Feyerbend believes, perceives general theory of relativity this way.

He (Feyerabend) sees his thesis - anarchism - as making provision for progress in whatever direction one chooses to go.

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44. Ibid, p. 27

45. Ibid, p. 27.

Even in the issue of a law-and-order in science, success can only be achieved if anarchistic moves are occasionally permitted to prevail. Following Feyerabend Newton Smith points out that Science

> ... is privileged neither in terms of method nor in terms of results; and in view of this, we ought to remove science from its pedestal and strive to create a society in which all traditions have equal access to power and education. Among the traditions which Feyerabend wishes to see benefit from this equal access are astrology, witchcraft and traditional medicine. 46

3 LAKATOS'S HARD-CORE NOTION OF SCIENCE

Lakatos enunciated what he terms 'hard-core' in his methodology. This resembles to a reasonable extent Kuhn's

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46. W. H. Newton-Smith, The Rationality of Science, p. 125.

paradigm,' for scientists involved in research programme are bound to agree with its fundamental tenets. The difference however is that in Lakatos' thesis there are several 'hard-cores' competing at any stage in history. And as Barkson points out,

> the hard-core' is a set of statements which are protected from refutation by the attempt to modify other assumption when apparent refutation arises. 47

It consists of a family of theoretical assertions and for a theory to be part of scientific research programme (SRP) it must share those aassumptions. There is also the negative heuristic of the programme. And this as Newton-Smith says "is a methodological principle stipulating that the components inthe hard-core are not to be abandoned in the face of anomalies."48 For instance, the Newtonian gravitational theory, the three laws of dynamics and the law of universal gravitation are seen by Lakatos as constituting the hardcore. The essence of the appeal to the negative heuristic is to ensure that anomalies emanating from the application of the theory are not taken as refuting these postulates. The heat produced by anomalies is to be cushioned by the modification of either auxillary hypotheses, observational hypotheses, or hypotheses stating initial conditions. As to what is to be done in the face of anomalies, the guidance is provided by the positive heuristic of the programme. And this comprises of a

(47. William Barkson: "Lakatos one and Lakatos Two: An Appreciation" Boston Studies in the Philosophy of Science, Vol. XXXIX, In Esssays In Memory of Imre Lakatos Edited by R. S. Cohen, K. Feyerabend, et al.

48. W. H. Newton - Smith, Op. cit., p. 9 79.

partly articulated set of suggestions or hints as to how to change, develop the 'refutable variants' of the research programme, how to modify, sophisticate, the 'refutable' protective belt.[49]

When successive modification by the 'positive heuristic' is unable to produce any new independent tests which are passed, the research programme is 'degenerating.' If there is a remarkable success in the research programme in guiding us to new discoveries, then it is progressive: in the longrun progressives are chosen over degenerating research programme.[50]

3 (1) LAKATOS ON POPPER'S METHODOLOGY

Lakatos was no doubt an apostle of Popper. He confe-

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49. Ibid, p. 80.

50. William Borkson "Lakatos one and Lakatos Two: An Appreciation" in Boston Studies in Philosophy of Science, Vol. 39, p. 51.

ssed that his life was changed by the work of Popper. Despite his intellectual leaning up to a certain age, he was forced to abandon his earlier school of thought as a result of the enormous influence the philosophical orientation of Karl Popper had on him. He expresses this thus:

> I was nearly forty when I got into the magnetic field of his (Popper) intellect. His philosophy helped me to make a final break with the Hegelian outlook which I had held for nearly twenty years. And more important, it provided me with an immensely fertile range of problems, indeed with a veritable research programme. 51.

This shows the respect that Lakatos had for Popper and his work. In spite of this, however, he was still very much critical of his philosophy of science, where he had cause to do so.

Foremost, he disagrees with Popper's demarcation criterion which he believes is incapable of telling us which proposition is scientific or not and what condition one could give up his demarcation criterion. He posits that this criterion is subject to easy falsification. To achieve this does not require more than just showing that the scientific achievements were unscientific and that the best scientists in their most memorable moments broke the rules of Popper's game of science.

Also he rejected one of the criteria of scientific enquiries which Popper advocates which is that there should be laid down rules in advance under which a given theory

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51. Imre Lakatos "Popper on Demarcation and Induction" in The Philosophy of Karl Popper, Book. I., p. 241.

stands refuted. Lakatos argues that such rules, if they exist, will make life impossible even for the most brilliant scientist,

> for in large research programmes there are always known anomalies: normally the researcher puts them aside and follows the positive heuristic of the programme. 52

Anomalies, he (Lakatos) argues, in big research works are shelved aside as problematic instances to be solved later at a later stage or there will be an offer of ad hoc solution. These modalities in Lakatos view are accepted even by the best scientists. For instance, rather than see mercury's anomalous peribelion as a falsification of the Newton's theory of our planetary system, and invariably as a reason for its rejection, it will be rather set aside as a problematic instance to be tackled at a later date.

He strongly believes that the presence of anomalies in scientific enquiries should not be enough reason to reject its outcome entirely. Such anomalies, he posits, can always be taken care of. A research programme he argues further bustles with activity. "There are always dozen of puzzles to be solved and technical questions to be answered..." 53

Lakatos was also resentful of Popper's dismissal of inconsistent system as irrational. This standpoint he (Lakatos) reasons lacks spirit of tolerance as there is

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52. Ibid, p. 247

53. I. Lakatos "Changes in the problem of inductive logic" in The Problem of Inductive Logic, Proceedings of the international Colloguium in the Philosophy of Science, London, 1965, Vol. 2 Edited by I. Lakatos (Amsterdam, North Holland Publishing Company, 1968) p. 316

always an opportunity for a research programme to overcome infantile diseases such as inconsistent foundation and occassional adhoc moves. "Anomalies, inconsistent foundation can be consistent with progress. The old rationalist dream of a mechanical, semi mechanical or at least fast-acting method for showing up falsehood, unproveness, meaningless rubbish or even irrational choice has to be given. "up."[54] He remarks that a decision cannot easily be taken moreso if one does not demand progress at each single step, when a research programme has degenerated hopelessly or when one or two rival programmes have achieved a decisive advantage over the other. It is not just possible to find instant rationality. Neither the logician's proof of inconsistency nor the experimental scientists verdict of anomaly can defeat a research programme at one blow. There is always opportunity for one to become wiser after the event. It is not impossible for nature to shout 'no' but human ingenuity in contrast to the posture of Popper may always be able to shout louder. And

> with sufficient brilliance and some luck any theory, even if it is false can be defended 'progressively' for a long time. 55

A given theory or a whole research programme, argues Lakatos, can only be rejected if there is a better one to replace it. As Gardner puts it "one research programme will supersede another if the new programme is, and the old one is not 'theoretically progressive" 56

54 Ibid p. 249

55. Ibid, p. 249

56 Michael R. Gardner "Predicting Novel facts" in the British Journal for the Philosophy of Science Vol. 33 No. I, March, 1982, p. 10.

By this thesis, Lakatos believes that he had separated between Popper's falsification and rejection, the fusion of which happens to be the main weakness of his 'naive falsificationism'.[57] Besides, he believes that 'crucial experiments in Popper's pattern do not exist.' An experiment, he argues, cannot be seen as crucial at the time it is carried out except perhaps psychologically. Even the Popperian trial and error thesis does not impress Lakatos.

> A theory can only be eliminated by a better theory, that is, by one which has excess empirical content over its predecessors, some of which are subsequently confirmed. And for this replacement of one theory by a better one the first theory does not 'even have to be falsified in Popper's sense of the term. Thus progress is marked by instances verifying excess content rather than by falsifying instances, and falsification and rejection become logically independent.[58]

Lakatos also does not believe in the eradication of old theory in preference to new ones. He believes, for instance, that it is quite in order and reasonable to retain and further elaborate on Newton's gravitational theory even after the discovery of mercury's anomalous peribelion. This view is in contrast to Popper's view which states that whichever theory that suffers such inadequacies should be done away with. Thus, Unlike Popper, Lakatos' theory "... explains some rearguard skirmishes for defeated programmes as perfectly rational and thus leads to the reversal of those standard histo-

57. Lakatos, "Popper on demarcation and Induction", p. 249.

58. Ibid, p. 250.

riographical judgements which led to the disappearance of many of these skirmishes from history of science textbooks. These reargruard skirmishes were previously delected both by the inductivist and by the naive falsificationist party histories."59

Popper believes that philosophy of science is a veritable instrument for the advancement of scientific knowledge. Lakatos says no to this view: his idea instead is that philosophy of science constitutes more of a guide to the historian of science than to the practical scientists. He concedes, however, that this may help in a way too just as Popper's philosophy had done, especially to those scientists whose scientific judgement was warped by the influence of previous worse philosophers.

Besides, Lakatos avers that Popper's demarcation criterion has nothing to do with epistemology, that it says nothing about the epistemological value of the scientific game. He says thus:

> One may of course, independently of one's logic of discovery believe that the external world exists, that there are natural laws and even that the scientific game produces propositions ever nearer to Truth, but there is nothing rational about these metaphysical beliefs, they are mere animal beliefs. 60

4. A CRITICAL EVALUATION OF THE CRITICISMS OF KUHN, FEYERABEND AND LAKATOS.

Kuhn, as we witnessed, was very much interested in the activities of normal scientists. His conviction was that

59. Ibid, p. 251.

60. Ibid, p. 254.

Popper's thesis was a complete failure as far as the practice of day to day activities of normal scientists are concerned. He sees Popper's recommendation of testability as a criterion for accepting or rejecting a theory as an unnecessary overstressing, which he posits is not a usual practice in normal science. He does not even believe Popper's thesis on a purposeful attempts to overthrown or refute a theory.

One seems to sympathise with Kuhn's view that attempts to refute theories are not primarily the aim of a normal scientist. Efforts to refute theories obviously cannot be seen as part of day to day activities of a scientist. It could happen that a scientist may have a strong case on the authenticity of a given theory; such conviction may motivate him into attempts to puncture the weak basis of such a theory. Apart from this, one cannot accept the fact that it is the pre-occupation of a scientist to always attempt to refute theories. Moreso if there is at all the need for him (a scientist) to refute theories his own theory will obviously not be his target. It is simply a fact that scientists apply all weapons to defend to the last their theories inspite of overwhelming opposition to them.

Kuhn's refusal to accept the existence of constant changes in the development of scientific knowledge without qualification is also understandable. This is on his conviction that in science, there is the need for some kind of stability as theories and systems of studies in science are not what should be changed without a very fundamental cause. In real fact, it does not seem a feature of normal science to change theories at very short intervals. A scientific theory could last a life time before the need for change arises. And as Kuhn points out, a revolution sets in only when a major crisis that is capable of rocking the foundation of an accepted system sets in. Amidst this crisis, a more suitable theory that is to serve a given scientific community emerges to replace the old one.

Kuhn's opposition to constant changes in science goes pari-passu with his rejection of a critical approach to it. It is difficult to agree with Kuhn that science is a conservative enterprise that abhors critical outlook. Science is an open system. It is open by its readiness to welcome new and innovative ideas. This underlines the dynamism of scientific adventurism. At any point in time, science is preclose from critical inquiry then the exciting feature that makes it versatile would have been killed. It looks an undisputable fact that criticism remains an enviable quality of this all-important enterprise that probes into the secret of nature which have significant impact on the life of man. At any point in time, criticisms ceases to be a factor in scientific enquiry, it is from them that knowledge and progress in it will stagnate.

Popper did not dispute the fact that in science there is an edifice, a kind of an organised structure that equips a scientist with a generally accepted problem situation into which his own work can fit like those of his counterpart.

But Popper does not see the emphasis Kuhn laid on normal science as being in conformity with this. He (Popper) rather sees his thesis on normal science as a recommendation of dogmatism to the study of science. It is only in this sense, says Popper, can one view Kuhn's argument on the existence of normal science: and as Popper rightly points out:

> normal science in Kuhn's sense exists. It is the activity of the non-revolutionary or more precisely the not-too-critical professional of the science students who accepts the ruling dogma of the day; who does not wish; and who accepts a new revolutionary theory only if almost everybody else is ready to accept it - if it becomes fashionable by a kind of bandwagon effect. To resist a new fashion needs perhaps as much courage as was needed to bring it about. 60

Popper posits further that the normal scientist Kuhn presents to us is a person one ought to be sorry for. 31
He opens his brain to accept but not his mind to ask questions. The normal scientists, according to Popper, is like a robot who accepts views without being given the opportunity to ask questions, to add his own view or to make an amendment. We can only see the normal scientist as one who had imbibed bad orientation and turtoring, one who has been patterned in a most conservative and primitive way. He needs nothing but our total sympathy; according to Popper. One cannot but see the situation in this way, and as we know, teaching especially at a higher level is done in such a way that individuals are given the

60. Popper "Normal Science and its dangers," in Criticism and the Growth of Knowledge in proceedings of the International Colloquium in the Philosophy of Science. London, 1965, Vol. 4, p. 52.

61. Ibid, p. 52.

chance to ask questions and possibly add their own knowledge which may invariably be of immense benefit to the issue at stake, Popper rightly puts the issue this way,

> ... all teaching on the university level and if possible below should be training and encouragement in critical thinking. The 'normal scientist as described by Kuhn has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination.' 62

It is a truism that a teacher is more enthusiastic most of the times with his students when there are visible signs of their participationnin the lectures through very critical questions on the topic at hand. As Popper reveals in his discussion with an Engineering teacher, Philip Frank in 1933, (the Engineering teacher) expressed his dissatisfaction with his students thus:

> They [his students] merely wanted to 'know the facts.' Theories or hypothesis which were not generally' accepted but problematic were unwanted: they made the students uneasy. These students wanted to know only those things, those facts which they might apply with a good conscience without heart-searching. 63

This Engineering teacher as reported above was obviously expressing his frustration with his students. His method of teaching as noted in his tone was to make the students critical and be self-reasoning, but unfortunately such approach made them uneasy which he regrets anyway. This testimony acquesces Popper's assertion that all processes of importing knowledge especially at the university level aim to cultivate critical mind. We agree entirely with

62. Ibid, p. 53

63. Ibid, p. 53

this position.

Obviously, in line with his rejection of critical approach to science, Kuhn stated that one theory - or a paradigm - predominates a given scientific community over a reasonable period of time. He favours "the domination of a ruling dogma over considerable periods and ... does not believe that the method of science is normally that of bold conjectures and criticism." 54

This position is not acceptable to us. Science cannot be accepted as a **pure method of indoctrination. It is** equally not a cult where one imbibes doctrines unreflectively. In theology and religion, this mannerism could be rational but in science, it is an aberration and its acceptance can only deal a devastating blow on it. As Popper points out, we accept that " ... in science, as distinct from theology, a critical comparison of the competing theories of the competing frameworks is always possible. And the denial of this possibility is a mistake."65

One is also at a loss at Kuhn's opposition to Popper's view that tests precede the replacement of a theory. Science is a practical discipline, we know; and that testing or experimentation is invariably linked to its studies, we also know. It seems rational that before any theory is accepted or rejected that such should be exposed to crucial testing. If we say, for instance, that oxygen (O_2) enhances combustion, and in turn reduces the Oxygen (O_2) content of an enclosed environment and yet the combustible capacity of

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54. Ibid, p. 55

65. Ibid, p. 57

Ideas are obviously guides to action. If all laws and theories are conjectures they cannot be relied upon. All knowledge cannot be seen as provisional. There should be a distinction between knowledge and conjectures. As Putmam rightly points out,

> a theory, as the term is actually used is a set of laws. Laws are statements that we hope to be true, they are supposed to be true by the nature of things and not just by accident. 84

It is quite wrong for Popper to equate laws with hypotheses. If his proposal is accepted then it becomes difficult to differentiate a proposition that has been tested and corroboration and those yet to be tested. As Putnam points out again,

> scientific theories are shown to be correct by their successes just as all human ideas are shown to be correct, to the extent that they are by their success in practice. 85

It can only be seen as a wrong use of terminologies for Popper to equate hypotheses with laws and theories.

It is not quite correct for Popper to insist that truth could not be reached. What can be true anyway is that no truth can be seen to be absolute. What is true today may not be the case tomorrow. But as long as there is no evidence to see it as nothing but the truth, we can take it that we have reached the truth at that point in time. That we rely on a piece of knowledge as a guide to actions is because we

84. Hilary Putnam, The corroboration of Theories" in the Philosophy of Karl Popper, p. 226.

85. Ibid. p. 229.

as esoteric. A scientist can write a specialized paper; one who is not trained to be a scientist can also write a paper that can contribute to the growth of scientific knowledge. Both papers could be comprehensible to both scientists and those not trained in science, but who are interested in scientific enterprise. In the same line of thought, there is always a chance for new ideas or frameworks of science to be evaluated. And in the view of Popper, being tied to a framework is like a prison and and as Asike points out "Enemies of scientific frameworks will always welcome discussions with partners or even opponents from other frameworks."[66] There is an opportunity by this for them to discover their so far unfelt chains, to break them and by so doing transcend themselves.[67] Such attitude agrees with a dynamic and positive attitude to science which ensures its advancement.

Feyerabend does not dispute the fact that he advocates irrationalism in the pursuit of scientific knowledge. His view on science no doubt, is a radical departure from those of other philosophers of science. He was both radical and revolutionary in every sense of the word. However, one finds most of his views especially those that conflict with Popper's very unacceptable, and even inconsistent. For instance, his thesis that science is

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66. J. I. Asike, "Scientific facts and their theoretical frameworks" In Uche, p. 41

67. Ibid, p. 41

without method is absurd, and infact he was unable to be consistent in this stance. As noted in his work, he is a champion of anarchism and plurality of theories. If anarchism and proliferation of theories are advocated by him rather than conjectures and refutations as enunciated by Popper, it means that it is such methodology that he propounds. Besides one really does not know the extent of rationality science can attain if there is no caution innthe proliferation of theories as Feyerabend favours. A discipline such as science and any other for that matter should have rules and regulations as guides for the formulation of theories; and such theories should have specific problems they tend to solve, and only those that have very strong foundation should pass as scientific theories.

This stance also opposes Feyerabend's view that anarchism should be a benefiting factor in scientific enterprise. If anarchism is understood to be disorder, lack of rules and regulations it is highly doubtful if it is an asset in an academic enterprise. In science, the procedure requires some elements of care to the extent of being meticulous. If care is thrown to the wind, then there is a breakdown in scientific research.

One fails to see too a strong basis for Feyerabend's opposition to Popper's view that a problem procedes scientific investigation. There can be quite sincerely a number of academic investigations that are not motivated by specific problems but certainly genuine scientific investigation are always motivated by concrete scientific problems.

Copernican revolution was motivated by the puzzle the universe poses to him: Newton's gravitational theory was an outcome of his (Newton) genuine interest to unravel the composition and the effect of natural forces on the creatures of the cosmos.

We also find it not quite easy to believe wholly Feyerabend's thesis that things are known by contrast and not by analysis. What we can understand by this stance is an urge for us to accommodate views other than the one that is accepted in the official circle. This view is anti-Kuhn[40] whose idea of scientific methodology is the acceptance of a paradigm which prevails in a given scientific community. Further, Feyerabend obviously in opposition to Popper believes that an idea that is not entirely sound should be improved upon rather than being discarded. This view contradicts Popper's recommendation of a crucial test: refutations for corroborations on the basis of such (crucial tests). A theory that fails our tests stands refuted and should be discarded rather than attempt to improve on the same.

We can acquire knowledge by contrast; analysis is also a medium of acquiring knowledge and in fact the strength of our knowledge is achieved by analysis. We cannot also accept the view that ideas that are found to lack solid foundation should be improved upon than a more serious attempt to source for new information and ideas for attending to our problems. A researcher who insists on improving on an idea that has failed may suffer mental depreciation and a possible loss of interest in a research he is carrying out. The best approach to a problem is when a particular assumed solution is tried without success an entirely new one should be sourced.

Also truth is a guiding focus in scientific enterprise. When a researcher has a concept of truth at the back of his mind, then his approach and where he is going will be a bit clear. Ironically, however, Feyerabend argues that the search for truth is not in consonance with scientific enterprise. One can hardly believe this. Truth as an ultimate goal is what keeps on the spirit of scientific enquiries. Being able to reach at this truth is entirely a different thing.

We cannot also accommodate Feyerabend's view that inconsistency is a benefiting factor for progress in science. A serious researcher is always anxious to achieve a consistent view than the converse. It is only such that ensures that there is coherence and rationality in his endeavours. Whenever inconsistency sets in, he then knows that a major problem is apparent in his work, which must be taken care of before further advancement.

There might not be the need for a uniform view in science, as Feyerabend believes, but whatever view one holds should have a basis that centres on accepted basic principles of science. Also Feyerabend views that all ideas, ancient, modern, metaphysical should all be accommodated for the progress of science can attract some sympathetic attention. Such sympathy centres on the fact that both metaphysics and science have the universe as their field of operation. While metaphysics is rested on abstract and immaterial entities science's focal interest is on material things. The two enterprises aim to uncover the mysteries of the world, harnessing the same for the service of mankind. Thus the material and the immaterial blend together to give us a picture of the whole. For instance, science's law of energy which is that energy is indestructible has a metaphysical counter-point in the doctrine of the immortality of the soul or the theory of the indestructibility of the human spirits. One can see a clear interaction between the body which is material and the soul which is immaterial. The soul functions well when it is properly fed with material things and the body is guided well too when the spirit is in the best form. To this extent, one can accommodate Feyerabend's view. But there is the serious need to note that Popper was not opposed to metaphysics, neither did he, like the 'positivists' dismiss it as a meaningless garbage. The point he made, instead, is that metaphysics and science deal with different realities, and that the methodology of investigating the two are as a result not the same. This point [illegible] is definitely not an anti-metaphysical stance.

Feyerabend fears that Popper's falsification theory is capable of wiping out science, and in fact would not have allowed it to start in the first place. This fear can only be if a scientist makes falsification his pre-occupation, without proffering alternatives. The Dalton's atomic theory which was that an atom is an indivisible part of an element was refuted by a more modern theory of atom which is that an atom is divisible into electron, neutron and proton. The fact that the Dalton's atomic theory which previously held sway was refuted and replaced by a much more modern theory, makes for a better understanding of an atom. Lakatos on his part thought his efforts were to improve on Popper's thesis. It is, however, clear that his success was not anything to write home about. One does not, for instance, understand what he meant by saying that there is no rationality in the distinction Popper made between science and non-science. It is to be noted that Popper's major interest in science was on its methodology. And for the methodology of any discipline to be clearly discerned, its features and peculiarities are to be fully appreciated. It was basically on this ground that Popper sought for this distinction.

Also, his rejection of Popper's refutation theory is questionable. The position of Popper on this is unambiguous: a scientist is to state clearly what his position is on a given theory. If the theory is unable to pass through crucial experiments on the basis of the information he supplies, there is then no rationality for insisting that such theory should be retained. Lakatos favoured tests of sequence of theories, can hardly work. A theory should be tested on its merits, if it cannot pass through crucial tests, it means it is not worth being retained in the family of science. There is no complexity on Popper's requirement that a scientist should state in advance conditions under which a theory is to be accepted or refuted. This requirement, no doubt, will motivate a scientist to look for a solid foundation for his theory before presenting the same as ways of tackling a given problem.

Equally absurd is Lakatos position that philosophy of science has more to do with the Historian of Science than with real scientists. It is non-disputable that philosophy of science is of invaluable service to both theoretical as well as practical scientists. With a full knowledge of the basis of science, its fundamental principles and rationality, a scientist will be able to put to proper scrutiny whatever methodology he has been using or intends to use to ascertain its appropriateness, and possibly looks for rooms of improvement.

We do not also see the usefulness of accommodating anomalies and inconsistencies as Lakatos advocates. The right thing to do when an inconsistency is detected in a scientific research programme is to reverse the gear, jettison the procedure we have adopted in search for a new one. If such an inconsistency is accommodated, it will certainly affect the final outcome of the research programme.

Even Lakatos thesis of hard-core' hardly stands the test of time. If there are as many 'hard-cores' as possible and if every scientist is in possession of one and at times more than one, it goes to show that the 'cores' are not really hard, as they were protected from change at all cost. As William Borkson points out:

> Scientists actually, contra Kuhn and Lakatos,

> have had enough intellectual independence to make up their own minds about what to take as fundamental, and enough independence to change their minds also, or to keep an open mind and try different alternative ideas as fundamentals. 68

It can be conceded, however, that Lakatos' criterion of measuring progress in science with the emergence of new discoveries should have valuable impact on the advancement of scientific knowledge. But his thesis of 'hard-core' lacks solid foundation, and one does not see any reason why a 'core' that is hard should need all the protection he gives it. Thus Lakatos thesis can hardly be a better alternative to Popper's philosophy of science.

5. CRITICAL SUMMARY

Kuhn as seen so far raised very serious objections to Popper's methodology. He disagreed entirely with Popper's thesis of falsifiability, and equally argues against Popper's view that science as an enterprise is critical. He in addition to his critical view on Popper's work advocated for a method of science which he posits is found in any normal science.

Kuhn's position that science is dogmatic can only be seen in the attitude of some scientists who in spite of overwhelming evidence against their thesis still hold tenaciously to their point. This kind of attitude exist but it does not make for progress in science. For science to be a dynamic discipline it is supposed to be critical. It is only such that can ensure its progress.

68. Ibid, p. 52.

Feyerabend does not favour any method going by his assertion. He disagreed with both Popper's falsification and corroboration theory. He argues that the only thing that can ensure progress in science is that anything goes'.

We cannot just agree with Feyerabend that science has no method. And in fact Feyerabend proposes a liberal method when he stated that the only thing that ensures progress in science is that anything goes'. But we knew certainly that as a rational discipline that it is not anything that goes in science. Any method that is applied in scientific research is expected to have a rational justification.

Lakatos, on his part, does not agree and infact, failed to see the rationale informing Popper's demarcation between science and non-science. He insists that contrary to Popper's view, philosophy of science is valuable only to the historian of science and certainly not for practitioners. He advocated what he called 'hard-core' and scientists involved in research programme must adhere to this.

The critical issue really in Popper's demarcation between science and metaphysics is whether there can be a complete demarcation between the two. As of the rationality informing this, Popper had in mind to show that the method of approach to the two are quite different. Finally it needs to be restated that philosophy of science is both necessary for practitioners as well as the historian of science. There is always the need for scientists to have frequent self-evaluation of the method they use, and should be prepared to discard ideas that are obsolete and unprogressive.

Thus though some of the criticisms levelled against Popper's methodology by Kuhn, Feyerabend and Lakatos could be defended, the alternative methods they offered are hardly better than Popper's.

CHAPTER FIVE

A CRITICAL EVALUATION OF POPPER'S PHILOSOPHY OF SCIENCE

In this chapter, a critical appraisal of Popper's thesis of falsification and corrobation will be carried out. In doing this, both his critical and substantive views will be evaluated. And this will include his theory of induction, probability; his thesis of demarcation, deduction, among others.

As we have noted from our discussion so far, Popper's thesis on the methodology of science is controversial. He believed he was recommending factors that could enhance the advancement of science, but ironically, his prescriptions run counter to the actual procedure in scientific research programme. For instance, his opposition to induction is misplaced and cannot be defended. Practising scientists, both in research work and routine scientific undertaking, proceed by inductive method. A researcher does not need to examine all the samples of any given object he is investigating before he reaches a conclusion. Such a procedure (examining all conceivable samples of an object), if scientists were to adopt it, will both be cumbersome and impossible, and even the progress that Popper seems to cherish in science cannot be attained. What obtains in scientific research in contrast to Popper's position is that after a test has been carried out on a number of samples of an object, and common features noted in the object, a generalisation about the object could then be made. It is this possibility of generalisation that makes science what it is. It is this possibility too that makes future tests and references to be easier for practising scientists. In the absence of this procedure, it is doubtful that science can attain the extent of progress it has now attained.

Besides, Popper was not consistent in his denial of induction. His falsification and corroboration theses have elements of induction. This is because in the attempt to falsify or corroborate a theory we proceed by inductive method. It is only after we have carried out a number of crucial tests that we are in a good stead to offer a statement about the tested theory. And this procedure is definitely carried out by inductive method. And as Feigl pointed out, "It is only by induction that we can assume that a well-refuted theory will stay refuted."[1] Thus before we can establish that a theory is refuted or corroborated, we must carry out a test which involves inductive method.

Popper's demarcation thesis is another area of his philosophy that generates controversy. It is obvious that his attempt at demarcating science from non-science (specifically, metaphysics) was to counter the position of the positivists. And this Popperian counterpoint to positivism is that metaphysics, though not a science is not meaningless. While his effort to discredit the positivists on their degrading remarks on metaphysics is commendable, it is puzzling that he had to use falsification as a criterion for demarcation. It is difficult in the first place to accept the fact that the possibility of a theory being falsified is what makes it scientific. This requirement is awkward as there are many theories in science that cannot be falsified and yet they do not lose their scientific status. Moreover, there cannot be a complete demarcation between science and metaphysics.

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1) Herbert Feigl "What Hume might have said to Kant (and a few questions about induction and meaning)" in the Critical Approach to Science and Philosophy in Honour of Karl Popper, p. 49

Kantians believe strongly in this and Watkins pointed this out when he stated that "according to Kantian presupposition there are certain metaphysical assumptions which must remain under the shelter of the umbrella of science so long as science exists."[2] This Kantian position is well taken as there is no way we can proceed in science without making basic assumptions about nature, and these assumptions are essentially metaphysical..

This Kantian position shows why there cannot be a complete demarcation between science and metaphysics. While Popper should be commended for his attack on the positivists for their degradation of metaphysics, it needs to be stated that his demarcation attempt is not a success. However, it could be conceded that purely metaphysical propositions need not to be corroborated by empirical test as it is the case with scientific hypotheses. Hence while it is necessary to note the differences in approach to the two (science and metaphysics), it is to be emphasised that a clear-cut demarcation between them is not feasible. Besides, it needs to be remarked that there are many theories that cannot be falsified (such as Newton's theory of universal gravitation and Copernicus' theory of the universe) and yet they are scientific.

Falsification generally as advocated by Popper as a methodology of science raises more questions than it could answer. To falsify as we understand it, is to prove wrong, to demolish the foundation of a system. And Popper was convinced that this is really what scientists do at any time they embark on research programmes. Which is to say that the pre-occupation of a scientist at any point in time is to try to falsify his theory or those of others.

2) J. W. N. Watkins "Metaphysics and the advancement of Science" in the British Journal for the Philosophy of Science, Vol. 26, 1975, p. 91

This position is definitely a misrepresentation of what practising scientists do, either in research programmes or in routine work. The impression one gets from Popper's position is that scientists are always bothered with negative affairs. This is not the case in research work in science. A researcher makes references to an existing theory, not necessarily because he wants to falsify but as a reference for illuminating something greater in the future. Thus, existing theories serve as reference points or guides to prospective researchers. They do not burden themselves with the fruitless effort of falsifying theories. Scientists start looking for alternative theory (invariably, a vote of no confidence on the existing one) when the one serving as a point of reference is beset with problems. This was the case with the thesis of Phlogiston and Combustion. When Stahl and his fellow researchers arrived at the conclusion that Phlogiston was responsible for combusition, they could not answer more questions on this revelation, such as the reason why metals gain weight after they have undergone combustion: "if there was escape of phlogiston the reverse should have been the case."[3] Some of these crucial questions that were unanswered by Stahl led to more researches on the causes of combustion.

It looks a negative effort for a scientist to place in front of his mind the ambition to falsify existing theories or his own as the case may be. There is thus no way falsification can be taken to be a methodology of science since it does not agree with what scientists do. And as Maxwell pointed out "a theory of scientific method in order to be acceptable, must have at least some contact with scientific practice, with the aims and appraisals of working scientists."[4]

3) Miller. p. 4
4) Nicholas Maxwell "A Critique of Popper's views on Scientific Method" in the Philosophy of Science, Official Journal of the Philosophy of Science Associastion, Vol. 39, No. 2, 1972, p. 133

Falsification as a methodology does not meet up to this expectation. And it is worth remarking that scientists try to build their theories on very solid foundation so as to resist easy refutation. Most scientists, in fact, would refuse to give up their views even in the face of overwhelming evidence. Given this position, it is doubtful if a scientist will put all his energy solely for the falsification of his theory. In real practice, all the evidences gathered by a scientist is aimed at strengthening his theory, not refuting it. In Popper's methodology, however, (as Quine pointed out) "evidence does not serve to support a hypothesis but only to refute it."[5] Besides, as Dr. C.S. Momoh pointed out "if a single abberrant instance can falsify a hypothesis, what happens to the legion other instances that had confirmed the hypothesis?"[6]

In the same vein, it is difficult to appreciate how Popper equates testability with falsification. He wants us to believe that at any moment a test is going on in science the aim of such a test is to falsify a theory. This is a distortion of the part that test plays in science. Testing in science, as we understand it, is essentially meant to ensure that one is in the right course: it is a way of confirming an idea that one already has. And this is a constant undertaking in experimental works. A proposition is usually made before an experiment is started. Test is used to ascertain the validity or otherwise of such a proposition. If there is disagreement between the outcome of the test and the proposition the experimenter will know either that there is a problem somewhere or that his proposition is wrong in the main. In essence, a scientist does not test primarily to refute either his own theory or those of others.

5) W. V. Quine "On Popper's Negative Methodology" in the Philosophy of Karl Popper, Book I, p. 218.

6) C.S. Momoh "The 'Logic' Question in African Philosophy" in The Substance of African Philosophy Edited by C.S. Momoh, (Lagos African Philosophy Projects Publications, 1989) pp. 170-171.

And for any theory in science to be regarded as a genuine source of knowledge it must have a clear-cut procedure of testability for ascertaining its authenticity. Any scientist that applies meticulously the laid-down procedure is expected to arrive at the expected goal of the theory. It can happen at times, however, that a scientist may have some doubts about a given theory. In this case, tests are carried out to confirm such doubts- it is only then that one can talk about an attempt at falsification. Besides, tests are carried out in science to arrive at the end result of a theory. For instance, if it is stated that a base combines with acid to form soap, the concern of practising scientists will not be on how to falsify this theory, but on how to mix base with acid to get soap.

The point against equating testability with falsification leads us to the issue of crucial test as recommended by Popper as a way of ascertaining a bold conjecture. This practice of subjecting a theory to maximum strain is really not a common feature in science. If, as earlier on pointed out, that there is more anxiety to corroborate than to falsify, then the question of subjecting a theory to maximum strain hardly arises.

Popper's thesis of probability which is an offshoot of his falsification and corroboration thesis is not acceptable to us. His contention that scientists aim at less probabilistic statements is a misrepresentation of fact. It is equally a misuse of terminology and a creation of confusion, for him to state that the less empirical content a theory has the more probabilistic it is. As we have noted earlier on, probability is an essential and, in fact, an unavoidable ingredient of science. The law of causality which takes this shape: 'If x then y' has ceased to be a veritable guide in scientific investigation. And as Reichenbach pointed out,

> causality was to be formulated as a law of exceptionless generating as an if-then always relation. Probability laws are laws that have exceptions, but exceptions that occur in a regular percentage of instances. The probability law is (thus) an if-then in a certain percentage relation. 6

Modern logic gives solution as to how to deal with such a relation and this is called 'probability implication'. Thus in modern science "The causal structure of the physical world is replaced by a probability structure, and the understanding of the physical world presupposes the elaboration of a theory of probability."[7] The composition of the cosmos which science deals with is so complex that the usage of probability in its approach becomes inevitable. For instance, such things as atoms, molecules, current, wave and so on are very complex to be predicted with certainty. As a way out, probability becomes inevitable. The same applies to the other forces of nature that are definitely beyond the control of man: the much that can be made in respect of them are probabilistic propositions.

A pharmacist, for instance, can produce a drug which in his estimation can last for five years. In making this prediction issues such as temperature and adequate care were put into consideration. There are yet other factors that could work against him which he could not fathom. These factors preclude him from making a categorical statement or even the type of 'x then Y'. The much he could do under the circumstances is to make an assertion ladden with probability. The same situation applies to a medical doctor. That a given drug has cured many patients that suffered a particular ailment does not mean that the same drug must cure all patients that have the same ailment.

6) Reichenbach, <u>The Rise of Scientific Philosophy</u>, p. 164

7) Ibid. p. 164

However, that he administers the same treatment that has cured several patients of a particular disease is an impetus for him to do the same to any patient that is afflicted similarly. That such a treatment must cure the patient is certainly what he cannot predict with ultimate certainty. There are many variables he cannot control such as the body chemistry of the patient concerned, and more especially metaphysical factors which he lacks control of. An engineer is similarly faced with the same kind of problem: after constructing a bridge, he cannot say for certain the length of time it could last. In all of these instances, probability factor comes into his rescue. And its usage is not baseless and arbitrary. When a scientist uses probability in his proposition, he reflects in this his intellectual capability acquired both by training, orientation, years of experience and understanding of environmental factors. And since his knowledge cannot be perfect, it is expected that whatever proposition he makes should have chances of being faulted. And there are new developments in nature that always stand in opposition to the ingenuity of scientific proposition. And the only feasible way to put such under proper control is by the application of probability principles. It is to be noted that at any point in time, a scientist aims at high probabilistic statements, in contrast to the position of Popper who believes that scientists aim at low probablistic statements. The more mature a given discipline is the higher the probability of its propositions.

Trial and error, stated Popper, is a feature of scientific discovery. This cannot be completely true. A scientist is not a blindman that is unaware of his destination. A scientist invariably has a target he wants to reach. The normal procedure in research programme is first the formulation of a problem, the statement of purpose as to how to tackle such a problem, then a proposition which is hoped to be corroborated, is made. After all these, the researcher proceeds with his work. If the proposition made is realised, then he knows he had reached his target but, where otherwise he may restate the problem in a different way and adopt a better procedure for its realisation. Thus, discoveries in science are made more often than not by purposeful research and the need to find a solution to a given problem. For instance,Galileo discovered the principle of Pendulum, but he could not determine the ways to establish the measurement of small interval of time. It was this problem that led Huygens into the task of finding appropriate method of measuring small intervals of time. He succeeded in doing this by making use of the principle of Pendulum developed by Galileo. This was not by trial and error. There can be one or two instances where discoveries in science are made by suddenness. But this is more of an exception than the rule. Researches in science are very serious businesses; they cannot just be cheapened as efforts into trials and errors. If science and its discoveries are seen as adventures in trial and errors, it follows then that anybody can be a scientist and that training in the field of science can as well be a luxury that can be done away with.

Popper sees scientific breakthrough in isolation; he did not see research as a continuous affair. This stance is in consonance with his falsification thesis and bold conjecture. A theory is either corroborated or falsified, he stated. And he as a result advised scientists to make bold conjectures and carry out a crucial test to refute it. He (Popper) did not see research and discoveries in science as a continuum: where a researcher continues from where his colleagues stopped - not to refute the position of his colleague but to make improvement. There are various instances in the history of science to show that research and discoveries in science are a continuous affair, that scientists are more co-operative and tolerant of colleague's ideas than Popper will like us to believe. For instance, Cavendish was actually the first to discover that water could be formed by burning inflammable gas in the air. Lavoisier repeated this experiment exactly the same way that Cavendish has done, and named this inflammable gas hydrogen - which means to give rise to water. Similarly, "Einstein's work climaxed the favour experiment of Michelson and Morley who had been unable to detect any difference in the velocity of light with changes in its direction through the ether."[8] Einstein assumed that the measured velocity of light in a vacuum is always constant. He also assumed that light travelled in quanta. He was finally to establish that without the 'ether' nothing in the universe can be seen to be in absolute rest just as no motion can be considered absolute - as the motion is relative. In the same continuous venture, the Dalton atomic theory climaxed Democritus' earlier speculation on atom. Also, Lavoisier has discovered that oxygen supports combustion. This thesis has not been disproved. Davy Humphrey, years later, was to add chlorine, an element which also supports combustion, which is an addition to oxygen already discovered by Lavoisier. In all these, we witness efforts more in co-operation for improvement rather than in confrontation. In this regard, science is a series of efforts towards the strengthening of a given hypothesis or to fill a gap. With these instances one is not left in doubt that scientific discoveries are a continuous affair.

8) Asimov, p. 589

It is really this cumulative nature of scientific knowledge that makes progress in science possible. It is surprising that Popper believes and wants us to also accept that science progresses by conjectures and refutations: that it is only by critical approach to theories and its subsequent overthrow that progress in science is attained. This is hardly the case. Progress in science is attained through co-operative efforts among scientists. One theory leads to the other; just as one discovery takes us to the other.

Research does not have to start from the scratch. If this has been the case, science would not have witnessed the extent of progress it has. The dynamism of science centres on the fact that no theory is unduly protected from change. Once a theory ceases to serve as a solution to the problem it hitherto did, such a theory ceases to be reliable, and the alternative left is to search for a replacement. There is no room for obsolete theories in science. For instance, the Dalton atomic theory, that atom is the smallest indivisible particle of an element has become obsolete and has now been dispensed with as we now know that an atom can be split into neutron and electron. In the same vein, the notion that all acids contain oxygen has since become obsolete with the discovery by Davy that hydrochloric acid has no oxygen and Gay Lussac also discovered that prussic acid had none. As a result, hydrogen is what is used to identify acid instead of oxygen, in line with the latest discoveries. Thus, while theories that have obvious weak points are replaced, genuine ones that have continued to answer questions posed to them, which have no traces of weak points are retained in spite of the number of years such theories have stayed.

It is to be remarked that stability gives rational image to science. It is definitely not in the best interest of science and scientists that their theories cannot endure for a reasonable space of time. If this cannot be achieved, it follows that scientists are either not thorough in their work or that science as an intellectual enterprise is not reliable. But we know that science and research work very serious affairs, and before a researcher makes a claim to knowledge, he must have done his home work properly. No researcher takes pride in positing porous hypothesis that cannot withstand the simplest criticism. It is thus the serious attitude that goes with science as a discipline, that makes knowledge in it to be cumulative. And this state of affairs does not allow a situation where a researcher has no where to make references prior to the assignment he is faced with. There are many theories, many discoveries that always serve as reference points to scientists in their research programmes. If theories in science were to be changing as Popper advocates, it will be quite difficult for students in the field of science to cope with such unregulated changes.

Popper sees laws or theories in science as mere conjectures or hypotheses. To him, no theory or law in science enjoys any status more than this. In essence, he sees laws or theories in science as performing tentative role that can be taken away once they are falsified, and such falsification is expected to come sooner than later. This position of Popper is quite unacceptable to us. The normal procedure in science is that a scientist carrying out an investigation or a research usually states what his problem is about and makes a proposition. After which he embarks on a number of experiments from which a conclusion is reached. If his proposition or hypothesis agrees with his conclusion, then this has become a theory or law as the case may be. It is wrong to see a proposition that has undergone a successful experiment as conjectures. Conjecture, as we understand it, is the same as a guess, and a guess is a most inappropriate word to describe a proposition that has passed through successful testing. Such proposition has obviously passed the stage of being seen as a mere guess that lacks a foundation.

Thus, a conjecture, in contrast to a theory, is a guess, a hypothesis that is yet to undergo a test and confirmation. It is not a reliable knowledge and it is not an avenue to tackle any given problem. It lacks authority. A theory, on the other hand, is a proposition that has been tested and found suitable as a reliable avenue to tackling a given problem. It is thus a reliable source of knowledge unlike a conjecture. Thus, as Putnam puts it "when a scientist accepts a law, he is recommending to other men that they rely on it - rely on it, often, in practical contexts."[9] And when an idea becomes a theory or law in science, such an idea or a theory has ceased to be a private or subjective affair. It has become a source of knowledge - a medium through which scientific problems are tackled. This is not the same with conjectures or guesses. The latter are very private and subjective and cannot be made reference points for the tackling of any scientific problem and they are not reliable as sources of knowledge.

There is thus the need to make a clear demarcation between mere conjectures and theories. It is wrong for Popper to see all theories as provisional conjectures. In correct usage, as Putnam

9) Hilary Putnam "The Corroboration of Theories" in the Philosophy of Karl Popper, Book I, p. 222

pointed out "a theory ... is a set of laws [and] ... laws are supposed to be true by the nature of things and not just by accident."[10] And this is not the same as saying that there is any absolutism as far as any law is concerned. What it simply means is that until otherwise proved, a piece of knowledge is taken as means of tackling a number of problems. A future development can fault this supposition, but until such is done, such knowledge remains a guide to action. Theories in science are not arbitrarily made or chosen; they are rather selected "by their successes just as all human ideas are shown to be correct, to the extent that they are by their successes in practice."[11] It is such successes that inform the so much reliability that scientists have on them. Conjectures or guesses do not enjoy such status as they have not been known to have achieved any measure of success or to have undergone successfully crucial tests and experiments. Thus, it is quite a misuse of terminology for Popper to see all theories in science to be forever conjectures. Conjecture ~~Looks~~ is a very feeble term to be used to describe a law or theory.. It could have been quite in order for Popper to state that no theory enjoys any permanent status that can preclude it from falsification. But this is not the same as seeing all theories as mere conjectures. And it is to be noted that science will definitely lose its rational image if all its laws are equated with mere guesses or conjectures. But ironically Popper advocates a rational image for science.

There is no doubt that Popper's thesis of falsification and corroboration is very controversial. His rejection of induction lacks

10) Ibid. p. 226

11) Ibid. p. 229

a solid basis and in fact is inconsistent with his thesis, for the procedure of falsification or corroboration is essentially inductive. For as Rynasiewilz pointed out "When a theory "E" is said to be falsifiable what is usually meant is that there is a set of singular observation sentences which falsifies (i.e. as inconsistent with) "E""[12] And this obeys the principle of induction. This is how Popper rejected induction and yet smuggled it in through the back door.

Popper's thesis of falsification and corroboration can, at best, be seen as a recommendation of what scientists ought to do and not what they do. It is quite in order to advocate a non-dogmatic stance, but his rejection of induction, probability and his emphasis on falsification is anti-scientific. Besides, Popper was not so much interested in the training of scientists as well as the daily activities of scientists. These are very serious omissions in his thesis. It was the little attention he paid to the routine activities of scientists that made it difficult for him to see the cumulative nature of knowledge in science and that scientists do not work in isolation. On the whole, one can say that Popper's thesis of falsification and corroboration is not a success. This methodology is in contrast to the actual practice in scientific research. It does not agree with the rational image which science possesses. And as Maxwell pointed out, Popper "failed to show that scientific enquiry can be viewed as a rational enterprise."[13] And this is an unfortunate situation as science cannot but be seen as a rational exercise.

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12) Robert Rynasiewilz "Falsifiability and the semantic Eliminability of Theoretical Language" in the British Journal for the Philosophy of Science, Vol. 34, No. 3, 1983, p. 227

CHAPTER SIX

TOWARDS A RELEVANT NIGERIAN SCIENCE CULTURE AND METHODOLOGY.

In this chapter, we shall examine the development of science in Nigeria with a view to evaluating attempts, successes and failures that have beset the advancement of science in Nigeria. We shall, at the end of this examination, recommend a more appropriate attitude to the pursuit and acquisition of science and technology for the country, which we believe will give the nation the kind of identity she requires as an independent nation that is anxious to acquire scientific and technological culture.

1. PREAMBLE

Nigeria, as a developing nation, requires all the skills, all the technical know-how, all the machineries and raw materials for her accelerated development. Thus in the national policy on education, emphasis is placed on the study of science and technology that could enhance the acquisition of skills needed for the operation of our industrial concerns. Both at the secondary schools' level and tertiary institutions, the study of science and technology is given a lot of encouragement. At the university level, admission is pegged at 40:60 percent in favour of science and technology. Scholarship, loans and bursary awards are more liberal to the science students, even at the expense of their counterparts in the Humaniities and social sciences.

In the late '70's,Nigerian students were sent to various countries abroad such as Bulgaria, Italy, Canada and Japan, all in the effort to raise middle level manpower to manage the country's industrial establishments. In essence, the national educational philosophy places a lot of emphasis on the promotion of science and technology, which the nation's policy makers believe is still at infant stage. But to what extent the nation has advanced in her quest for science and technology through this policy is still an open question.

2. THE STUDY OF SCIENCE AND TECHNOLOGY IN NIGERIA

As earlier on stated, there is emphasis on science and technology in the national philosophy of education. The policy is aimed at producing scientists that could man the nation's industrial concerns as well as men that are skilled enough to be self-employed, in one technical concern or the other. This explains the generous incentives that science teachers and students enjoy, both in secondary and tertiary institutions.

In spite of this, the nation has not been able to acquire the requisite science culture that will match the challenges of the new world. The country still depends on experts from the more advanced countries of the West for her technological needs. We still lack the capabilities for both maintenance and productive processes.

That the country is yet to meet up to expectations in developing scientifically, seems not to centre primarily on policy formulation, but in execution. That there is a national philosophy towards the promotion of science and technology is definitely a good idea, but where the real issue lies is on the actions taken in the spirit of this ambition. The 6-3-3-4 system is a very good example. The philosophy that informed this policy is the need to lay a solid foundation for science and technology in particular and education in general, right from the beginning of a child's career in learning. As the programme affects science, it was believed that the six years a child spends in the primary school will serve the purpose of exposing him to the primary principles of science. At the post-primary level, elementary technical problems as well as the fundamental principles of science are expected to be taught. It was in essence believed that at the end of the first three years at the post-primary school, a pupil will be able to discover himself and be aware of the appropriate discipline he can fit in very well. There is, nonetheless, emphasis on science. And as Bajah, etal puts it, at the junior secondary school level, studies in the sciences are done in such a way that the pupil "gains the concept of the fundamental unity of science, gains the commonality of approach to problem of a scientific nature."[1] At this level, too "... the fundamental unity of science"[2] is stressed. The whole idea is to equip the pupils for greater challenges in the pursuit of their career in science and technology.

At the senior secondary school level, pupils are then exposed to the basic sciences such as physics, chemistry and biology. The approach at this level is expected to be critical and mature with a lot of emphasis on practicals. And the essence of this, as Ikeobi and Bello, etal stated is,

> to ensure that learners are provided with continuous experiences in skills of defining problems, recognising assumption, critical thinking, hypothesising, observing, collecting and recording data, testing and evaluating evidence, manipulating variables, generalising and applying generalisation. 3

The emphasis, at this level on field and laboratory studies, is to ensure that pupils are exposed to practical application of the concepts they have previously known theoretically.

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1) Bajah, Ikeobi, etal "Philosophy and Objectives of integrated science" in <u>National Curriculum for Junior Secondary schools</u>, Vol. I (Lagos, Heinemann, Educational Books (Nigeria) Ltd. 1985), p. 3

2) Ibid. p. 3

3) Ikeobi, Bello, Etal, <u>National Curriculum for Senior Secondary Schools</u>, Vol. 3 (Lagos, Heinemann, Educational Books (Nigeria) Ltd., 1985), p. 76

There is provision for pupils that cannot proceed beyond the junior secondary school. This set is expected to concentrate attention on the study of technical and vocational related works such as woodwork and carpentary, designing, cookery and secretarial studies. The policy is in consonance with the national philosophy of self-reliance to ensure that everyone is reasonably equipped to source for livelihood for himself. This idea, immaculate as it is, has been met with varied problems especially at the execution stage. About ten years after the programme has taken off which [according to Bajah, Odunsi, et al] is meant to "equip students to live effectively in our modern age of science and technology,"[4] Most schools are yet to have basic equipments needed for their laboratories and workshops. Many students have passed through the first stage of the six years' programme in the post-primary institutions without knowing what a plier, typewriter, divider, burette and pipette are all about. Thus, everything they have known as far as science is concerned is theoretical. There have not been enough opportunity for pupils to subject to critical examination all the concepts they have known. The kind of environment necessary for the development of inquisitive and independent mind which are the hall-mark of a scientist are non-existent. They have been trained more or less to be robots that swallow doctrines without questioning their rationale and authenticity.

At the senior secondary level, these students who are expected to be trained as scientists are most ill-prepared to appreciate the intricacies, techniques, skills, patience and the critical mind needed for experimental and research activities. They are, amidst this state of affairs, introduced to the basic sciences such as physics, chemistry

4) Bajah, Odunsi, etal, p. 3

and biology. This kind of sudden exposure frightens the pupils: and more often than not, studies even at this level lack necessary teaching aids for practical purposes. By the time necessary equipments are made available for field and laboratory works, pupils are almost at the last lap of their programmes.

Thus, what is done at the six years* post-primary career is at best a crash programme. The first three years spent there is definitely not enough for a pupil to make up his mind on the career to chose, especially as the facilities that will enhance this study are absent. The remaining three yeara are spent with lots of anxiety as pupils at this level fight the war of survival.

At the higher institutions, the problems that the students had contended with at the post-primary level rear up their heads again. Facilities necessary for the standardization of the laboratories and libraries are either in short supply or are completely absent. Research grants that should assist the lecturers to update their knowledge which should positively affect the students are unavailable and where available, are grossly inadequate. It is under this kind of environment that students graduate as scientists, engineers, pharmacists and medical Doctors. This explains why a Nigerian trained engineer cannot detect common fault in a machinery; experts must have to be flown in from abroad to attend to this. Similarly, locally trained medical Doctors practice essentially by trial and error, with diagnosis and prescriptions being largely a guess work to the extent of abuse and enormous risk to their patients. In the course of treating one ailment a more serious one crops up.

The story is the same in the field of Agriculture. The Nigerian trained agriculturalists have proved quite incapable of meeting the challenges of technological revolution that has taken place in the field of agriculture. Farmers in the country are yet to know what hybrid crops are all about, as our agriculturists are yet to wake up to such challenges. The most primitive method of farming, harvesting and storage has remained very much with us. Yet we have many graduates in agriculture, botany and zoology.

In the circumstance, the nation has no better option than to rely on the developed countries of the world for technical assistance in basically all fields that have something to do with science and technology. Our scarce foreign exchange that could have been invested in more productive ventures is wasted annually to pay these 'experts' while our own talents are lying undeveloped and unexploited. This situation calls for a change of attitude and a better approach to the training of our scientists.

3. TRAINING OUR SCIENTISTS: WHAT TO DO

As a developing nation, Nigeria needs to train her scientists that will make them suitable for our needs in industries, agriculture and medical services. This training is to spread at all levels of education. And as Novikova stated "a scientific world outlook as a whole is shaped through general school, specialised and university education ..."[5]

The 6-3-3-4 system, as earlier on pointed out has been beset with tremendous problems to the extent that one is bound to ask whether rigorous thinking was done on it even at the conception stage.

5) L. Novikova "Introduction" in the Problems of the contemporary world (NO. III), Civilization, Science, Philosophy, Theme of the 17th World Congress of Philosophy (Moscow, U.S.S.R. Academy of Sciences, 1983), p. 7

There is presently not much to justify the huge amount of resources already expended and is still being spent on the project. The products of the programme are still as raw as ever.

There is thus the need to train our scientists in the best tradition. And as Popper recommended, our scientists need to imbibe the culture of critical and dynamic approach to scientific issues. They should be receptive to new ideas and be prepared always to question any knowledge in science from whatever source, as no theory in science is precluded from critical questioning. And as Asike puts it, Popper believes that "... our beliefs are replaced by competing theories and competing conjectures. And through the critical discussion of these theories we can progress."[6] At the same time, this critical attitude should not necessarily be geared towards destroying theories, but rather to build it on a more solid foundation.

Similarly, our scientists should be in love with adventure, with sourcing new problems and finding their solutions, if possible. And as Popper advises "one of the things a philosopher may do and one of those that may rank among his highest achievements is to see a riddle, a problem, or a paradox, not previously seen by anyone else."[7] Nigerian scientists should be daring and develop the culture of seeking for innovative ideas, and at the same time be able to ask fundamental questions about the existing source of knowledge. Presently, our scientists, unfortunately, seem to regard truth in science as sacred which should not be questioned. This is a timid approach to science

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6) Asike, p. 40

7) Popper, <u>Conjecture and Refutations</u>, p. 184

which must be jettisoned. And as Popper advises again, no theory or statement of knowledge is "immune from criticism, whatever its source may be."[8] Nigerian scientists need to cultivate this kind of attitude so as to free themselves from their present dogmatic mentality and unprogressive mind.

While it is accepted that our scientists should not be dogmatic in their approach to the study of science it has to be remarked that Kuhnian thesis of paradigm may to some extent be useful. This is because there are certain basic principles one has to understand before he can at all know how to begin a scientific enquiry. These basic principles for a beginner in science should be imbibed, at least in the first instance before there can be effective begining in scientific investigation. Thus Kuhnian thesis seems to be more relevant at the early stage of science **development** — :t where the emphasis should be on the understanding of first principles. In spite of whatever weak point Kuhn's thesis of paradigm may have, there could be some good points in it for an early starter in science development. The same point goes for Lakatos 'hard-core' theory. There are certain basic principles that should be assumed if we must make any advancement in scientific research. If this principles are attacked from the beginning of the research we may find ourselves in a vicious circle. But at a later time there may be a need to raise questions about this principles which Lakatos failed to emphasis. It is at this stage that criticism becomes inevitable and progressive.

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8. Popper . Open Society And Its Enemies, Book II, p. 378.

It has to be realised that in corroboration of theories or its refution that testability is necessary. And experimentation is a cardinal feature of scientific research. And this cannot be carried out in the absence of necessary equipments. Popper emphasizes the need for testability in scientific research. And as Giedymin pointed out,

> In Popper's methodology simplicity [testability, strength] as a criterion of choice of hypotheses is combined with corroboration [confirmation] ... defined in pragmatic terms of severe criticism or ingenious and honest attempts at falsification. 9

Testability thus is unavoidable in authentic scientific research programme either to collaborate or refute a thesis. For a genuine work to be done in this direction the necessary equipments have to be available.

It is to be stated, at this juncture, that the 6-3-3-4 system which is believed to hold the key to the science and technological needs of the country is misconceived in the first place, which probably underlies the obstacles in its execution. It is wrong to believe that three years is good enough to evaluate if a pupil is good enough to pursue a career or not in the sciences. This is more real in the face of dearth of equipment and conducive environment for mental development. In our kind of environment, it is definitely wrong to expect one's mental facility to take definite shape within a short space of three years.

There was nothing fundamentally wrong in the structure of our erstwhile system of education, where after a six-year primary school a pupil will have a straight five years' programme in the post-primary

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9) Jerzy Giedymin "Strengh, Confirmation, Compatibility" in <u>Critical Approach to Science and Philosophy</u>, p. 57

school. Argument in favour of the five-year programme is to the effect that it is in the final year that a pupil is made to take a definite stance on whether to be a scientist or not. There is no doubt that such a length of time gives enough room for thinking and rethinking, evaluation and re-evaluation of one's ability with a view to taking a rational position in the face of one's mental capability and interest, of course. Those who cannot cope with post-primary education should be given the opportunity to go to technical schools, suitable for their level where skilled men such as carpenters, barbers, tailors, mechanics, etc. are trained.

At the post-primary school level, emphasis should be laid on both ideas and practice in the study of science. The minds of the students should be trained to be critical, inquisitive and adventurous. They should be brought up to always seek the rational basis for any piece of knowledge. More especially a lot of emphasis should be laid on practicals and as Novikova pointed out "practical requirements [are] a powerful stimulus to the development of science itself for the systematic utilisation of scientific technologies in industry demanded the accelerated elaboration of scientific theories."[10] If students are made to undertake practical works, they will cultivate the habit of objectivity [as Popper advises] in approaching scientific matters. And to achieve a reasonable practical exposure, trips to industries, farms and research institutes should be encouraged. These kind of trips, if properly co-ordinated, will no doubt dymystify all the theories students have been cramming all along without proper digestion. When it is stated, for instance, that current flows, that

10) Novikova, p. 7

molecules combine to form complex elements, that acid and base dissolve to form soap, students are definitely at a loss. But when they are made to witness some of these assertions empirically, its assimilation becomes quicker. In fact, all the myths surrounding science, as Popper remarked, vanishes once they are subjected to critical evaluation. And this critical evaluation can only be achieved by experimentation and testability, in the normal scientific procedure of making a proposition, gathering equipments, carrying out tests, carrying out observation and reaching a conclusion. Pupils should be allowed to enjoy the benefit of subjecting scientific theories to a critical evaluation through empirical tests.

Another point that is worth mentioning is the obvious fact that many pupils are not keen in making careers in science. This should not be allowed to continue. There should be a conscious effort to cultivate interest in science in as many pupils as possible. Thus, besides the immediate material incentives that should be given to prospective scientists, there might be the need for indoctrination of a kind. It is a well known fact that most pupils are attracted to study law when they see how gorgeous lawyers and judges dress; just as many want to be journalists when they see all the glamour and the excitement that go with journalism. In the same vein, there is the need to indoctrinate pupils right from the primary school level that science, as an academic enterprise, is prestigious and carry privilleges both in public perception and personal remuneration. The respect that scientists command among the members of the society should be emphasized: how they have changed the life of man by understanding and harnessing the laws of nature. If this indoctrination is properly done, and early enough, it will certainly bring out the best in those that already have the potential to be scientists.

There is besides, the need for a change of system as to the ways that professional scientists such as engineers, medical personnel and technologists are trained presently in the country. The present system is defective and has obviously not served our purpose well enough.

The viable system to adopt in the training of this set of scientists is that, at the completion of post-primary education, students who have the potentials to undertake careers in science should be made to undergo preliminary training in the basic sciences such as chemistry, biology, physics and mathematics. At the end of four-year programme, those who are good in physics and mathematics should be encouraged to undertake training in engineering, those who are exceptionally brilliant in biology, physics, chemistry and to some extent, in mathematics, should be encouraged to train as medical doctors or pharmacists. The others who cannot cope very well in any of these professions should be encouraged to undertake post-graduate studies in the single disciplines, so also are the set who have natural inclinations for specializing in the single disciplines. This set of students are going to constitute the future researchers as well as educators of would-be scientists in the country. It is to be remarked that great scientists that have made discoveries such as Albert Einstein, Lavorsier specialized in physics and chemistry in that order.

The essence of this system is easily discernible. Foremost, it makes for a smooth entry into any of these core-professions that are generally seen to be difficult. A student that is keen on studying engineering, who started first with physics and mathematics, is definitely on the right course, just as a prospective student of medicine who starts with physics, chemistry and biology. Besides the obvious advantage of easy entry into the stated core-professions this system makes for maturity and thorough mastering of the theoretical frameworks of the professions one intends to enter. Physics and mathematics and to some extent, Chemistry are the bedrock of engineering. If a prospective engineering student has a proper grounding in them, his problem of making a career in it is half-solved. The same situation is applicable in physics, chemistry and biology as they affect medicine and pharmacy.

There should also be a well organised programme for the set of pupils who either dropped out of the secondary schools or could not go beyond it. This set of pupils should be encouraged to acquire technical skills. In this wise, the huge amount of money being currently wasted in the importastion of equipments that are hardly installed for use for the 6-3-3-4 system at the post-primary level should be usefully employed in building technical schools all over the federation. The standard of such schools should definitely be below those of colleges of technology. Its orientation should be towards the acquisition of practical skills. Since the beneficiaries of this programme should be the set of pupils that have acquired at least basic education, it is expected that they should be able to absorb instructions easily. Their areas of studies should cover technical, secretarial and vocational education. Thus, such trades as mechanics, elementary electricity, typing, sewing and weaving, cabinet making should be taught. The graduates of these institutions will definitely constitute the vanguard for the nation's technological development of the lower cadre. And if meticulous planning is done and enough resources invested in the programme, it will be discovered that the days of having drop-outs dotted all over our streets will be gone for good. In addition, the long queue of applicants with little or no skill besieging government institutions and private concerns for employment will drastically reduce, for everyone would have been given the opportunity to actualise himself; to be useful to himself and the society at large, which agrees with the national philosophy of self-reliance.

And since the nation needs high level manpower for complex and sophisticated scientific adventure, there is no reason why enough resources should not be pumped into our universities to enable them meet up with such challenges. To train scientists with obsolete or sub-standard equipments is like sending a soldier poorly trained into war-fronts to face enemies that were better trained and sophisticated. The scientific world is competitive with every nation guiding whatever skills she acquires jealously. Thus, the talk of technological transfer low or high is simply self-deception. And as Susu, Agu, et al pointed out,

> some people talk about technology transfer with the expectation that all the benefits of technology will accrue once the technology in advanced countries is transplanted to us. Such talk in the existing circumstances is like wishing a fruit tree to be transplanted to a poor soil which has not been prepared to suit the germination and survival of the tree. 11

11) Susu, Agu, etal, National Curriculum for Junior Secondary Schools, Vol. 2C Introductory Technology, Agriculture, Home Edonomics (Lagos, Heinemann, Educational Books (Nigeria) Ltd. 1985), p. 2

The above analogy applies to a large extent to the state of affairs in our quest for advancement in science and technology. We need to lay a solid foundation before this quest could be realised. Besides, it is ridiculous that the nation should be crying for the dearth of well-trained scientists and yet the few ones that are great assets to the country are not encouraged to stay to contribute in building the nation. Equally, there have not been sufficient efforts made to attract Nigerian scientists home. As Joe Garba, the United Nations (UN) General Assembly President pointed out, "It was a question of making proper proposition for Nigeria's technologists now exhibiting such skills overseas to come back and do it here."[12] He believes that to attract this set of people home there is the need to propose adequate incentives and 'conducive practising atmosphere' for them. Garba is convinced that African nations, Nigeria inclusive, have not done enough in a bid to acquire the requisite science and technology for which the Western world is known. Joe Garba is definitely correct in stating our attitude to the development of science and technology. It is ironical that Nigeria spends so much hard currency paying foreign experts and yet nothing substantial is done to attract our own indegenous scientists home from abroad. Worst still, those who are at home practising are frustrated out as a result of hostile environment. There is the need for serious and urgent actions in this line to stem the tide: our human resources (home and abroad) should be provided with enough favourable conditions to practice here. If half of the resources spent annually on the expatriates is spent on our indigenous scientists, there is definitely no reason why they should prefer to practice abroad.

12) Joe Garba "African Nations blamed for technological backwardness" in the Daily Champion, Vol. 3, No. 6, January 6, 1990, p. 9

The same attitude of lethargy is extended to research programme. Not much has really been done in respect of this as we shall see in the preceeding sub-section.

4. RESEARCH PROGRAMME

The nation cannot boast of a developed research culture that can ensure a steady growth in science and technology. And, as Popper remarked, "... continued growth is essential to the rational and empirical character of scientific knowledge ... if science ceases to grow it must lose that character."[13] And that we are not growing scientifically is not as a result of dearth of research institutes in the country. There are several of them all over the country with each having its specific areas of interest such as medicine, agriculture and technology. But in spite of the existence of all these research institutes, the nation is yet to make a resounding breakthrough in research programmes. We have continued to rely on the achievements of the ingenious scientists of the Western world as our platforms to the good things of life.

The reasons for this are not far-fetched. The research institutes are run like the civil service. At the beginning of each year, they get their mandatory statutory allocation. And up to 90% of this allocation is spent on personal emoluments and salaries. And out of the 10% that is left, a part will illegally go into private purses and the remnant will be invested into research findings.

And how serious are the researches carried out? This is a crucial question that needs to be answered. It is most unfortunate that Nigerian scientists have not taken it upon themselves to make bold conjectures, propose a problem and look for its answer as Popper advises.

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13) Popper, Conjectures and Refutations, p. 215

Even from Feyerabend our researchers may have a lot to learn from. They should be able to break away from conceptual strait-jacket as adherence to rigid methodology is not to their interest. In this wise piece meal engineering of their object of investigation may be very useful. At any point in time, a system of approach to the tackling of a given problem could be evolved as the situation dictates it; which is to say that a researcher does not need to tie himself to the apron string of obsolete methods. This approach may make room for advancement of knowledge. And as Feyerabend stated" ... the idea of a fixed method, or a fixed theory of rationality, rests on too naive a view of man and his social surroundings."[14] For a solid science culture for Nigeria, our scientists do not need to be tied to any particular method. Problems should be tackled as they come with appropriate system. But sad enough our scientists are yet to appreciate this fact. They have not

14. Feyerabend, Against Method, p. 27

come to appreciate Popper's contention as Feyerabend points out that "an investigation starts with a problem."[14] As Garba also stresses, we (scientists) "have not gone out to do it ourselves."[15] Thus, besides the issue of poor financing, Nigerian scientists are not adventurous enough, they are scared of looking for new ideas - which is unfortunate.

While it is quite in order to make the above points, there is nonetheless the issue of what have really been done with the findings of some research works. This is where the nation as a whole, particularly those at the helm of affairs should be blamed. Some of these findings are either allowed to die at their embroyonic stages or the idea is exported abroad for further development which at long last does not serve the interest of the nation.

For Nigeria to be able to make appreciable impact in the science and technological world, the issue of research has to be taken seriously. The research institutes need not be run like the civil service where appointments are done by every criterion other than merit. This factor has crippled most of the research institutes in the country. A man who knows close to nothing as far as science is concerned is appointed to head a research institute. And such promotion of mediocrity will breed nothing but incompetence.

Our research institutes, should be headed by renowned scientists, whose interest in science and research work goes beyond petty considerations such as self-aggrandisement. It will then be the responsibility of such leaders to decide who should be recruited and what responsibilities to assign him.

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14) Feyerabend, Against Method, p. 26

15) Garba, p. 9

Then specific challenges (assignments) should be given to each of them from time to time, depending on the area of interest. It will be the responsibility of the federal ministry of science and technology to monitor what progress each is making on any assignment given to it. And given that all the resources, finance and materials which they need are provided then there will not be any excuses for each of them not to justify its existence.

There is no reason why our industries should be crying for raw materials on which they spend a lot of our scarce foreign exchange, while we have universities and research institutes that are supposed to be of immense assistance in sourcing these locally. The industries should by right, from time to time, be making requests on their needs through the federal ministry of science and technology which should in turn be channelled to the appropriate institutions. And they (the industries) should be prepared to fund such researches. Research institutes that are unable to live up to expectation should have no reason to continue in existence.

Discoveries by the nation's research institutes should not be ignored i. Whatever finding that is made by any research institute should be fully commercialized: either by selling its patent right to a firm that can afford it, or the right reserved for the institute that made the finding, while the federal government will assist it to go into its mass production for purposes of sales to the public.

It is very disturbing that the country still tolerates the exportation of unprocessed agricultural raw materials such as cocoa and rubber. And invariably when these products are processed abroad we turn around to import them at exhorbitant prices. This can only happen in a country where there is no adequate science culture and genuine anxiety to join the rest of the advanced world in technological break-through. This issue takes us to our next sub-section - Agriculture - where we have suffered one of the greatest set-backs in spite of abundance in human and natural resources.

4(a.) AGRICULTURE

We are in a tropical region with soil types that grow a wide variety of crops. We are blessed with a vast area and our climate is not hostile to farming. There are agricultural institutes located all over the federation. In spite of this obvious favourable conditions the country still spends huge amount of foreign exchange annually in food importation. This, again, is the result of a lack of science culture and in this case, the neglect of the agricultural sector. Olulade stressed this point, among others, (that we are responsible for our continuous importation of food and raw materials) when he stated thus:

> the neglect of the agricultural sector coupled with the import substitution of the government, ... low import tariff, and other price distortion made agriculture less competitive. All these encouraged massive importation of food, industrial raw materials and other intermediate inputs. 16

This state of affairs has persisted in spite of the clarion call by the government to the populace to go back to the land.

That nothing substantial has been achieved, even in the face of all this call, implies that the right steps are yet to be taken. It is a fact, for instance, that most of the food we eat in this country is provided by farmers in the hinterland. These farmers work themselves almost to breaking point in carrying out this important task.

16) K. R. Olulade "The march towards greater food production" in The Nigerian Interpreter, Vol. 3, No. 2, March/April, 1989, p. 21

Yet their efforts are meagerly rewarded. And this situation is created by conditions beyond their control, which persist because of lack of sufficient interest in their affairs by the government.

However, there have been efforts recently by the federal government through the Directorate for Road and Rural Infrastructure (DFRRI) to reach the rural areas, but only a fraction of them have been reached as at now. Most of the problems of the rural dwellers still persist. An average farmer in the hinterland still makes use of obsolete cutlasses and hoes. He has to burn his bush to prepare it for cultivation. He cultivates low yield crops, and makes use of little or no chemical to enhance the growth of his plants and stop pests from pestering them. There is a fertilizer factory in Nigeria, and its major raw material comes from crude oil (and Nigeria is an oil producing nation), yet the nation imports tonnes of fertilizer annually. Another sad dimension to the problem is that rural farmers hardly get these fertilizers to use.

The agricultural institutes we have in the country, have not done enough to help the rural farmers. All their research findings, especially hybrid crops are yet to be introduced to the rural farmers. There have not been any conscious effort to educate them on the most up-to-date method of farming. They still make use of most out-dated and primitive system of farming with resultant low productivity.

Storage system is another major problem that rural farmers have to contend with. The problem of storage is threatening agriculture in general; it is not a peculiar problem of a rural farmer. Olulade again points out this problem when he says:

> Notable among the problems facing the sector [agriculture] is lack of modern storage facilities to help farmers store their products in order to stem post-seasonal harvest losses. This problem according to experts is responsible for up to 40% loss of the total farm outputs and is indentified as one of the key factors responsible for a shortfall in total food production. 17

This issue [storage problem] has even forced the farmers to sell their wares at rock-bottom prices, which is even preferable to the quantity that is merely wasted. The impact of this on farmers and their productive capacity has continued to affect the nation adversely.

Amidst this situation, it seems there is the need for the nation to have a practicable agricultural policy, that will boost our food production, to feed the teeming population as well as provide raw materials for our industries. This calls for large-scale agriculture. There is, however, the need for a special attention to be directed to the rural farmers. They need to be educated in the most modern techniques of farming. They require a steady supply, at subsidized rates, materials necessary for the control of pests in addition to fertilizers that will enhance the growth of their plants. Most importantly, there should be functional storage facilities at their disposal which will minimise the extent of losses they sustain every year. There is also no reason why we should continue to export our primary produce abroad, which we in turn import as finished or processed goods for our domestic consumption. If the nation is able to develop ways and means of processing our primary products that are greatly needed by the industries, the culture of waste will be minimised, and even the surplus that will be exported abroad will attract a substantial foreign exchange than the primary products presently do. Besides, there is the need for our agricultural research

17) Ibid. p. 22

institutes to embark on discovering hybrid crops which knowledge should be made available to all the farmers in the federation. With intensive research even some brand of crops which we believe cannot grow here will be found to be suitable in some parts of the country. It is for these kinds of researches that the research institutes were established. And as Biosiya points out,

> some of the objectives of the International Institute of Tropical Agriculture (IITA) and the National Root Crop Research Institute, Umudike (NRCRI) are to develop and improve varieties of cassava, cocoyam and other crops with high yields, resistance to major disease, insect pests, namatodes, wide adaptability, good storage characteristics and high consumer acceptance. 18

If the institutes are able to accomplish these goals, and make the knowledge available to farmers, the nation would have been on the right course to self-sufficiency in food production.

The rural areas also need infrastructural facilities such as road, water, electricity, hospitals, among others, so as to improve the conditions of their existence. It is only with such that they will have enough strength and enthusiasm for their farms. On this issue nonetheless "the activities of DFRRI at opening up the rural areas through road construction, water and electrification schemes are commendable." [19] Its activities, however, need to be streamlined to achieve more result. Most rural areas are yet to benefit from their activities.

What the nation needs to be self-sufficient in food production is dynamic policies matched by actions. Larry Koiyan, the Chairman of Directorate of Food, Roads and Rural Infrastructure (DFRRI), puts this point clearly as reported in an interview with Akpasubi, "Nigeria ... must avoid a spasmodic approach to the food crisis: A calm and

18) B. Biosiya "Cassava and Cocoyam" in The Nigerian Interpreter, Vol. 3 No. 2, March/April, 1989, p. 20
19) Olulade, p. 22

co-ordinated effort to solve the problem must be put in place. No more, no less."[20] A country of over one hundred million people, blessed with abundant natural and human resources, should have no reason to import food from across her shores. The same attention is what is needed to harness the potentials of our traditional medicine which is presently in disarray. A detailed discussion on this is given in the next sub-section.

4(b) TRADITIONAL MEDICINE

Traditional Medicine: this is another area where the nation's enormous resources are being wasted unceasingly as a result of the inability of the government to fashion out a positive policy on it. A lot has been said about traditional medicine, its essence and the need to harness its potentials. But very little has actually been done. And yet, the government has health as its priority programme where substantial amount is being expended annually.

There is no doubt that the Western medical practice has not really lived up to expectation. Their practice is still very much dominated by trial and error. Predictability, which is a cardinal feature of science, is still very much at a minimal stage as far as the practice is concerned. Some chronic ailments have defied their ingenuity. In such cases, the much they have succeded in doing is to apply palliative measures aimed at lessening the pains and at times prolonging the life of its victims. Total cure has been impossible. In this regard, one can count such ailments as diabetes, hypertension, sickle cell and most recently, aids (acquired immune deficiency syndrome). Even very common ailments as malaria, muscular pains and injuries have not been tackled with very positive and certain

20) Jackson Akpasubi "The Rural Revolution Manager" in The Guardian, Sunday, Vol. 6, No. 4, 607, February 4, 1990, p. 5

results. Mentally deranged patients are known to have spent years in psychiatric hospitals without any improvement in their conditions. Some had even died in the process of undergoing treatment.

These are on the part of the efficiency of medical Doctors. In the pharmaceutical department, the story is more pathetic. Is it not sad that all the pharmaceutical companies in Nigeria operate either as store-keepers to parent companies abroad or as motor-assembly plants? Virtually all the raw materials they need for production are imported from across our shores. What Nigerian pharmacists do really is merely to get these imported chemicals, dissolve them, place them into sections and mix them together as drugs. In spite of this, drugs are not available in the market. When they are there, their prices are most prohibitive. Thus, the medical Doctor's inefficiency is worsened by the non-performance of the pharmaceutical sector. It is even more surprising that most drugs are brought into the country at their consumable stage. And yet, the nation can boast of thousands of qualified pharmacists and dozens of pharmaceutical companies. What they (pharmacists) seem to be more interested in is to edge out the patent medicine dealers so that they will be in full control of dispensing drugs to end users.

The situation in our health sector, as it is presently structured, is pathetic and unacceptable. And this underlies the need for a full exploitation of all possibilities of our traditional medicine. This requires a lot of work in form of research and production. At the stage it is now, a lot of problems are involved. The activities of the traditional practitioners are so unco-ordinated which accommodates even charlatans, that do a lot of harm to the practice. Besides, the ingenuity of most of the practitioners is personally-centred, as there has not been thorough research on this, which makes no room for universalisation of such ideas that can enhance access to willing and potential practitioners. There have not really been intensive scientific research on the herbs they use with a view to improving on their qualities and more especially to allow mass production and adequate preservation so that they will serve a good number of people. It is to be noted too, that practitioners of any profession require training for the acquisition of requisite skills or to improve the one they already have. In traditional medicine, there has not been co-ordinated and elaborate arrangement to train practitioners. This has made the practice to look primitive and crude and unreceptive to new ideas. Besides, there are problems of measurement in traditional medicine and lack of predictability which are important features of science. There is also the absence of consistency, which is equally very essential. Consistency makes for improvement of ideas and more especially easy references to past works.

These are the fundamental issues that call for attention in our traditional medical practice. And some of these factors deny it the character of objectivity which Popper sees as inevitable in scientific practice. And the fact that, in spite of all these constraints, it has continued to contribute to tackling our health problem is enough to make the modern scientist to accept the challenge of investigating it properly. It is true that Western trained Doctors have constituted major obstacles in institutionalising traditional medicine. As pointed out by Okediran, Professor Lambo, a former WHO (World Health Organisation) deputy director general, stresses this point when he said that,

> Western-trained doctors have been brainwashed to be unreceptive to change and to consider their practice as superior to alternative and traditional medicine which majority of our people in the rural areas rely on. 21

He went further to call for the integration of traditional medicine (alternative medicine) into our health care delivery.

We do not have a better option than to do this, especially when it is realised that traditional medicine in its practice makes use of the principle of probability just as is the case in the Western medical practice. And the principle of probability is very much permissible in scientific research and practice. And as a matter of fact, traditional medicine just in the spirit of scientific research, proceeds by identifying a problem, diagnosing it, carrying out a number of tests, observing before reaching a conclusion. There are, however, a number of metaphysical assumptions that are made which are not absent anyway in the Western scientific method.

While the above facts remain valid, a number of steps still need to be taken to bring out the best from the traditional medical practice. There is first of all the need to identify who is and who is not a traditional healer. It is not every claimant that should be taken seriously. In line with this, there should be a conscious effort to train practitioners. This remains an essential feature of science - to train people in its theories, subject matters and methods. And to execute this project, there has to

21) Wale Okediran "Doctors under the scalpel" in The Guardian, Vol. 6, No. 4, 593, Sunday, January 21, 1990, p. 5

be a designated institution charged with the responsibility. The target of such institutions should be to train candidates on the modality of traditional healing methodology. Such institutions could be made annexes of existing colleges of medicine; this looks the most appropriate thing to do. The essence of this is to promote co-operation among the two distinct practices. There is definitely what each of them should benefit from the other. The former director general of WHO (World Health Organisation), Halfdan Mahler, made this point as highlighted by Okediran when he stated thus: "let us not be in any doubt, modern medicine has a great deal still to learn from the collector of herbs."[22] There is also a lot that traditional healers should learn from Western-trained Doctors.

Beside training practitioners, there is also the need for the existence of a research institute made up of traditional healers whose speciality is on herbs identification, and Western trained scientists. This set of people will have the responsibility of identifying and analysing herbs that have healing powers. They should also be bothered with how to mass-produce and preserve them for present and future consumption. To accomplish this task requires a lot of research: experiments, observation and critical evaluation, bearing in mind that it is life that is involved in the whole affair. A thorough work on this too, will equally enhance the integrity of and predictability on the curative power of our local herbs. There will also be a more precise way of offering

22) Ibid. p. 5

explanation in the various procedures involved in its application.

The issue of empirical verifiability is no doubt to be raised at any step taken by traditional healers. This should not be a problem so long as it is realised that the essence of all steps taken in traditional healing method cannot all be empirically demonstrated. The issue of sacrifice and the idea behind this really come to mind. This is where traditional medicine differs from the Western system. And the belief system in African culture informs this (sacrifice). Professor Wande Abimbola highlighted this point when he was analysing the essence of sacrifice in healing in Yoruba culture. He stated that,

> the Yoruba hold the belief that medicine alone cannot cure a sick person. To them medicine only helps the body of the patient to regain its normal function. But sacrifice is also essential for keeping the soul at rest and in agreement with the ancestors and the gods. 23

This assertion is to a great extent the belief in African traditional healing system. The traditional healers believe that the sacrifice they make has meanings. And, in fact, to those who believe in the existence of gods and ancestors which are real anyway in the context of African culture, sacrifice is not an empty indulgence. And this is not supposed to be a major point of disagreement between the traditional healers and their Western trained counterparts. Its relevance is significant in diverse ways. It shows the African belief in the immortality of the soul. And as Makinde points out,

> the soul is the thing that gives life to the body. And because its essence is life, the soul does not admit the life's opposite which is death. 24

23) Wande Abimbola "Ifa as a body of knowledge and as an academic discipline" in the Journal of Cultures and ideas, An African Journal of Trans-disciplinary studies, Vol. I, No. 1, December, 1983, p. 2

24) M. A. Makinde "Immortality of the soul and the Yoruba theory of seven heavens" in The Journals of Culture and ideas, Vol. 1, No. 1, December, 1983, p. 31

The ancestors died only physically not spiritually. Thus, they still have power over the living. The belief system of the Africans on life is that life is a continuum, even when one is dead, one's spirit lingers on and one keeps in touch with the living, especially those of one's lineage.

Another significant point in making sacrifices in African healing system is that such things as ailments are believed to be punishment for crimes that one has committed. And crimes, once committed, are believed to affect in addition to the criminals, the entire social order. Thus, even after one afflicted with physical ailment is cured physically, one needs to be healed spiritually by ways of appeasing the ancestors and the gods. It is only when this is done that complete healing is believed to have been done. This sacrifice, as Arinze pointed out, is essentially meant "to remove abomination."[25] It is to be remarked that it is mostly in very serious ailments that references are made to causes other than the physical. And in actual fact, when one's spirit and conscience are fighting one as a result of a crime one has committed it will not be surprising that a serious ailment will follow. Definitely, the medical science does not go beyond physical aspect to establish the causes of ailments, and this is, of course, in consonance with the orientation they are exposed to. The Africans go beyond physical to spiritual and this cannot constitute any impediment to the development of medical healing. This is because a sick person is not asked to go and make sacrifices without the prescription of concoction made from herbs which he has to take

25) F. A. Arinze, Sacrifice in Ibo Religion (Ibadan University Press, 1970), p. 35

alongside. In essence, the contention of Ohaeri that "the widespread belief in the supernatural causation of disease can militate against society's evolution in medical thinking",[26] cannot be defended. The belief in supernatural causes does not stop healers from desperate search for herbs for his patients. The reference to the supernatural and subsequent sacrifices are essentially to calm the spirit and soul of the diseased. At least, at the completion of the sacrifices recommended, there is a psychological relief, even before the real ailment is cured.

Thus, in a co-operative venture between traditional medicine and the Western system of medical practice, some of the metaphysical assumptions made in traditional medicine could be accommodated. There should not be any insistence for complete empirical testability, but there should be questions on its rationality, such that steps that cannot be rationally justified should, with the passage of time, be done away with. This is in accordance with the spirit of scientific enquiry.

There is every need and urgency to harness all the potentialities in our traditional medical practice. If this is successfully done, the impact it will make on our healthcare system will be tremendous. It is, in addition, a venture that will serve as a vote of confidence in our indigenous science and technology - which is a credit to our culture.

On the whole, Nigeria needs to evolve and nurture a science culture. It is a ruse to believe that we shall have a transfer of Western technology to the country. The only thing that can alleviate

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26) J. U. Ohaeri, "Articulating a new Philosophical basis for traditional Medicine Practice in Africa" in <u>the Journal of African Philosophy and Studies</u>, Vol. 1, Nos. 1 & 2, 1988, p. 6

our plight in scientific development is the harnessing of our human and natural resources with dynamic and implementable policies.

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