AN EVALUATION OF IMRE LAKATOS' ATTEMPT TO RECONCILE NORMATIVE AND DESCRIPTIVE ACCOUNTS OF SCIENCE

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DECLARATION

This thesis is my original work and has not been presented for a degree in any other university or any other award.

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DEDICATION

I dedicate this work to my Parents: Mr Wallace Njenga and Regina Njenga. Wishing them peace, health, happiness and longevity.

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ABBREVIATIONS

IBE- Inference to the best explanation

HS- History of Science

PS-Philosophy of Science

NAS-Normative account of science

DAS- Descriptive account of science

NPS- Normative philosophy of science

DPS- Descriptive philosophy of science

MSRP- Methodology of Scientific Research Programmes

HC- Hard core

SSR- Structure of Scientific revolutions

LSD- Logic of scientific Discovery

PB- Protective Belt

PH- Positive Heuristic

NH- Negative Heuristic

LMSRP-Lakatosian Methodology of Scientific Research Programme

RP- Research Programme

PRP- progressive Research Programme

DRP- Degenerative Research Programme

DEFINITION OF TERMS

A descriptive account: Is the conceptual understanding serving mainly to label, describe, or classify. It is also called the informative account of science.

A Normative account: Is an account that emphasizes on how to comply; it involves statements that make claims about how things should or ought to be. It is also referred to as the prescriptive account.

Falsification: Is a theory by Karl Raimund Popper that holds that science works by attempting to falsify theories rather than proving them to be true. A theory which is shown to be true is discarded or at least modified.

Logical Positivism: is a philosophical doctrine formulated in the 1920s in Vienna. It holds that scientific knowledge is the only meaningful knowledge and all traditional metaphysical doctrines are to be rejected as meaningless.

Paradigm: Signifies a set of domain assumptions that in a strong sense serve to define a field of study. It is made up of the general theoretical assumptions, laws and techniques for the application that the members of a particular scientific community adopt.

Positivism: a theory by Auguste Compte that holds that any system that confines itself to data of experience excludes apriori or metaphysics.

ABSTRACT

In philosophy of science, philosophy has the role of analyzing the foundations, methods and implications of science. This role involves analyzing the way science ought to be and the way it is. In this quest, the normativist philosopher of science argues that it is only possible to understand science in reference to certain norms and standards. The descriptivist philosopher of science on the contrary argues that it is only possible to understand science in reference to its actual activities. This means there is a controversy between the normative and the descriptive accounts of science. Karl Popper in his theory of falsificationism, gives a picture of science that is normative in character. Thomas Kuhn on the contrary, in his theory of scientific revolutions, gives a picture of science that is descriptive in character. When Imre Lakatos perceives this diversity, he thinks that he could reconcile it using his methodology of scientific research programmes. He is of the view that it is possible to give a picture of science that is both normative and descriptive in character. This raises the question on whether he succeeded or not. The problem of this study, therefore, is to evaluate the success of his attempt to reconcile Popper and Kuhn in their normative and descriptive accounts of science. In order to achieve this goal, this study adopts an analytic, descriptive and synthetic method which enables a successful critical understanding of the relevant primary and the secondary sources. The study finds out that Lakatos in his attempt partially succeeds (the strengths of his methodology) and partially fails (the weaknesses of his methodology). Nevertheless, the study concludes that Lakatos provides an account of science that is far much better than either the normative or the descriptive account (each on its own). This conclusion is further informed by Gilbert Herman's theory of 'Inference to the best explanation'; a theory that helps to assert that Lakatos' approach, despite its weaknes, is an account of inference to the best explanation.

CHAPTER ONE

INTRODUCTION

1.1 Background of the study

From the modern time, and since the renaissance, especially since Francis Bacon, science became the measure of everything. Theology, the science of God, was replaced by the science of nature, the laws of God were replaced by the laws of nature, God's power was replaced by the forces of nature. At a later date, God's design and judgments were replaced by natural selection whereas theological determinism was replaced by scientific determinism. In short, notes Popper (1994; 82-83), God's omnipotence and omniscience were replaced by the omnipotence of nature and by the virtual omniscience of natural science.

In the contemporary world, science enjoys a greater prestige compared to other institutions. Scientists are widely considered superior because they are able to gather and interpret evidence which they use to arrive at conclusions that are 'scientifically proven'. Science has pervaded nearly every aspect of our lives and has managed to open up domains of new insights which have led to great technological advancements and innovations. It is therefore one of the most powerful tools that humanity has developed in quest for understanding and controlling the world (Swedin: 2005:91).

Based on these facts, it is therefore necessary to try and reflect on how science works. To begin with, in the nineteenth century, philosophical reflection on science resulted in a tradition of natural philosophy both in Britain and in Continental Europe. In Britain, it reflected in the writings of Mill, Pearson, Whewell, and others. In continental Europe, especially in Austria, it reflected in the writings of Bolzano, Mach, and others. Today, the philosophical reflection on science is what is referred to as philosophy of science or the science of science and it has its roots therefore in both the British and the Austrian traditions, alongside many other influences (Sarkar & Pteifer, 2006:1).

In this task of trying to reflect or figure out how science works, Frank (2004:1) notes that, in order to understand not only science itself but also its role in the world today which includes its relation to ethics and religion, we need a coherent system of concepts and laws within which the natural sciences as well as philosophy, and the humanities, have their place. Such a system is the so called "philosophy of science".

In this system, when philosophers are concerned with stipulating what ideal science ought to do, and in the process, they make claims on what ought or should be the case, they are engaging in a normative account of science. When they are concerned with looking at what is the case in science rather than what ought to be the case, they are engaging in a descriptive account of science. This means that in this system there is a disagreement or a controversy on the best approach or criterion of interrogating science. The question that comes up is; should it be through the normative approach (which is apriori) or should it be through the descriptive approach which (aposteriori)?

This controversy however, is perfectly reflected in other words, in the popular debate that has currently captured the attention of philosopher's of science. This debate is on whether philosophy of science (PS) and history of science (HS) are either principle alternatives or mutually exclusive disciplines. In this debate, the philosopher of science or the normativist philosopher of science on one hand, sets forth and recommends a set of evaluative standards which supposedly contribute to the criterion of good science. The historian of science or the descriptivist philosopher of science on the other hand, does not issue pronouncements about how science ought to be practiced but instead describes how science really is (Losee, 1987: 1-31).

This debate interestingly attracted the attention of Imre Lakatos who thought that both PS and HS should constitute a synthesized approach that is both philosophical (normative) and historical (descriptive). In this spirit, he asserted that *'History of science without philosophy of science is blind; philosophy of science without history of science is empty'* (a dictum he paraphrased from Immanuel Kant) (Lakatos, 1970:91). Lakatos cited this paraphrase as a starting point for his attempt to reconcile philosophical with historical accounts of science. Thus, he argued that in order to perfectly understand science, there should be a harmonized methodology that integrates philosophy of science (PS) and history of science (HS).

In order to understand into details the two sides of the debate (the normative and the descriptive), it is a prior condition to interrogate each account on its own. Since Karl Popper and Thomas Kuhn provided the main works that capture this debate, this study uses their theories to interrogate each side respectively. In the normative account therefore, Popper being one of the main proponents is the typical representative. In his theory of falsificationism, he argues that a system should be considered as empirical or scientific only if it is capable of being tested by experience. In his view, the job of a scientist is to subject hypotheses and conjectures to the severest possible tests and to temporarily accept those that passed the most severe tests. The scientist should abandon a theory as soon as they encounter any falsifying evidence and immediately replacing it with increasingly bold and powerful new hypotheses (Popper, 1959: 19). For him, a philosopher of science is a methodologist whose role is to propose a series of methodological rules and norms that promote the growth of knowledge.

In the descriptive account, Kuhn is among the main proponents and a typical representative. As a descriptivist, he challenges the traditional views about the nature of science and the prospects of scientific progress. Unlike Popper, Kuhn presents an image of science that aims at analyzing and describing how science works. He argues that science should be descriptive, informative or explanatory rather than prescriptive or normative in character. For him, the historian of science seeks to create a narrative which has explanatory force, a narrative within which facts fall into a recognizable pattern (Kuhn, 1962: 70).

Consequently, in reaction to the perceived disagreement, Lakatos (1970:91) asserted that “Philosophy of Science without History of science has no guide (is blind) and history of science without Philosophy of Science has no substance (is empty)”. By this he meant that, science should not be evaluated according to the blind falsifiability criterion of Popper; nor should it be rejected as essentially a social exercise with little relation to objectivity of reality, as Kuhn argued. Instead, science should be viewed as a development of research programs some of which are progressive while others are degenerative, each with its own hard core, protective belt, and negative and positive heuristic. For him, this approach which he exemplifies through his methodology of scientific research programmes, should perfectly blend the normative picture of science by the philosopher of science (Popper) and the descriptive picture of science by the historian of Science (Kuhn).

This claim as a result, raises our eye blows because it brings about the question of its success in reconciling the perceived controversy between the normativist and the descriptivist philosopher of science.

1.2 Statement of the problem

The preceding background demonstrates that there is a disagreement between the normative and the descriptive accounts of science. Imre Lakatos perceives this disagreement and attempts to harmonise it by providing an account that is both normative and descriptive in character through his methodology of scientific research programmes. Interrogating Lakatos' attempt therefore, necessitates this research and brings about the problem of this study which is to evaluate the success of Lakatos' methodology in reconciling the normative and the descriptive accounts of science. In other words, the study examines the two contrary accounts of science through Popper and Kuhn respectively and then determines the extent to which Lakatos' methodology of scientific research programmes succeeded or failed in providing a harmonised account.

1.3 Research objectives

This research is guided by the following aims and objectives;

1) To examine the normative account of science through Karl Popper's theory of falsificationism.
2) To examine the descriptive account of science through Thomas Kuhn's theory of scientific revolutions.
3) To evaluate the 'methodology of scientific research programmes' through which Lakatos attempted to reconcile the normative and the descriptive accounts of science.

4) To assess the success of Lakatos' model in harmonizing normative and descriptive accounts in Popper and Kuhn respectively.

1.4 Research questions

The following questions guide this study;

1) What is a normative account of science and how does Karl Popper exemplify it in his theory of falsificationism?
2) What is a descriptive account of science and how does Thomas Kuhn exemplify it in his theory of scientific revolutions?
3) How does Imre Lakatos reconcile the normative and the descriptive accounts of science through 'methodology of scientific research programmes'?
4) Is Lakatos successful in harmonizing the normative and the descriptive accounts of science?

1.5 Research Assumptions

This research is founded on the following assumptions:

1) That Karl Popper exemplified a normative account of science.
2) That Thomas Kuhn developed a descriptive account of science.
3) That the methodology of scientific research programmes formulated by Lakatos is an attempt to synthesize philosophy and history of science.

4) That there is a need to evaluate Lakatos' model with an aim of determining its success in harmonizing normative and descriptive accounts of science.

1.6 Scope and Limitation of the study

This study limits its scope to assessing whether Lakatos is successful or not in formulating a methodology that harmonizes the normative and the descriptive accounts of science through Popperian falsificationism and Kuhnian structure of scientific revolutions respectively. It is therefore strictly concerned with interrogating the normative account through Popper' theory falsifcationism, the descriptive account through the Kuhnian theory of scientific revolutions and finally the methodology of research programmes through which Lakatos attempts to give a synthesis to Popperian and Kuhnian diversity.

1.7 Justification and Significance of the study

In several disciplines; be it philosophy, economics, ethics, business, languages, or politics, there is a logical gap between the way things ought to be and the way things are. In other words there is a gap between the normative (ought) and the descriptive (is). Any research on this gap is especially the attempt to gauge the possibility of bridging this gap, is therefore justifiable.

In philosophy of science, this gap is perceived between the normative and the descriptive accounts of science. Imre Lakatos in an attempt to blend this gap formulates a methodology that attempts to reconcile the normative criterion by

Popper and the descriptive criterion by Kuhn. Evaluating Lakatos' attempt is therefore necessary and significant not only for the philosopher of science who tries to understand how science operates, but for the practicing scientists whose role is to find out the why of things.

1.8 Literature review

The purpose of this section is to analyze the literature behind; the 'is-ought' dichotomy or the 'normative-descriptive' dichotomy in different disciplines, the construction of a normative account of science (based on the 'ought'), the construction of a descriptive account of science (based on the 'is'), and the possibility of constructing an account of science that is both normative and descriptive (that harmonizes the 'is' and the 'ought' in science).   .

1.8.1 The is-ought dichotomy in different disciplines

According to Simpson & Weiner (1989), 'ought' or prescriptive or normative means relating to an ideal standard or model, or being based on what is considered to be the normal or correct way of doing something. A word is said to have a normative meaning if its use in any context makes implicit reference to some standard or conduct. 'Is' or descriptive means serving mainly to label, to describe, or to classify. A word is said to have a descriptive meaning if it is used in any context to refer to facts or putative facts.

To Bernardo (1988:79), the 'is-ought' dichotomy or what he names the 'great division' is considerably widespread among contemporary philosophers and lawyers. It permeates all aspects of life. Any philosopher or specialist in every discipline worth the name must focus on the question of ‘what ought to be’ and ‘what is’. For Bernardo (ibid) however, this conceptual paradigm means the semantic distinction between the descriptive and the prescriptive uses of language.

According to Hunt (2002: 40), Hume (1711-1776) in the famous passage of his Treatise (1739/40, p. 177), as he was arguing against the argumentative praxis of ethicists of his time, is generally given the credit for being the first philosopher to point out that statements concerning the verb ‘is’ are different in kind from statements containing the verb ‘ought’. He stated that from the descriptive (what is) (or is not), nothing about what ought to be (or ought not to be) can logically be concluded. This, for Schurz (1997) is what is called Hume's is-ought thesis. In ethics, the important question this thesis brings about is on whether and under which conditions it can be true.

Generally, in ethics, the normative-descriptive dichotomy is in the version of Hume’s ‘is-ought’ dichotomy. Sims (2003:14) points out that normative ethics also called prescriptive or evaluative, involves intrinsic value of right or wrong and has an overriding importance for determining how we ought to act. It seeks to uncover, develop and justify basic principles that are intended to guide behavior and decisions. Descriptive ethics by contrast, is concerned with describing, charactarising and studying the morality of a people, a culture or a society. It focuses on ‘what is’ the case or how people actually behave or how they make decisions.

In linguistics, notes Huddlestone (1984:47), the prescriptive grammar describes when people talk about how language should or ought to be. It aims to tell its readers what grammatical rules they should follow. A good example is when one goes to a doctor’s consultation room and the doctor gives prescriptions for medication, it often includes directions about how one should take medication as well as what one should do or not do when taking medication.

Huddlestone (1984: 48), further notes that descriptive grammar aims to present the grammar that underlies the actual usage by speakers of a certain language. In an example, the descriptive grammarian would see a sentence like ‘to go where no man has gone before and would try to describe how the mental grammar can cause that ordering of words rather than saying that the surface form is faulty due to prescriptive rules’.

According to Simpson & Weiner (1989), prescriptive grammar dictates how one should speak and what type of language one should avoid. Normally, this is found in English classes where the aim is to teach people how to use language in a very particular way that is described as either proper or correct. On the contrary, descriptive grammar focuses on describing the language as it is used not on how it should be used.

When it comes to decision making, most decisions are either normative or descriptive. Bell, Raifa & Tversky (1988:9), note that the issue of decision making for every individual is an act of compromise and the most important task is to know how real people as opposed to imaginary, idealized, super rational people without psyche, can make better choices in a way that does not violate their deep cognitive contents.

For this purpose, Bell Raifa & Tversky (1988: 10) further note that in the usual dichotomy that distinguishes the normative and the descriptive sides (the 'ought' and the 'is') of decision making, there should be added a third component: prescriptive side. The purpose for this is to identify the best decision to make, assuming that an ideal decision maker who is fully informed, is able to make a choice with perfect accuracy and full rationality.

In philosophy of science, the normative descriptive-dichotomy (N-D) also takes the version of Hume's 'is-ought' dichotomy. This is affirmed all the way from Aristotle when he defined science in his physics (184a10-15) as "an asymmetric inquiry leading to the knowledge of properties of things by way of their causes" accounting for the reasons why they behave the way they do and why they have to be the way they are.

Kantorovich (1993:237-238) adds that in philosophy of science, this dichotomy refers to the context of justification i.e., the question whether the methodological rules of confirmation or refutation, acceptance or rejection of any hypotheses are normative or descriptive. The normative makes recommendations to scientists about how to do good science and the descriptive does not make recommendations to scientists but instead analyses the actual behavior of scientists.

1.8.2 Normative philosophies of science.

According to Turner (2010:1) the normative approach or what he calls normativity, is everywhere; it pervades all the realms of our lives. In his view, we do not merely have beliefs; we claim that we and others ought to hold certain beliefs. We do not merely have desires; we claim that we and others not only ought to act on some of them but not others. We assume that what somebody believes or does may be judged reasonable or unreasonable, right or wrong, good or bad, in accordance with certain standards or norms.

Kantorovich (1993:238) notes that in philosophy of science, this approach provides an account of what scientists ought to do by following certain norms and standards. This is affirmed by Bridgman (1960:28) who argues that philosophy of science is a normative pronouncement about the proper relationship between scientific concepts and instrumental procedures, every concept not linked to measuring procedures be excluded from science.

According to Losee (1987:49) from its inception in the early 1920s, the Vienna circle staked out a normative programme for philosophy of science. Carnap, Neurath and Hahn who were among the members of the Vienna circle, issued a manifesto in 1929 which outlined an appropriate criterion undertaken from a prescriptive perspective. Its intent was to prescribe proper procedure to those who would practice science.

Hempel (1959: 108- 129) notes that various criteria of empirical significance like verifiability, falsifiability, confirmability, and translatability into an empiricist language were proposed. Apparently, they were later abandoned as too inclusive or too exclusive.

According to Popper (1959: 34), the efforts of the members of the Vienna circle were futile. He maintained that whether a proposition is empirically significant depends not on some alleged intrinsic meaning that it possesses, but rather on the way in which it is defended.

For French and Saatsi (2011: 18) however, though Popper is not a logical positivist, he shares many of the beliefs of logical empiricism. A key problem for him is in the central issue in general epistemology – Hume's problem of induction and the defining feature of his philosophy is the fact of its being an inductive sceptic, combined with the view that science is rational. His approach is an attempt to reconcile inductive scepticism with scientific rationalism in his theory of falsificationism – a rationally acceptable alternative to inductivism – a methodology which is primarily normative in character.

Popper in his books '*The Logic of Scientific Discovery'* (1959) ', argues that all scientific theories must be falsifiable by the presentation of contradictory data. To be scientific, for him, a proposition must be expressed in a form subject to falsification through observing contradictory data. In his exact words he held that from a logical perspective, a system of theories is scientific only if it is refutable or falsifiable:

> "I shall not require of a scientific system that it shall be capable of being singled out, once and for all, in a positive sense; but I shall require that its logical form shall be such that can be singled out, by means of empirical tests, in a negative sense: it must be possible for an empirical scientific system to be refuted by experience" (1959, 40–41).

Warburton (1999:21) notes that according to Popper, one cannot 'prove' a theory through scientific observation. To assert proof is to commit the logical sin of ignoring possible alternative explanations and therefore what is characteristic of scientific method is not a verificational procedure but falsification.

For Sarkar and Pfeifer (2006), it is thus not too much to say that Popper's perception of the asymmetrical logical relation between verification and falsification that is normative lies at the heart of his philosophy of science. A universal scientific theory cannot, in principle, be verified, but a single counter instance can and does decisively falsify it.

According to Laudan (1984), Popper's influential discussion of the nature of scientific aims or values in his 'logic of scientific discovery', presupposed that the cognitive goals and aims in any account of scientific rationality is to play a key role in justifying methodological rules precisely because such rules are intended as means to the end specified in our vision of aims of science. Popper therefore believed that the aim of science was to develop ever truer theories about the world.

Reichenbach (1938) a contemporary of Popper, took a similar view with Popper. He believed that one could fault someone's methodology by showing that it failed to promote one's aims. Like Popper, he insisted that an agent's purpose or goals, including cognitive ones are not a rationally negotiable matter.

From the above literature, it follows necessarily that the role of the philosopher of science in the normative or the prescriptive approach is to propose and issue recommendations and standards by which scientific theories ought to be evaluated. According to Losee (2001: 264), philosophers of science from Aristotle to Kuhn have sought to develop evaluative standards applicable to the practice of science. These efforts have been informed by a prescriptive intent but some observers have found this prescriptivist project to be a bit presumptuous because for a philosopher of science to educate scientists about proper evaluative science, he must appeal to the actual science as a source of the recommended standards.

1.8.3 Descriptive philosophies of Science.

Contrary to the normative account, Kantorovich (1993:238) points out that this approach is constructed on what scientists do by carefully analyzing case studies from the history of science or for that matter by scrutinizing contemporary scientific practice. This approach is based on the fact that philosophy of science needs to be informed by careful work in the history of science and not just by accepting scientists' own pronouncements about how their work proceeds.

According to Holton (1984), the theoretical physicists of his day appeared to have little interest in the recommendations of philosophers of science because they changed their attitude from normative to descriptive. The descriptivist, he further argues, makes no recommendations about "proper" evaluative practice. Instead, he seeks to uncover the methodological standards and procedures that actually have informed scientific practice.

The descriptivist philosopher of science therefore should take seriously Feyerabend's admonition, "return to the sources". Holton concluded that from the works of Einstein, Millikan, Bohr, Kepler, Mach, and Stephen Weinberg, among others, certain thematic principles have been important in the historical development of science. These principles express scientists' basic commitments about the context of discovery and the context of justification (ibid).

Toulmin (1974: 392) formulated a model for descriptive philosophy of science which is an application of the Darwinian Theory of Evolution to the historical development of science. Toulmin recommended that philosophers of science shift attention from logical relations between propositions to the progressive modification of concepts. He maintained that important questions in science often take this form:

> "Given that concepts c1, c2. . . are in some respect inadequate to the explanatory needs of the discipline, how can we modify/extend/restrict/qualify them, so as to give us the means of asking more fruitful empirical or mathematical questions in this domain?" (Toulmin, 1974: 392-394)

Toulmin held that conceptual development is an "evolution" within which "natural selection" operates on a set of "conceptual variants". It is the "fittest" concepts that survive. Toulmin's evolutionary model matches well with Kuhn's description of scientific revolutions (ibid).

In the 1960s and 1970s the notion of history of science, history of philosophy, and philosophy of science occupied distinct and independent intellectual realms with the contribution of Kuhn, who like Holton and Toulmin argued against earlier contributions.

According to Hanson (1962), views of the neo-positivist, philosophy of science mainstream, explicitly contesting the mainstream dismissal of history of science in principle, are irrelevant to the proper work of philosophers of science. Together with Kuhn in his book "*The Structure of scientific revolutions"* which he published the same year (1962) they believed that a philosophical understanding of science was possible only via the deep historical embedding of the philosopher's work.

French & Saatsi (2011:55-56) note that Kuhn's argument that the traditional accounts of science, whether inductivism or the normative (falsificationism), do not bear comparison with historical evidence. Along with Hanson and Kuhn, there are several philosophers prized as prescient advocates of a historicized philosophy of science, such as Gerd Buchdahl, Ernan Mc Mullin and Georges Canguilhem.

According to Kuhn (1970) the normative account is faulty since it only deals with how scientific methodology ought to look like in line to a set of explicitly declarable rules and norms. Instead he argues, it should be a descriptive account which looks at how science really operates. Scientific change, therefore, is not in general cumulative and progressive, but should develop through a series of distinct stages: immature science (when there is no generally accepted paradigm), normal science (when there is an agreed upon paradigm), and revolutionary science (when there is a shift between paradigms).

Chalmers (1976:90) notes that a key feature in Kuhn's theory is the emphasis placed on the revolutionary character of scientific progress, where a revolution involves the abandonment of a theoretical structure and its replacement by another, incompatible one. This therefore characterizes Kuhnian approach as descriptive in character.

Apparently, Laudan (1977) in his normative naturalism criticized the descriptive account arguing that the term history of science is ambiguous. To explain this he was of the view that in English language it may relate to either the actual past or the writings of historians about the past. In his 1984 book *The Science and Values*, he critiques the logical empiricists and the post-positivists as he stresses the need for centrality and the interdependence of values, methods, and facts as prerequisites to solving the problems of consensus and dissent in science.

Feyerabend (1969:198) in his criticism on Kuhn, said that “I am troubled by the following question: are we here presented with methodological prescriptions which tell the scientist how to proceed; or are we given a description, void of any evaluative element, of those activities which are generally called “scientific”? Kuhn’s writings, it seems to me, do not lead to a straightforward answer”. He was of the view that they are ambiguous in the sense that they are compatible with, and lend support to, both interpretations. To Feyerabend therefore, Kuhn’s account is normative in the following sense: it attempts to tell us how scientists do behave; and how scientists do behave is essentially how they should behave. Kuhn defends his idea by arguing that description and prescription go hand in hand. For Popper, in contrast, they come apart.

According to Losee (2001:276), it might seem that the descriptive approach subsumes the philosophy of science under the history of science and therefore it is easy to think that descriptive philosophy of science can be much more useful (and interesting) than prescriptive philosophy of science. This observation shows that the controversy between the normative and the descriptive account demands a lot attention so as to determine which approach is better than the other.

1.8.4 Attempts to synthesize Normative and Descriptive philosophies of science

In the last two subsections, it has been demonstrated that the normative and the descriptive accounts or approaches to science are in conflict. The picture of science the normativist provide is contrary to that provided by the descriptivst, each approach firmly asserts that it offers a better picture of science. This subsection as a result looks at the literature behind the possibility of constructing a picture of science that captures the two diverse approaches together.

According to Losee (1987:1), since the normative philosophies (signified as philosophy of science) and the descriptive philosophies (signified as history of science) are second order interpretations of first order subject matter, they are open to several possibilities for a relationship between them. These possibilities according to Losee are:

a) That the two are mutually exclusive interpretations of science whereby they share the first order matter. In this possibility, the practice of philosophy of science precludes historical considerations and the practice of pistory of science precludes philosophical considerations.

b) That one discipline is dependent on the other. In this case, philosophy of science may be dependent on history of science and the vise versa either in a strong or a weak sense. In the strong sense, no philosophy of science can be undertaken without historical enquiry and the vice versa. In the weak sense, there are at least some aspects of philosophy of science that require historical enquiry and the vice versa.

c) That the two disciplines are interdependent such that the practice of philosophy of science requires historical enquiry and the practice of history of science requires philosophical analysis. PS and HS may be interdependent in either the strong or the weak sense.

d) That there is a de facto overlap of the disciplines. In this case, although neither discipline is dependent on the other, certain of the conclusions reached within a philosophy of science and history of science are the same.

e) That PS may be subsumed under HS. On this view, PS, like the history of botany and the history of physics, would be part of HS. PS would be that part of HS within which narratives are developed that reconstruct the evaluative practices of scientists. A philosophy of science subsumed under history of science would have no distinctive methodology of its own. Rather, its methodological standards would be those standards appropriate to HS.

Imre Lakatos (1970) takes the third possibility that asserts that the two disciplines are interdependent. He based his argument on the fact that recent philosophy of science has become historical and recent history of science has become philosophical. It is in light of this possibility and in light of the conflict between the normative and the descriptive philosophies of science that he formulates a model that he labels 'the methodology of scientific research programme' under the slogan that *'History of science without philosophy of science is blind, Philosophy of science without history of science is empty'.*

In his methodology therefore, Lakatos aims to combine the normative approach in Popper's methodology and the descriptive approach in Kuhn's methodology into a single model of theory-change, which preserves progress and rationality while it avoids Popper's naive falsificationism, and respects the actual history of radical conceptual change in science.

Agassi (1963), influenced Lakatos argument with his assertion that the accounts given by historians of science were influenced by their philosophies of science, with inductivists constructing inductivist history of science, conventionalists constructing conventionalist history, and Popperians constructing Popperian history.

Fereyabend (1975) as he developed his most challenging and provocative of the contemporary accounts of science welcomed Lakatos as a fellow anarchist because his methodology does not provide rules for theory or programme choice. He says; "the methodology of research programmes" provides standards that aid the scientist in evaluating the historical situation in which he makes his decisions; it does not contain rules that tell him what to do."

Kuhn (1977: 3-20) argues against Lakatos. He is of the view that PS and HS are mutually exclusive disciplines. Whereas the historian seeks to develop a narrative which renders plausible and comprehensible the events it describes, the philosopher seeks to discover and state what is true at all times and places rather than to impart comprehension of what occurred at a particular place and time. Eventually, he posed the following question; "given the deep and consequential difference between the normative and the descriptive accounts of science, what can they have to say to each other?"

According to Gierre (1973), as he responds to Kuhn's question, the association between PS and HS is a marriage of convenience. He argues just like Popper that PS is a normative discipline which sets standards of explanation, confirmation and theory replacement. For him, such an analysis may be undertaken independently of any specifically historical analysis.

Smart (1972) emphasizes on the independence of PS. He notes that the philosopher of science typically selects examples from the historian of science because it is convenient to do so. These examples are selected to illustrate his methodological principles. For him therefore, the fictitious examples would do equally well if the philosopher were sufficiently clever to invent them. He concludes that the Knowledge of HS is not required in the practice of PS.

Feyerabend (1970) in reaction to Kuhn's question supports the association of PS and HS as a marriage of convenience. As he called in to question the entire enterprise of PS, he maintained that the analyses of explanation, confirmation and theory comparison given by the philosopher are of value to neither the scientist nor the historian of science. According to him therefore, PS is an irrelevant academic exercise which may safely be ignored.

However, looking on the same problem differently, a recent study by Mbugua (2014) shows that Lakatos' account of science may be labeled as descriptively adequate but normatively inadequate. Mbugua attempts to reconstruct the history of genetic and evolutionary theories of same-sex sexual behavior through Lakatos' model. He argues his point from the tests for the applicability of Lakatos' model carried in London by Lakatos' students and colleagues at the London School of Economics. He found out that Lakatos' account is weak in stipulating how things ought to be and very strong in explaining how things are.

In other words he found out that Lakatos' model does not perfectly blend the normative and the descriptive accounts. His finding is relevant to this study because it is a recent study testing the applicability of Lakatos' methodology and therefore his findings will further shape the main goal of this study which is to evaluate the normative and the descriptive accounts of science thought the contributions of Popper and Kuhn respectively.

1.8.5 Gap in the literature

The above literature shows that in the construction of the normative or prescriptive philosophy of science, Karl Popper in his methodology of falsificationism is in the front line in formulating an account that stipulates how scientists ought to act. On the contrary, in the construction of the descriptive philosophy of science, Thomas Kuhn in his theory of scientific revolutions is in the front line of constructing an account of science based on analysing the actual behavior of the scientists.

The disagreement between the two approaches, as seen, attracts different reactions. Among them, is Imre Lakatos in his methodology of scientific research programmes who is in the front line of constructing an account of science that attempts to harmonize the diversity between the normative approach (philosophy of science) and the descriptive approach (history of science). He proposes a methodology that is both normative and descriptive in character.

Thus, the gap in this literature comes out when one is intends to find out whether Lakatos' in his attempt actually succeeds in reconciling the two different accounts of science or not. This study therefore is interested in evaluating whether the methodology that Lakatos provides is successful or not in reconciling philosophy of science (the normative account) and history of science (the descriptive account). In other words, the study is interested in determining the extent to which Lakatos in his methodology of scientific research programmes attempts to fill the gap between the 'is' and the 'ought' in philosophy of science and eventually, with the help of the theoretical framework, determine whether he succeeded or failed.

1.9 Theoretical Framework

In order to achieve the aims and objectives of this study, Gilbert Herman's model of 'Inference to the best explanation' will be the guide. IBE ('Inference to the best explanation') was formulated by Gilbert Herman, an American philosopher born 1938. It suggests that a given theory is to be accepted only if it is superior to its rivals on the grounds of its providing a better explanation than its competitors (Bird &Ladyman, 2013).

IBE also called abduction is a process by which we infer everyday intuitively by using common sense. Charles S. Peirce (1839-1914) coined this term (abduction) and justified it on the basis that humans have a natural inclination to the truth, a tendency to guess correctly. For him, our intuition or perception can pragmatically guide our inference as long as we test its consequences (Peirce, 1998).

The reason as to why this model perfectly fits as a guide to this study is because of the fact that in everyday life we are forever inferring, explaining and deriving conclusions about the way things are and the way they ought to be. It is therefore very informative and should be used to the latter in the evaluation of the 'is-ought' dichotomy in science and in any other attempt to reconcile the diversity between the way things are (is) and the way things should be (ought).

According to Bird (1998) the word "explain" can be used in more than one way. We talk of people explaining things and of facts explaining things. When we talk of facts explaining, our notion of explanation is factive, that is to say that the fact in question must exist and it must explain the thing being explained.

Harman (1965) argues that one of the pressing issues of IBE is on how to judge that one guess or hypothesis is sufficiently better than another. As he coined the term IBE from Abduction he however presumed that such a judgment would be based on considerations such as which hypothesis is simpler, which is more plausible, which explains more, which lead to less ad hoc and so forth.

According to Lipton (2004) IBE model is a tool for inference whose process is guided by explanatory considerations characterizing the best explanation among competing ones as the one who would if correct, be the most explanatory or provide the most outstanding explanation. Lipton calls this "the loveliest" explanation. For him, loveliness is about understanding a plausible dimension of explanatory, a promise of unifying hitherto unconnected phenomena.

For Clayton (1997) IBE does not only provide the most adequate model of explanation in science but also a viable methodology for philosophers and theologians. He demonstrated the feasibility of IBE as a methodology in theology by providing theistic beliefs in terms of IBE arguments. He further held that if an argument fails to prove conclusively, which is a characteristic of theistic beliefs; it can still constitute a better explanation for some phenomena than any of its competitors.

According to Peacocke (2004), biological evolution and Christianity needs to be at odds, advocates to the use of IBE in theology. He explained that it is IBEs emphasis on plausibility; rather than an absolute requirement for deductive proof that allows theology to adopt this model of explanation more readily while the model is also an adequate guide for science and everyday life. In short, he pointed out that, IBE makes possible the reunification of scientific and philosophical method.

In this study however, IBE provides an epistemological basis for evaluating and assessing whether Lakatos methodology of scientific research programmes is the loveliest explanation. In the controversy between Popper and Kuhn, Popper objects earlier arguments by inductivists on the growth of science and thinks that his methodology of Falsificationism is a better explanation to how science progresses.

Kuhn on the contrary, argues that Falsificationism is naive and therefore discarded any kind of Falsificationism arguing that his theory of scientific revolutions is the most appropriate explanation of how science progresses. Lakatos (1970) attempted to reconcile the two; he labeled their theories as Popperian and Kuhnian research programmes. For him, his theory is the best explanation.

1.10 Methodology

The methodology that this study uses is primarily library research because philosophy of science is a second order subject. It involves a careful philosophical analysis, examination, explanation, description and evaluation of primary and secondary materials that are relevant to this study. These materials include books, scientific journals, publications, commentaries, critiques and philosophical papers on science, philosophy of science, logic, ethics and grammar. The study however majors on the works that focuses on the 'normative-descriptive dichotomy' in philosophy of science and the criteria of identifying these materials is not through author catalog but subject catalog.

The study narrows down to four methods: Analytic, descriptive, synthetic and evaluative. The study uses the first two methods (i.e. the analytic and the descriptive) to examine the normative and the descriptive accounts of science by Popper and Kuhn respectively.

Taking into consideration the diversity between normative and descriptive accounts of science (in Popper and Kuhn), the study uses the third and the fourth methods (i.e. the synthetic and the evaluative) to scrutinize how Lakatos harmonizes the diversity and to evaluate the outcome of his attempt.

CHAPTER TWO

POPPERIAN NORMATIVE ACCOUNT OF SCIENCE

2.1 Introduction

The main objective of this chapter is to examine the normative account of science through Karl Popper's theory of falsificationism. The intention here is to identify features of a normative philosophy of science and then determine whether these features are in Popper's theory falsificationism. In order to achieve this goal, this chapter analyzes the meaning of science, Popper's response to the inductivism, Popper's normative problem of demarcation and finally Popper's theory of falsificationism.

2.2 The meaning of science

As pointed out earlier, today, we are in the age of science. Throughout this century, man has made extra ordinary progress both in his understanding of the universe, and in his use of scientific knowledge to improve the quality of life. According to Ravetz (1971), the vigor and the successes of scientific research and of the effectiveness of its technological application, are mostly seen and accepted as indicators of the quality of a nation's life. It has also been widely acknowledged as the exemplar of rationality in the pursuit of reliable knowledge. It is therefore a prerequisite to ask this question; "what exactly is the meaning of science?"

Etymologically, the encyclopedia Britannica (1964:144) traces the origin of 'science' from the latin word *scientia* which means knowledge. According to

Aristotle *Scientia* is the latin translation of the Greek word *episteme* which is understood as the true knowledge of nature achievable through demonstrative method and assessable through attainment of the causes (Prior & Posterior Analytics, 71b10-15).

Generally, the term 'science' is understood to explain and predict the world in which we live in. Though various religions also try to understand and explain the world, astrology and foretelling attempt to predict the future and historians attempt to understand the world, they are not considered as science (Okasha, 2002: 39). This raises more curiosity and tension in reference to the actual meaning of science. Nevertheless, the question on "what science is" normally seem to be so easy to answer especially when one thinks that subjects such as physics, chemistry and biology constitute science, while subjects such as art, music, and theology do not (ibid).

However, in order to have a better understanding of the meaning of science, we have to put into consideration the goal of science, the role of explanation in science, and then the role of philosophy in interrogating science.

2.2.1 The goal of science

According to Kosso (1992:13) it cannot be possible to answer the question 'what is science?' without looking at the goal of science. Basically, the goal of science is to describe the world and build up a simple system of principles from which facts can be observed. This system involves a model that describes, explains, summarizes and predicts nature. It further captures the explanatory power of science which satisfies the need to understand facts (Thompson, 2007:21). Apparently, the goal of science or the purpose of science is not only to describe the world, but also to understand the mechanism generating our observations. These mechanisms are the theories that scientists develop as an explanation to a certain phenomena.

Theories according to Kosso (1992:15) are answers to the "what is going on here?" question. They are an account of the underlying composition and the unseen causes of the world as we see it. They help science provide an account of what is going on behind the phenomena of what we experience. They do this by interrogating the why of things and by describing objects and events that are not apparent and making sense out of the happenings in the world that are apparent. Theories also aid science to understand the composition and the underlying causes of the manifest phenomena in order to make sense out of the phenomenal world we inhibit.

Therefore in this pursuit of understanding science which is motivated simply by an urge to satisfy curiosity, or to explain the world, we can thereby define science as an activity that interrogates phenomena or the world we live in through different ‘theories’. However, in the pursuit of trying to substantiate what science is through its role, we cannot fail to analyze explanation which constitutes one of the major tasks of science.

2.2.2 The role of explanation in science

In the definition we got earlier on ‘what science is’, we can deduce that knowing the ‘why’ of things involves providing explanations of why things are the way they are. Popper, Hempel, and Ernest Nagel (1901–85) all proposed an ideal of objective explanation and argued that explanation should be restored as one of the aims of the sciences. Today, science has an extra role; it is not only interested in questions of what will happen (prediction) ‘but also in the questions of why what has happened did happen’ (Bird, 1998:42).

On this view then, there are two important questions that come up: ‘what exactly is scientific explanation, and what exactly does it mean to say that a phenomenon can be explained by science? According Okasha (2002:40), scientific explanation is an answer to a why question that involves questions like; ‘why?’, ‘what caused it to happen?’ and the like. It involves the expressions of the scientist’s curiosity, and answers to them are expected to satisfy it.

Hempel (2001:69), in his covering law model for explanation argued that scientific explanations are usually given in response to what he called “explanation seeking why questions”, these are questions such as why is the earth not perfectly spherical?’, ‘why do women live longer than men?’ all these questions among others demand explanations.

The interesting point to note here is that in every aspect of life there is a need to infer, explain and derive conclusions on why things are the way they are and whether they are in conformity with the way they ought to be. This therefore triggers the question on whether modern science can explain everything or else explain a great deal of the world (Cornwell, 2004). This signifies that we cannot define, discuss or talk about science without doing some explanations.

According to Nola & Irzik (2005:77), explanations consist of two parts: e*xplanandum* and *explanans*. The *explanandum* is the phenomena to be explained. The *explanans* are further phenomena that do the job of explaining appealing to the phenomena. On this view then, the actual role of explanation in science is inevitable in its definition.

2.2.3 The relevance of Philosophy in understanding science

In order to grasp a better understanding of the actual nature of science, the kinds of knowledge that should be called science and what really qualifies knowledge as science, ‘philosophy’ is a tool that has to be considered. To Rosenberg (2005: 4), philosophy is a prior condition for understanding the history, sociology and other studies of science through its methods, achievements and prospects.

The philosopher therefore has the principle task of analyzing methods of inquiry used in various sciences. You may wonder why this task should fall to philosophers, rather than to the scientists themselves. This is a good question. Part of the answer to this is that looking at science from a philosophical perspective allows us to probe deeper- to uncover assumptions that are implicit in scientific practice, but which scientists do not explicitly discus (Okasha, 2002:12-13).

According to Benjamin (1937:21) due to this task by the philosophers, there emerged a new discipline called variously the "philosophy of science", "the science of science", "the philosophical science", "the logic of science", "the metaphysics of science" or the "metaphysical science". Klemke, Hollinger and Kline (1980:3) define this discipline as the attempt to understand the meaning, method, and logical structure of science by means of a logical and methodological analysis of the aims, methods, criteria, concepts, laws and theories of science.

Officially, the first methodological systematic, logical reflection on philosophy of science was in the 1920s with the Vienna circle. With the logical positivism, PS was seen as a fundamental part of the university studies and the logical positivism account of science provided an environment for reflection on science as an exemplary knowledge and source of information about the world (Makumba, 2005:75).

PS has different concerns, the first concern is on the methodology by which science arrives and posits truths concerning the world and then critically explores alleged rationales for these methods. Under this, it treats the acceptability of theories in science and the nature of the relation between evidence and hypothesis. The other concern is on the meaning and the content of scientific results. This concern is closely related to metaphysics and the philosophy of language and it looks at specific results in the sciences (Op cit: 76-77).

Despite all this, philosophers and scientists have not agreed on the nature of PS. According to Losee (1980:1-3), there are four view points on the nature of PS. They are:

a) PS as a formulation of the world-views that are consistent with and somehow based on important scientific theories.

b) PS as an exposition of the presumptions and predispositions of scientists; a view that tends to assimilate philosophy of science and sociology.

c) PS as a discipline in which concepts and theories of science are analyzed and clarified.

d) PS as a second order criteriology. In this, the philosopher of science inquires what distinguishes philosophy of science from other investigations; the procedures scientists follow in investigating

> the nature; the conditions required for the scientific explanation to be correct and the cognitive status of scientific laws and principles.

Losee favours the fourth perspective in which the analysis of scientific methodology is a second order discipline whose subject matter is the procedures and structures of various sciences (ibid). To this study, this perspective is also the best because it involves analyzing different theories by different philosophers of science.

Nevertheless, so far this study has been able to define the nature and meaning of science through its etymology, goals, role of explanation and its relation to philosophy. This acts therefore, as a stepping stone to interrogating Popper's reaction to inductivism and his theory of falsificationism.

2.3 Popper on the inductivist account of science.

This subsection looks at Karl Popper's reaction to the earlier account of science by the inductivists. The section begin by pointing out that though he was not a member of the Vienna circle, Popper had close links with it, he was never invited to attend the Vienna a circle's seminar, and was majorly characterized by Neurath as the circle's official opposition.

The problem of induction also known as Hume's problem involves the question on whether inductive inferences are justified, and if so under what conditions. Etymologically, induction comes from the Latin word *inductio,* Cicero's term to translate the Greek *epagogé,* of which Aristotle referred to as the passage from the particular to the universal. This was already validly criticized by Sextus Empiricus in his Outlines of Pyrrhonism. He argued that induction cannot justify the acceptance of universal statements as true (Gattei, 2009:45).

Inductivism is also defined as a point of view that emphasizes the importance to science of inductive arguments. In its most inclusive form, it is a thesis about both the context of discovery and the context of justification. With respect to the context of discovery, the inductivist position is that scientific inquiry is a matter of inductive generalization from the results of observations and experiments. With respect to the context of justification, the inductivist position is that a scientific law or theory is justified only if the evidence in its favour conforms to inductive schemata (Losee, 2001:133).

Inductivists think that inductive inference is justified by the principle of induction, but the main question is "how is the principle of induction itself justified?" If we try to justify it inductively by experience, we get into a circle, since any induction from experience depends on the principle of induction (Gilles, 1993:29). This is Popper's point of departure. He wanted to releave science from the embarrassment inflicted by the induction notion, an embarrassment intensified by logical positivism which considered induction as the hallmark of science.

According to Hume, "are we justified in reasoning from [repeated] instances of which we have experience to other instances [conclusions] of which we have no experience?" Hume's answer is in the negative: we are not justified in doing so, however great the number of repetitions. Popper tells a well-known example: the repeated observation of many white swans does not allow us to conclude that all swans are white, since the logical content of the conclusion exceeds by far any particular statement referring to a limited number of observations, however great (Gattei, 2009:45).

In reference to the empirical problem (or psychological problem, as Popper labels it) the question is: "Why, nevertheless, do all reasonable people expect, and believe, that instances of which they have no experience will conform to those of which they have experience?"The reason, Hume observed, is to be found in custom or habit: for we are conditioned by repetition and by the mechanism of the association of ideas—indeed, without it, we would not be able to survive.

Popper accepts Hume's negative opinion about the logical problem: induction is logically invalid and in no sense justified. But he rejects Hume's opinion about the psychological one, that is, that induction is a fact, and at any rate needed. His conclusion is clear-cut: "Induction simply does not exist, and the opposite view is a straight forward mistake (ibid).

Therefore, according to Popper (1963: 45-46) knowledge-particularly scientific knowledge-evolves from individual experience and cannot be verified through inductive reasoning. For the inductivist, science proceeds by first collecting observations or data and then inferring laws or predictions from this data by induction. We cannot simply observe without a theoretical background, in fact the belief that we can start by observation alone, without anything in the nature of a theory, is absurd because no one can ever observe and verify all possible evidence to prove a scientific hypothesis correct, it is necessary only to discover one observed exception to the hypothesis to prove it false.

To illustrate this point, he narrated a story on how he began a lecture to students of physics by instructing them to take a pencil and a paper, carefully "observe" and write down what they had observed. The students asked him what exactly he wanted them to observe, why? Because the instructions 'observe!' is absurd. This is indeed how Popper puts it:

> "Observation is always selective; it needs a chosen object, a definite task, an interest, a point of view, a problem. Its description presupposes a descriptive language with property words; it presupposes similarity and classification which in its turn presupposes interests, points of view and problems" (Popper 1963: 46)

Thus, according to Popper, any attempt to justify principle of induction inductively by experience, gets into a vicious circle or an infinite regress. The method of science for him is the method of bold conjectures and ingenious severe attempts to refute them. His methodology however was as a result of the impact of the problem of induction, which we have analysed and the problem of demarcation which we will interrogate next before evaluating his theory of falsificationism.

To the problem of induction, he concluded that therefore there should be non-inductive theory of scientific method. This is what he calls the theory of falsificationism or the method of conjectures and refutations.

2.4 The normative problem of demarcation

The problem of demarcation is the "the problem of finding a criterion which would enable us to distinguish between the empirical sciences on the one hand, and mathematics and logic as well as 'metaphysical' systems on the other hand." It is also what Popper names as "Kant's problem" (Gattei, 2009:45-46).

Popper in his normative problem of demarcation is not only trying to demarcate science from pseudo-science, but also making an important evaluational claim- a ranking of one as better than the other. His demarcation criterion however is meant to indicate scientific progress and improvement and consequently establish science as rational in ways that a pseudo-science is not (Grim, 1982:128-132). Ideally, an adequate criterion of demarcation ought to prescriptively guide scientific behavior rather than describe it and also sharpen the understanding of what is rational. Therefore, the task Popper sets is not merely to identify criteria of demarcation, but to justify their evaluative force.

In doing so, he stated that a scientific statement has to comply with the following requirements (Popper, 1959:17-32):

a) It should be logically consistent and not be a tautology; because a tautology does not add knowledge although it might give the impression to do so.

b) It should be 'inter-subjectively' testable; 'a scientifically significant physical effect may be defined as that which can be regularly reproduced by anyone who carries out the appropriate experiment in the way prescribed'.

c) It should be possible to falsify the statement; this requirement should be understood in a methodological way, this means even when it is not possible to falsify the statement yet, it should in principle be possible.

Anyone who makes a scientific statement should indicate by which empirical evidence he would withdraw his statement. This criterion is Popper's most important one and the one he uses to distinguish his approach from the logical positivists. It also leads to a solution of Hume’s problem of induction; of the problem of the validity of natural laws. The root of this problem is the apparent contradiction between what may be called ‘the fundamental thesis of empiricism’; and Hume’s realization of the inadmissibility of inductive arguments (Popper, 1959:47-49).

This contradiction arises only if it is assumed that all empirical scientific statements must be ‘conclusively decidable’, i.e. that their verification and their falsification must both in principle be possible. If we renounce this requirement and admit as empirical also statements which are decidable in one sense only—unilaterally decidable and, more especially, falsifiable—and which may be tested by systematic attempts to falsify them, the contradiction disappears: the method of falsification presupposes no inductive inference, but only the tautological transformations of deductive logic whose validity is not in dispute (ibid).

Therefore, it is clear that Popper’s account of demarcation is normative in character and hedges centrally on his falsifiability criterion, a theory that has been widely criticised by Kuhn and others precisely for stressing this feature and it paves way for the next discussion on how Popper exemplifiers this theory of falsificasionism.

2.5 Popper’s theory of falsificationism

As pointed out earlier, Popper is among the most forceful advocates of an alternative to inductivism. He was uncompromising in rejecting the view that the method of science is inductive in any way. He proposed falsificationism which is an alternative idea that we learn from experience by refutations so that the empirical character of scientific theories is shown by their clashes with experience (and if they are false by their refutability) (Chalmers, 2013:55-57).

Falsificationism requires that a theory be ranked as scientific if it runs the risk of being refuted (Popper, 1959:265). A theory should be framed in such a way that there are some conceivable or possible results of observation or experiment which if reached, would falsify a theory. Therefore, there should be severe testing of theories all attempting to falsify the theory in question. If a theory survives those severe and critical tests, it stands a better chance to face further tests, but it can never be established on the basis of its survival.

When a theory becomes more fit to thrive (tentatively) awaiting further and more critical tests, that theory is said to be corroborated. The assessment of corroboration establishes relations such as the compatibility or incompatibility of the theory with one of its potential falsifiers. If incompatibility amounts to falsification, and therefore the decision to accept it implies the decision to regard the theory as falsified, compatibility alone must not make us attribute to the theory a positive degree of corroboration: the mere fact that a theory has not yet been falsified can obviously not be regarded as sufficient (ibid).

What then determines the degree of corroboration is not so much the number of corroborating instances, "as the severity of the various tests to which the hypothesis in question can be, and has been, subjected."As a consequence, "the corroborability of a theory—and also the degree of corroboration of a theory which has in fact passed severe tests, stand both, as it were, in inverse ratio to its logical probability"; for they both increase with its degree of testability and simplicity (Popper, 1959:267). The ratio negativa of the criterion of falsifiability finds its complement in the idea of corroboration, in the positive support provided by experience to theories. Indeed, corroboration grows out of the very problems of induction and demarcation.

Nevertheless, for Popper, although observation and deductive logic cannot establish the truth of scientific generalization, they can establish their falsity or refutation. He rejected as "pseudoscience" any system of beliefs that could not pass this "falsifiability criterion" and that relied on predetermined "laws" of human behaviour. He argued that, from the observation that ‘this swan is black’; we can infer by deductive logic that the generalization that ‘all swans are white’ is false. This however culminates to Popper’s conjectures and refutations or what he calls the falsificationist account of scientific method.

It is clear that this account is normative in character because Popper strictly argues that science does not start from observations as the inductivists claim, but starts with conjectures where the scientist tries to refute, criticize and test theories. A conjecture which has withstood a number of severe tests may be tentatively accepted, ‘but only tentatively’. Further, Popper argues, we can never know a scientific theory, law or generalization with certainty, because it may break down in the very next test or observation (Popper, 1959:266).

These elements of his methodology are indicators that his is a theory that dictates how science ought to be practiced and the norms and standards that should guide scientific progress. Popper repeatedly declares his methodology and approach in philosophy of science as normative in character because his account of science, ‘all we need is to replace the “action agent” with “the product of scientific agent”, be it a hypothesis, an argument, a design of an experiment whatever’.

2.6 Conclusion

From the discussions above, it is clear that the approach in Popper’s philosophy of science is an antidote to the earlier accounts of science by the logical positivists and the inductivists. It is also clear that a normative philosophy of science involves more about what scientists ought to do and less about what scientists do (the features of normative philosophy of science). In Popper’s theory of falsificationism, it is discovered that Popper dictates what critical rationalists methodology would require of scientists. This approach therefore is normative in character and falls under the ‘ought’ side of the ‘is-ought’ dichotomy.

It can therefore be concluded that since Popper’s approach has normative features, his is a normative account of science. However, this does not mean that his account is perfect and without any shortcomings. In reaction to Popper’s approach, Kuhn thinks otherwise and in this sense disregards Popperian efforts. In the next chapter, this study looks at how Kuhn differs with Popper as he takes the ‘is’ part of the ‘is-ought’ dichotomy through his theory of scientific revolutions.

CHAPTER THREE

KUHNIAN DESCRIPTIVE ACCOUNT OF SCIENCE

3.1 Introduction

The aim of this chapter is to examine the descriptive account of science through Kuhn's theory of scientific revolutions. In order to achieve this goal successfully, this chapter substantiates the nature of descriptive account of science, the nature of a historical approach to science and then the features in Kuhn's account that make his theory descriptive.

3.2 The nature of a descriptive account of science

Generally, the verb 'describe' signify the act of giving details about something or giving an account of something with enough clarity and details in order to be understood by somebody else. It also means to give an account of something by giving details of its characteristics.

Benjamin (1937:8), in a comparison he made between the description and explanation, argues that description is an endeavor to symbolize the obvious features of events, whereas explanation tries to represent the less obvious features. For him, since knowledge of the less obvious features is presumably based on the knowledge of the more obvious, description must precede explanation.

The task of science therefore is to describe and not to explain since no one would maintain that the task of science is merely to explain and not to describe, in fact one may conclude that the very minimum which science can do is to symbolize the obvious features of events. Hence, science is at least descriptive which means it tells something factual about the world or rather about what is the case in science (ibid).

To be more specific, a descriptive assessment of science contrary to a normative account (which is philosophical), is historical and looks at how things are or how science is, rather than looking at how it ought to be. It aims to discover and display the repeated patterns that are accepted by the practitioners of science as constituting progress (Losee, 2004).

The descriptive philosophy of science therefore comes in as a reaction to the normative philosophy of science. It is based on the assertion that "historical approach is more relevant to the development of a more comprehensible understanding of how science works than the philosophical approach".

3.3 The historical approach to science

Critics with a historical orientation mounted a formidable attack on the standard view of science. They argued that the normative or the standard view of science fails to include in its conclusions historical data drawn from the history of science (Perumalil, 2006: 56). Philosophers therefore are required to study the history of philosophy and science in order to understand the very concept of science and philosophy of science.

Auguste Compte,William Whewell, Pierre Duhem, and Earnst Mach were among the pioneering scholars in the historical approach. They were most notable for propounding comprehensive theories of science that are rightly regarded as classic. Their accounts depended crucially on the record of how science developed over time. Their aims were more comprehensive and their purpose was to construct an accurate and an all embracing theory of science which wove analysis of the logic of science into their narratives of its history.

Later, this tradition was carried forward with vigor by George Sarton, Emile Meyerson, Ludwig Fleek, Helene Metzger, J.B Conant, Alexander Koyre, Gerald Holton, I.B Cohen and Thomas Kuhn who addressed western science as a whole and proposed new ways of thinking about how science functions and changes. Their interests influenced their selection of topics and their interpretations attempted to clarify the comprehensive enterprise called science (Laudan, 1977).

According to Scillip (1974), the most noble and important contributions about the nature of the history of science are Thomas Kuhn's *structure of scientific revolutions (1962)* and Joseph Aggassi's *'towards a historiography of science'* (1963). Kuhn's structure of scientific revolutions (1962) which is of our interest here is a book that was one of the most influential and discussed, quoted and misquoted in the 19th century. It influenced directly and indirectly a wide range of disciplines, fields of inquiry and even areas of popular culture.

When Kuhn began his carrier as a Historian of science, he found that his preconceptions about the nature of science were shattered. He came to realize that traditional accounts, whether the inductivist or falsificationist, did not bear comparison with historical evidence (Chalmers 1981:91-92).

He therefore, developed a theory of science that was more in keeping with the historical situations as he saw it. A key feature of his theory was its emphasis on the revolutionary character of scientific progress, where according to him, a revolution involves the abandonment of one theoretical structure and its replacement by another. This approach made Kuhn one of the architects of the historical turn of the 1960s (Chalmers 1981:92).

In the next topic this study interrogates how Kuhn exemplifies his account of science in his work the *"structure of scientific revolutions"*.

3.4 Kuhn's account of science

Kuhn's picture of science can be summarised in an open ended scheme that continues in a sort of spiral. This scheme comprises of; Paradigm, prescience, normal science, crisis and revolution in science and then paradigm shift.

3.4.1 Kuhn's notion of paradigm

Etymologically the word paradigm is from two Greek words "para" and "deigma" meaning "pattern". It reflects the structure upon which a concept is based. It creates a framework through which we view the world and a way to give our observations some meaning. According to Kuhn, both the logical positivism and Popper's falsificationism missed an essential component of the "paradigm" in their analysis of the structure and workings of science. This component is what he named “paradigm”, a notion that revolutionized philosophy of science (Hung, 1997:357).

In Kuhn’s view a ‘paradigm’ is an exemplary model of successful science; for example the Ptolemy’s geometric astronomy, Newton’s physics, the electromagnetic theory of light or Darwin’s theory of evolution. A paradigm dominates a field for a time and provides scientists in that field with a theoretical framework, a set of assumptions, an orientation toward particular sorts of problems, and rules for how these problems should be approached and proposed solutions appraised (Chalmers, 1976:90).

What characterizes a paradigm is the fact that the relevant authorities do not discuss whether or not this structure behind the concept is valid but they rather simply accept it. Scientific research and thought is defined by “paradigms,” or “conceptual world-views” that consist of formal theories, classic experiments, and trusted methods. Scientists typically accept a prevailing paradigm and try to extend its scope by refining theories, explaining puzzling data, and establishing more precise measures of standards and phenomena (ibid).

Paradigms should therefore include standard ways of applying the fundamental laws to a variety of situations and the instrumentation (instrumental techniques) necessary for bringing the laws of the paradigm to bear on the real world. They also consist of some general metaphysical principles that guide the activities within a paradigm (Chalmers, 1982: 91).

Additionally, paradigms determine at a subconscious level, what information (we accept as well as that which we ignore), “the kinds of questions we ask, and how we look for answers”. Therefore, all paradigms contain some very general methodological prescriptions such as, “make serious attempts to match your paradigm with nature” or “treat failures in attempts to match a paradigm with nature as serious problems”.

After understanding Kuhn’s notion of a Paradigm the next question is on how we come to settle into a Paradigm.

3.4.2 Prescience

According to Kuhn, ‘Prescience’ is the term that describes the situation that one finds in a theoretical field before a single paradigm has been widely accepted. It is a period when there are many schools of thought around. Each scientist or a small group of scientists has his or her own metaphysics, ontology, theory and point of view about the correct way of practicing science (Chalmers: 1976: 90-92).

In this situation, scientists disagree over what phenomena need to be studied or explained, what method they should use, and what observations are relevant. As they argue, talk and compete, they tend to make different observations, perform different kinds of experiments and place different interpretations and descriptions same area of study (ibid).

Normally, during this period, there is not only a little progress, but also lack of consensus with no single set of theoretical premises that they can take for granted as the foundation from which to work. Eventually, one of the schools, because of its success, triumphs over the others. It becomes accepted as a paradigm and as a result, Kuhn argues that science progresses at a steady pace (ibid).

According to Kuhn (1996: 23) the transition from prescience to science typically occurs when one school of thought triumphs over its rivals and thereby establishes itself as a paradigm. From then on, there is a general agreement about which phenomena are worth studying, which problems need to be addressed, what methods should be employed and how findings should be presented. This is what we will analyse in the next subtopic.

3.4.3 Normal science

At this stage, we acknowledge that mature science is governed by a single paradigm which according to Kuhn sets the standards for legitimate work within the science it governs. It coordinates and directs the puzzle-solving activity of the groups of the normal scientists that work within it.

The existence of a paradigm capable of supporting normal science tradition is the characteristic that distinguishes science from non-science. Thus, Newtonian mechanics, wave optics, and classic electromagnetism all constituted and perhaps constitute paradigms and qualify as science. Much of the modern sociology lacks a paradigm and consequently fails to qualify as science (Kuhn, 1996: 24-26).

Within science proper, when science progresses at a steady pace, Kuhn distinguishes two kinds of activities, which are, normal science and revolutionary science. Here we are going to analyse the nature of normal science and in the next paragraph we look at what entails a crisis and revolutionary science. According to Kuhn, the normal science is a tightly disciplined activity in which most scientists are engaged most of the time. It consists for the most part, in working out the limits and implications of the dominant paradigm of a given field (ibid).

In this period, scientific knowledge is collected and accumulated through study and research under the guidance and constrain of a paradigm. For Hung (1997)

> "It is a period of accumulation of knowledge as if the scientist-workers can now proceed to build the cathedral of science according to the architectural plan (paradigm)" Hung (1997; 376).

Normal science however, involves a detailed attempt to articulate a paradigm with the aim of improving the match between it and nature. In fact Kuhn portrays normal science as a puzzle solving activity governed by the rules of a paradigm. These puzzles are of both a theoretical and experimental nature (Chalmers, 1982: 92).

Normal scientists must presuppose that a paradigm provides the means for the solution of the puzzles posed within it. A failure to solve a puzzle is seen as a failure of the scientist rather than as an inadequacy of the paradigm. For Kuhn, puzzles that resist solutions are seen as anomalies rather than falsifications of a paradigm. However, all paradigms will contain some anomalies and rejects all brands of falsificationism.

3.4.4 Anomalies, crisis and revolution in science

In the period of normal science, scientists occasionally encounter difficulties (or anomalies) that cause brief delays. When more difficulties accumulate, a sense of crisis emerges. In other words, this period involves a time for responding to problems thrown up by normal science, problems that lead to a theoretical crisis within a given period.

According to Kuhn (1996: 66ff), the mere existence of unresolved puzzles within a paradigm does not constitute crisis. In fact paradigms will always encounter difficulties (or anomalies) and it is only under special sets of conditions that the anomalies develop to undermine the confidence of the paradigm. An anomaly is seen as a serious threat if it is seen striking at the very fundamentals of a paradigm and yet persistently resisting any attempts by the members of the normal scientific community to remove it.

For Kuhn, an analysis of the characteristics of a crisis period in science demands the competence of the psychologist as much as that of a historian. When anomalies come to be seen as posing serious problems to the paradigm, normal scientists begin to engage in philosophical and metaphysical disputes and try to defend their innovations. Once they are not able to defend their paradigm, it becomes weakened and undermined. The seriousness of the crisis deepens when the rival paradigm makes its appearance (Chalmers, 1982: 95).

Eventually, a different paradigm or architectural plan is sought and finally the old one is replaced. According to Kuhn (1996: 96ff), the new paradigm is so different from the old one because the half completed building has come down and work according to the new plan has to start afresh. Therefore, when the new science under the replacing paradigm cannot be built on the existing edifice, science starts all over again from the ground level. This for Kuhn is a scientific revolution in which there is a Paradigm shift (Hung, 1977: 377).

The history of science, according to Kuhn, consists of periodic revolutions or what he calls paradigm shifts which are separated by longer periods of 'normal scientific activity' (Horner & Westacott, 2000:114).

Under the pressure of increasing criticisms, Kuhn declared that he was dissatisfied with the term paradigm and suggested the term disciplinary matrix as a substitute. He argued that this phrase captured two essential meanings of the original term 'paradigm' which are a set of agreed upon problems and solutions that constituted scientific training and a set of preferred values and methods that constitute everyday every scientific practices in "normal science". A disciplinary matrix however provided the warrants for the method of normal science and was charactarised by what scientists did most of the time. Occasionally, paradigms or the so called disciplinary matrices altered dramatically bringing about periods of revolutionary change (Enos, 2009:491).

3.5 The descriptive nature of Kuhn's account

To Psilos & Curd (2008) Kuhn's view of how scientific theory progresses (i.e. oscillates between periods of normal science - a routine, cumulative, puzzle solving work involving experimentation within a paradigm, and a paradigm shift or a scientific revolution that involves a critical situation where an irresolvable tension within normal science results in a change of a paradigm) is strictly descriptive in character.

In their view (Psilos & Curd, 2008), Kuhn in 'the structure of scientific revolutions (1962, 1970, 1996, 2012), which may also be called the "the theoretical history", argued that natural science has two elements which are the descriptive and the explanatory element.

On one hand, descriptive element identifies a general pattern in the development of science whereby science involves a puzzle-solving enterprise which shows a cyclical pattern of normal science, crisis, revolution and new normal science. On the other hand, the explanatory element proposes an explanation of the pattern identified in puzzle-solving in the process driven by adherence to a paradigm (an exemplary puzzle solution) (Psilos et. al, 2008).

In other words, according to Sathis (2007), Kuhn's theory of science should be seen as the outcome of two inputs:

> A reflection on the actual scientific practice as well as the actual historical development and succession of scientific theories; and a reaction to what was perceived to be the dominant logical empiricist and Popperian images of scientific

> growth: a progressive and cumulative process that is governed by specific rules as to how evidence relates to theory Sathis, (2007: 132).

These elements in his approach are incompatible with the Popperian concept of scientific progress an account that is (as demonstrated in the previous chapter) more of how science ought to progress. Nevertheless, it is beyond reasonable doubt that the way Kuhnian methodology operates marks a sharp break from both the positivism's and Popper's account of science and has descriptive features.

We can therefore, conclude that Kuhn is presenting a descriptive account of science because his methodology strictly deals with the actual work of the scientists and all these aspects of Kuhn's writings bring the impression that his account is purely a descriptive one, that is, because he aims at describing scientific theories or paradigms and the activity of scientists (Chalmers: 1976: 98).

Consequently, in courtesy of Kuhn, descriptive terms in his account such as paradigm, paradigm shift, incommensurability, and normal science have permeated nearly every realms and mostly in academic journals and books.

3.6 Conclusion

The goal of this chapter has been to examine the nature of the descriptive account of science through Kuhn's theory of scientific revolutions. It has been determined that a descriptive account looks at more of the actual practice of science is and less of the way science ought to be practiced. Kuhn's approach conforms to these features and as a result it can be concluded that his is a descriptive account of science. This therefore insinuates that Kuhn’s philosophy of science is in the ‘is’ part of the ‘is-ought’ dichotomy and it is an account that differs the previous accounts of science which are inductivism and deductivism (falsificationism).

If Kuhn’s account of science in his theory of scientific revolutions is descriptive in nature, it therefore can be re-branded as Kuhnian descriptive account of science, a contrary account to the Popperian normative account of science elaborated in the previous chapter. In the next chapter, this study interrogates how Lakatos attempts to harmonise these two different accounts of science through his methodology of scientific research programmes.

CHAPTER FOUR

IMRE LAKATOS' NORMATIVE-DESCRIPTIVE ACCOUNT OF SCIENCE

4.1 Introduction

The goal of this chapter is to evaluate Imre Lakatos' methodology of scientific research programmes (LMSRP) with an aim of determining its success in resolving the controversy between normative and descriptive accounts of science. In order to achieve this goal, this chapter begins by analysing the argument between the normativist and the descriptivist philosopher of science and the nature of the philosophical-historical account of science as a stepping stone to interrogating and evaluating the LMSRP. In the actual task of giving an evaluation of the LMSRP, this chapter identifies the strengths and the weaknesses of Lakatos methodology. The former signifies the success and the latter signifies the failure of his attempt in reconciling the normative and descriptive accounts of science.

To put the objective of this chapter differently, Losee (2001; 202) points out that the rational reconstruction of scientific progress was among the most debated issues in the 1960s. It focused mostly on the conflict between Popper and Kuhn who provided the basic texts with conflicting accounts of science. As a result, there followed a period of exposition, comparison and attempted reconciliation with the contribution of Lakatos being among the most attractive attempt to harmonise the normative and descriptive accounts of science.

In the debate, Popper exemplified a philosophical methodology which is normative in character (as seen in chapter 2) and Kuhn on the contrary, exemplified a historical methodology which is descriptive in character (as as seen in chapter 3). Lakatos in reaction to the controversy in the two approache, attempted to formulate a methodology that is both philosophical and historical based on his maxim: "history of science without philosophy of science is blind; philosophy of science without history of science is empty" (Lakatos 1970: 91).

Before we go into details on how he attempted to resolve this controversy, it is sensible to start by highlighting once the actual argument between the normative and the descriptive account and then look at the nature of the so-called 'philosophical-historical' account of science. A rigorous elaboration of these two areas will enable us to have a clearer picture of Lakatos' account of science.

4.2 The argument between the normative and the descriptive account

From the point of view of the normative philosophy of science (the inductivism and deductivism or falsificationism), descriptivism and especially Kuhn's descriptive account of science seen in his normal science is a bad practice because of its quasi dogmatic element in its operation. This is because in the normal science, you do not question the basic rules at all and they are not tested, instead, you simply apply them. To the normativists, this is bad science because according to them, as we saw in chapter 2 (Poppers' falsificationism), theories should be continuously tested in order to possibly falsify them (Popper, 1980: 80- 81).

In defense of the descriptive account of science, Kuhnian paradigm theory shows that this approach serves vital functions for the goal of science which is production of knowledge. Thus, the descriptive philosophy of science is a reasonable practice and the normative philosophy is wrong in this respect (Kuhn, 1970: 182-187). More interestingly, he argued, the normal science provides the principle which was missing in falsificationism: the principle for acceptance of hypothesis that allows halting tests. In this way emerging difficulties are not immediately taken as indicators of its failure in principle but as exposing the incapability of the respective scientists. To the descriptivist account of science therefore, the normative philosophy is unreasonable because of the continuous tests.

In this altercation, both the normativist led by Popper and the descriptivist led by Kuhn thought that they got the scientific method right. This therefore brings about the question of who was right and who was wrong? According to Lakatos, there is a possibility of having a model that captures the two diverse approaches together without contradiction thus having a picture of science that is both normative (philosophical) and descriptive (historical).

4.3 The nature of a philosophical-historical account of science

Basically, both History of science (HS) and Philosophy of science (PS) belong to the sciences of the culture; they both study the cultural phenomena called science. In general terms both disciplines have the same subject matter i.e. the bulk of science and they coincide in their subjects namely; particular concepts of science like principles, methods and programmes (Perumalil, 2006: 56).

According to Hung (1997: 422), PS is an apriori study. It is an investigation of how science should be done and reason alone yields the correct methodology of science. HS on the contrary is an aposteriori study. It is an empirical study and it attempts to describe how past scientists actually worked, and how science actually developed.

Hitherto, we have seen into details their difference though the accounts exemplified by Popper the philosopher of science and Kuhn the historian of science. These differences made sense because any pair of disciplines that deal with the same subject essentially have their differences. However, what is of more importance here is not their differences, but their tendency to be interdependent. We are therefore going to look at the interdependence of historical and philosophical accounts of science through Lakatos methodology of scientific research programmes. Here interdependence means PS take HS seriously and HS take into consideration logical methodological questions (Perumalil, 2006: 56-57).

To expound further, a philosophical- historical account of science, is an account that puts together the ‘ought’ (the normative) and the ‘is’ (the descriptive). It is based on the argument that looking at history of science and philosophy of science together will enable us to have a more successful philosophical assessment of science, philosophy of science and history of science. This is the perspective that Lakatos subscribes to.

For Lakatos, philosophy of science should learn from historiography of science and historiography of science should learn from philosophy of science so as to have a proper picture of how science operates (Lakatos: 1970: 92-95). He developed this argument through his methodology of scientific research programmes, a methodology that comprises of progressive and degenerative research programmes, which is constituted of the hard core (HD), the protective belt (PB) and then the negative heuristic (NH) and the positive heuristic(PH).

4.4 The Scientific Research Programmes

Generally, a research programme is defined as ‘a professional network of scientists conducting a basic research’. For Lakatos, it has a very particular meaning in his philosophical image of the growth of scientific knowledge. He customarily uses the British spelling "programme" to distinguish how he uses it in his philosophy of science from its ordinary use.

He further defines it as a ‘sequence of theories or components of large cognitive units within a domain of scientific inquiry whereby each later, or successor, theory, is held to mark an advance over its predecessor (Lakatos, 1978:47).

4.4.1 Research programmes’ ‘hard core’ and ‘protective belt’

A research programme’s “hard core” is a set of assumptions about the world that are constitutive of the programme and cannot be surrendered without giving it out all together. It is rendered unfalsifiable or immune to refutation by the methodological decision of its protagonists. Any misgiving in the match between an articulated research programme and observational data is to be attributed, not to the assumptions that constitute the hard core, but to some other part of the theoretical structure (Chalmers, 1982; 81).

The maze of assumptions that constitute the other part of the structure is what Lakatos refers to as the protective belt (PB). It is a set of further claims of the theory which surrounds the hardcore and functions as auxiliary hypotheses. On one hand, theories are needed to apply the components of the hardcore to explanation and prediction, and on the other hand, they may be changed to avoid treating components (Lakatos 1978: 49). However, for Lakatos, what is of more importance is the fact that the protective belt consists not only of explicit auxiliary hypothesis supplementing the hard core but also assumptions underlying the description of the initial conditions and also observation statements.

He further argued that a research programme is a structure that provides guidance for the future research in both positive and negative ways. He calls these the positive and the negative heuristic.

4.4.2 Negative and positive heuristic

Lakatos (1978: 48) made free use of the term 'heuristic' in charactarising research programmes. For him, a heuristic is a set of rules and hints to aid discovery or invention. In an example, Chalmers (1999) notes that a heuristic for solving crossword puzzles might be “start with the clues requiring short word answers and then proceed to those requiring long word answers”.

According to Lakatos, the guidelines for work within a research programme should therefore be divided into Negative Heuristic (NH) and the Positive Heuristic (PH). The NH specifies what the scientist is advised not to do. Here, scientists are advised not to tinker with the HC of a programme in which they work. However, if a scientist does not modify the HC, then he or she has, in effect, opted out of a specific problem.

The PH of a programme constitutes that which specifies what scientists should do rather than what they should not do within a research programme. With this task, it is more difficult to characterize specifically compared to the NH. The PH however, gives guidance on how the HC is to be supplemented and how the resulting protective belt is to be modified in order for a program to yield explanations and predictions of observable phenomena (Lakatos, 1978: 48-49).

Eventually, Lakatos illustrated the notion of the PH with the story of Newton’s early development of his gravitational theory. Here, the PH involved the idea that one should start with simple, idealized cases and then, having mastered them, one should proceed to more complicated and more realistic cases. Thus, the PH should be sufficiently coherent to be able to guide the future research and determine whether a current research programme is progressive or degenerative.

4.4.3 Progressive and degenerative research programme

The deciding question here is whether the research programme (RP) is progressive or degenerative. According to Lakatos, a RP is said to be progressive or degenerative depending on whether they succeed in leading or whether they persistently fail to lead to the discovery of novel phenomena. For him, the basic methodological rule is "give up a degenerative research programme (DRP) in favor of a progressive research programme (PRP) (Chalmers: 1976: 80).

A DRP, according to Lakatos, is characterized by theory development that occurs primarily in response to empirical difficulties; one finds new alternatives exceptions or reinterprets results in order to explain why the original theory no longer agrees with the observations. It does contradict new data and makes no or (hardly any) new predictions and predictions (ibid).

A PRP is characterized by theory development that pre- empts experimental results. Theories in a RP have inherent possibilities for further development which enable researchers take advantage of prior experiments taking place to test these possibilities. Unlike the DRP, it does not contradict any new data and makes new predictions and new explanations (ibid).

According to Lakatos, one cannot reasonably say that he should give up a research progamme at the first sight of difficulty. That would be too hasty. For him therefore, the methodological rule of throwing out a DRP should be supplemented with the regulation that one should give a RP that has come into a degenerative phase a certain amount of time to regain its force and once again become progressive (Curie, Kieran, Meskin & Robinson, 2014).

In other words, if a DRP contradicts new data, this does not falsify the whole RP. This means there are many ways of modifying a RP in such a way that the contradiction disappears. These modifications involve adding to the ‘core’ of a RP extra ad hoc assumptions that serve only one purpose; ‘to explain away the contradiction’. These extra assumptions as we mentioned, constitute the PB of a DRP. However, the more new predictions and explanations a RP provides, the more likely it is progressive (ibid).

4.4.4 Problem shift

In Lakatosian language, a RP is essentially a sequence of theories within a domain of scientific inquiry whereby each later, or successor, theory, is held to mark an advance over its predecessor. The move from one theory to another within a RP is called a problem shift. Scientific knowledge progresses over time when asking the question ‘when is a problem shift progressive?’

In addition, Lakatos’ methodology of research programmes can also be evaluated in terms of progressive and degenerative problem shifts. It is progressive or degenerating depending on whether it succeeds in leading or whether it persistently fails to lead to the discovery of novel phenomena (Howson, 1976). Lakatos further argued that it is often worth being loyal to a DRP for some time (as it may manage to recover) though if there is an alternative PRP on the horizon, the rational thing to do is to jump ship and join the progressive one (Lakatos, 1970:95).

After pointing out how a research progamme operates the next question in line therefore is; “does the Lakatosian methodology of scientific research programme reconcile the Popperian normative and the Kuhnian descriptive accounts of science? If it does, how?

4.5 The evaluation of Lakatos’ attempt to reconcile Popperian normative and Kuhnian descriptive accounts of science

To begin with, in order to understand and evaluate how Lakatos’ account of science harmonizes normative and descriptive accounts of science, it is important to understand how his approach takes the form of Hegelian dialectic. A dialectical method is basically a discourse or a debate between two or more people holding different points of view about a subject but wishing to establish the truth through reasoned arguments. This is the case we have seen in the debate between the normativists and the descriptivists. According to Miller (1977), the Hegelian triad operates in a three way movement that involves a thesis, antithesis and synthesis.

Psillos & Curd (2008) note that, in order to have a clear picture of Lakatos’ rational reconstruction of history, it is important to put into consideration Hegel’s view. Hegel argued that history has an underlying logic, an idea or a thesis, which may govern some historical epoch, then, has within itself certain contradictions (internal tensions) which in due course, give rise to an opposing idea, the antithesis. The creative friction between the thesis and the antithesis brings about a third idea, the synthesis which is a resolution of that struggle. This is the same case in this study because Popper provides the thesis, Kuhn provides an opposing idea, the antithesis and Lakatos provides a harmonized solution, the synthesis.

In the last two chapters, we saw Popper offering a thesis (chapter 2) and Kuhn offering an antithesis (chapter 3). In this chapter Lakatos introduces as a synthesis the notion of "research programmes" through which he shifted ground from the static relationship between facts and theories to the dynamic nature of scientific practice.

Lakatos argued against Popper's view that scientific truth changes when theories are falsified (falsificationism) and Kuhn's thought that theories were not falsified so much as overthrown (paradigm shift). Instead he made scientific practice, rather than beliefs about the truth of theories, his subject (Zack: 2010:375). For him, the history of science operates in terms of competing research programmes which unlike Kuhn's paradigms, can coexist. Building on Duhem's notion that hypotheses cannot be refuted, he argued that there is no such a thing as a sudden paradigm shift and there is no such a thing as absolute refutation (Rosenberg, 2005; 163).

Additionally, Lakatos argued that, instead of theories being summarily rejected at the first conflict with observation, science in his view is seen to proceed by continually adjusting and developing the protective belt around the hard core of a research programme; this is a systematic process that forms part of normal science whereby the notion of paradigm is replaced with the notion of research programmes complete with hard core and auxiliary belt.

In this way, he offers a picture of science that preserves progress and rationality while it avoids Popper's naive falsificationism and respects the actual history of radical conceptual change in science (Chalmers, 1976: 80-81). This means therefore that Lakatos' account of science has normative as well as descriptive features or in other words it is a synthesis of the normative and the descriptive picture of science.

If this is the case, the next question is triggered; "is Lakatos successful or not in reconciling normative and descriptive account of science?" or else does his model succeed or fail in blending normative and descriptive accounts of science?

The answer to this question is not straight forward because there are no controlled experiments that can be done in order to determine whether Lakatos' account is successful or not in reconciling the normative and the descriptive accounts of science. Nevertheless, after, analysis, argumentation and examination of the normative account through Popper's theory of falsificationism, and the descriptive account through Kuhn's theory of scientific revolution, it turns out that Lakatos' account is neither normatively and descriptively inadequate nor is it normatively and descriptively adequate; it is either normatively adequate and descriptively inadequate or normatively inadequate and descriptively adequate.

Normatively and descriptively inadequate means that Lakatos' framework is totally inappropriate i.e. the picture of science he gives does not at all capture any normative (philosophical) or descriptive (historical) features. This definitely is not the case. Normatively and descriptively adequate means that Lakatos' model is appropriate and perfectly reconciles the normative (ought) and descriptive (is). This, yet again is not the case because the 'ought' (philosophical) and the 'is' (historical) are two levels of reality that cannot be fully reconciled or put together perfectly at the same time.

According to this study therefore, Lakatos' account is either normatively adequate and descriptively inadequate or normatively inadequate and descriptively adequate. This means that his framework can either be strong in setting standards and norms on how science should be and weak in explaining and accounting for the actual practice of science; or weak in stipulating how science ought to be done and strong in explaining and giving an account of the actual practice of science. Therefore, his methodology can be characterized as having strengths and weaknesses which act as a gauge in determining the extent in which he succeeds and the extent in which he fails in reconciling normative and descriptive accounts of science. The strengths signify his success and the weaknesses signify his failure.

In a nutshell, the tension between the normative and the descriptive philosophies of science as elaborated previously, is simply in the disagreement whereby the normativist account (inductivism and deductivism- Popperian falsificationism) argue for setting norms on how good science should be practiced and the descriptivist account (Kuhnian paradigm theory) argue for giving a generalized description of how science develops. This in other words means there is a discrepancy or a tension between theory and practice in science (the is-ought dichotomy').

In this tension, Kuhn in his antithesis did not want to talk about theories like the verificationists or the falsificationists (Popper) who thought that theories should be subjected to an experimental situation whereby the experiment serves either to confirm incase of verificationism and undermine or refute incase of falsificationism. Instead, he coined the word paradigm to mean a whole way of looking at things or a whole conception of a discipline.

In resolving this tension, Lakatos thought that Kuhn did not go very far, his idea of a paradigm still means the same as Poppers idea of a theory. For him, instead of a paradigm, he chose the term research programme (RP) (Lakatos; 1978). He was of the view that Kuhn's idea of a Paradigm, just like Popper's theory of falsificationism, portray a picture of science that appear that like something static, something that can be fixed in a period of time, one that we should hold onto come what may against any encounter of contradiction in reality.

Lakatos in trying to resolve the tension through the methodology of scientific research programmes, argued that there are elements in a RP that should be held onto come what may (the hard core elaborated earlier) and there are elements that should be somehow flexible, revisable and dynamic (the auxiliary hypotheses elaborated earlier). However what is amazing about Lakatosian research programs is the assertion that scientists should accommodate anomalies while at the same time remaining committed to the hard core. This signifies the spirit of flexibility and firmness in his methodology. These elements as highlighted here and as discussed previously demonstrate the strength of Lakatos' methodology and therefore show the extent to which his methodology partially succeeded in reconciling Popperian normative and Kuhnian descriptive account of science.

The major weakness of Lakatos' RP comes in when he brings about the concept degenerative research programmes (DRP). This is because he did not specify the amount of time a DRP should take before regaining. As pointed out previously, Lakatos argued that one should not give up a research programme at the first sight of difficulty, that would be too rush. Instead, he argued, a DRP should be given a certain amount of time to regain its force and once again become progressive. This argument triggered the question how long? (Lakatos, 1978:49-50).

Lakatos did not give an answer to the question above and as a result attracted the critique by Feyerabend who argued that Lakatos, in all practicality did not provide any methodological rule to follow (Feyerabend, 1978). Using his rule (Lakatos'), every conclusion can be defended as rational which means he did not succeed in differentiating between rational and irrational methods. Without breaking Lakatos' rules, one can claim that it is rational to give up a RP because it is degenerative, and that one should stick with it because it is in a stage of temporary difficulty.

Furthermore notes Johansson (2016: 112-115), there are well known historical examples that demonstrate that it can take several decades before one is able to overcome temporary difficulties. As a result, Feyerabend charactarised Lakatos' methodology as 'verbal ornamentation' signifying that Lakatos' was not a methodology at all, but mere words that appear like a methodology. For him therefore, Lakatos' approach was not different from epistemological anarchism, Feyerabend's own position (Feyerabend: 1978). These shortcoming or weakness therefore, demonstrates the failure of his methodology in reconciling normative and descriptive accounts of science.

With the help of the theoretical framework of this study, his account can therefore be referred to as 'an account of inference to the best explanation' because it gives a better, or lovelier picture of science than the one exemplified by the normative and the descriptive philosophers of science separately.

4.6 Conclusion

The aim of this chapter has been to evaluate Lakatos' MSRP through which he attempted to reconcile normative and descriptive accounts of science in Popper and Kuhn respectively and to determine whether he is successful or not in providing a harmonized picture of science.

However, this chapter fulfils its objective because it is able to bring out the extent to which Lakatos' framework reconciles normative and descriptive accounts in Popper and Kuhn respectively through analyzing the strengths and the weaknesses of his approach. After evaluation of this attempt, it can therefore be concluded that he did not perfectly succeed or totally fail; instead he succeeded partially and failed partially. This is because his account (as we saw earlier) is either 'normatively adequate and descriptively inadequate or normatively inadequate and descriptively adequate' and not totally normatively and descriptively adequate nor normatively and descriptively inadequate.

In the next chapter we are going to look at the findings, conclusion, achievements and recommendations of the entire study.

CHAPTER FIVE

FINDINGS, GENERAL CONCLUSION AND RECOMENDATIONS

5.1 Introduction

The general aim of this study has been to evaluate the success of Lakatos' attempt in reconciling normative and descriptive accounts of science. In order to achieve this goal, the study examined the normative account of science through Popper's theory of falsificationism in chapter two, the descriptive account of science through Kuhn's structure of scientific revolutions in chapter three and the possibility of their reconciliation in Lakatos' methodology of scientific research programmes chapter four.

5.2 Findings of the study

The first objective of this study was to examine the normative account of science as through Karl Popper's theory of falsificationism. It was realized that Popper, in reaction to the inductivist's and the Positivist's account of science, formulated a methodology that was normative in character. This was found out in chapter two whereby Popper, in his account, was of the view that science should essentially be concerned with producing norms, rules and cannons of how science ought to be practiced. Due to these features, it was concluded that Karl Popper formulated a normative account of science, an account that stipulates how science ought to be practiced.

The second objective was to examine the descriptive account of science throug Kuhn's theory of paradigm shifts in his structure of scientific revolutions. It was found out in chapter three that Kuhn essentially provided an account of science contrary to that of Popper. It was discovered that unlike Popper, who presented an image of how science ought to operate, Kuhn presented an image of the actual scientific practice. In other words, it was discovered that Kuhn's account looked at how science is whereby no recommendations are appended about how science ought to be practiced.

His theory, involved an abandonment of one theoretical structure and its replacement by another and it involved discovery of repeated patterns that are accepted by practitioners of science as constituting progress. These features therefore, strictly show that his account is purely a descriptive in character.

The third objective was to evaluate Lakatos' methodology of scientific research programmes; a model through which he attempted to resolve the diversity between the normative and the descriptive accounts of science. It was realized that the debate between Popper and Kuhn logically takes the Hegelian triadic formulae of 'thesis', 'antithesis' and 'syntheses'. Popper being the leading philosopher of science provided the thesis, then Kuhn as the leading historian of science, provided the antithesis and Lakatos in reaction to this conflict came in with a synthesis as an attempted solution. This solution is what Lakatos formulated in a very interesting way through his MSRP a structure that has different elements that enable it to capture the normative and the descriptive characters in Popper and Kuhn respectively. It was discovered that Lakatos concept of ‘research programmes’ certainly avoids the naivety of Poppers’ falsificationism whereby a theory is falsified simply because of one counter instance and on the other hand, it seems to be a more reasonable idea of theory replacement than Kuhn’s irrational theory of paradigm shifts because it avoids the Kuhnian paradox of incommensurability. This means therefore that, despite these weaknesses in Popperian and Kuhnian theory, the LMSRP (Lakatosian methodology of scientific research programmes) captures the good elements in them thus making a harmonized account of science that explains more perfectly, the way science operates.

The fourth objective was to determine whether Lakatos was successful or not in his attempt to blend the normative and the descriptive accounts of science. Despite the difficulties encountered in making this judgment, it was discovered that Lakatos succeeded to some extent and failed to some extent.

5.3 General Conclusion

From the findings of this study, it is clear that from Popper's theory of falsificationism, normative philosophies of science provide norms and recommendations on how science should be done whereas from Kuhn theory of paradigm shifts, descriptive philosophies of science do not give recommendations on how science should be done, but instead looks at the actual practice of science. In the attempt to reconcile the normative and descriptive philosophies of science, it was also clear that the picture of science that Lakatos exemplify through his methodology of scientific research programmes is one that is better than either normative account on its own or the descriptive account on its own.

Therefore, this study generally concludes that Imre Lakatos in his attempt succeeds to some extent and fails to some extent. On the part of success, Lakatos reconciled the normative and the descriptive accounts of science through the strengths of his methodology of scientific research programs whereby he captured the normative and the descriptive features in his account of science. On the part of his failure, Lakatos failed due to his inability to specify the amount of time (how long) a degenerative research programme is to be given in order to be progressive once again.

However, regardless of this weakness and despite the fact that the 'is' and the 'ought' are two levels of reality, Lakatos still found a way of putting the Popperian normative account together with the Kuhnian descriptive account in his model.

Thus, he deserves some credit and the picture of science he provides should be labeled ‘the Lakatosian normative-descriptive account of science’. This conclusion is enforced by the theoretical framework of this study that asserts that his approach can perfectly be referred to as ‘an account of inference to the best explanation’.

5.4 Achievement and contribution of the study

This study as it is evident from the findings, tried to achieve the objectives that had been set initially and despite the challenges encountered, most of them were satisfactorily achieved to a large extent.

However, there are three main areas in which the study made remarkable contributions. These were:

a) A clarification of the nature of a normative account of science.

b) A clarification of the nature of a descriptive account of science.

c) A clarification of the possibility to merge the normative (ought) and the descriptive (is) accounts of science.

In the clarification of the meaning of the normative account of science, this study began by examining the meaning and the role of science, philosophy of science and eventually of the normative philosophy of science. In this case, the study examined one of the most prolific flag bearers of the normative thought, and identified the features of a normative account through Karl Popper’s theory of falsificationism. Through his theory of falsificationism therefore, the actual nature of a normative account of science was clarified.

In the clarification of the descriptive account of science, this study begun by exemplifying the meaning of a historical approach to science as an eye opener to understanding the actual nature of a descriptive or historical philosophy of science. Through Kuhn's descriptive approach to science in his theory of scientific revolutions, the actual nature of a descriptive account of science was clarified.

In the clarification of the possibility to merge the normative (ought) and the descriptive (is) in philosophy of science, this study begun by highlighting the argument between the normativist and the descriptivist philosophers of science and then examined the nature of a philosophical-historical account of science which acted as a stepping stone to substantiating Lakatos' MSRP. It was found out that Lakatos' attempt to merge the normative and the descriptive accounts of science partially succeeded and partially failed. However, it was confirmed that his was a better account than either the normative or the descriptive (each on its own) because it captured normative and descriptive features and can therefore be referred to as an account of inference to the best explanation.

5.5 Recommendations

1) That further study be undertaken to check if there are case studies that show Lakatos' account as normatively adequate and descriptively inadequate.

2) That further study be undertaken to check if there are case studies that show Lakatos' account as normatively inadequate and descriptively adequate.

3) That further study be undertaken on other case studies that show Lakatos' account as normatively inadequate and descriptively inadequate.

4) That further study is undertaken to check if there are other case studies that show Lakatos' account as normatively adequate and descriptively adequate.

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