CRITICAL RATIONALISM AND 'NORMAL SCIENCE' - A PROPOSED RESOLUTION OF THE POPPER/KUHN CONTROVERSY AND ITS IMPLICATIONS FOR THE SOCIOLOGY OF SCIENCE

by

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ABSTRACT

The thesis seeks to examine the relevance of philosophical and sociological theories to the explanation of the cognitive content of science.

During the past two decades, two apparently irreconcilable perspectives have emerged - one stressing the significance of the 'internal logic' of scientific enquiry while the other focusses upon the over-riding importance of the 'external' (i.e. the social , political and subjective) context of scientific discovery. The 'internalist' view is best expressed in the work of Karl Popper. Popper, a critical rationalist, argues that science can only be understood through a 'rational reconstruction' of the logic of its development - a logic which is expressed in the application of the hypothetico-deductive or falsificationist method. Thomas S. Kuhn, in contrast, argues that while logic and rationality play a role within science, the activity of scientists is best captured empirically through an analysis of the institutional framework of science and of the actual behaviour of scientists. Imre Lakatos, although himself a critical rationalist, differs in important respects from Popper. He seeks to explain why it is rational to ignore potential falsification of productive theories. His analysis seeks to take into account elements of Kuhn's work but retains a commitment to 'rational reconstruction'.

The argument of this thesis is that a reconciliation between Popper and Kuhn is possible through a reinterpretation of Kuhn's views. Lakatos's approach suggests that both internal and external factors are relevant to an understanding of the growth of scientific knowledge. The logic of science is, however, explicable uniquely by internal factors.

Contemporary accounts within the sociology of science underplay or ignore critical rationalist criteria, preferring to explain science through the cultural, social and bureaucratic milieu within which it operates. The debate concerning the respective significance of critical rationalism as against Kuhnian sociological analysis remains controversial. This theoretical confrontation has, however, led to a greater interest in the empirical investigation of the scientific community. Such empirical work has been conducted within a broadly Kuhnian perspective. There remains the task of seeing whether such empirical evidence can be interpreted in a way consistent with Lakatosian versions of critical rationalism.

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CHAPTER I

EXPOSITION OF KARL POPPER'S CRITICAL RATIONALISM

By critical rationalism, Popper means, essentially, the challenging of scientific theories and their subsequent acceptance or rejection according to methodological rules. It attempts to answer the question: What makes us choose one theory rather than another? Criticism is basic to all of Popper's views and can best be understood through an analysis of his views on the growth of scientific knowledge.

According to Popper, all scientific knowledge is grounded in tradition, that is previous knowledge. One does not start at square one but with some background knowledge. A theoretical framework is thus essential to serve as a guideline in research. "All knowledge is theory-impregnated, including our observations", Popper states.[1] Scientific tradition has its source, in the fifth and sixth centuries before Christ, in the early Presocratic Greek philosophers, especially those of the Ionian school. They introduced the rationalist tradition by discussing and criticising myths until then uncritically accepted. Within this tradition, the growth of knowledge consists of modification of earlier knowledge.[2] The growth of scientific knowledge is defined by Popper in terms of progress. Progress does not mean

> ...accumulation of observations... but the repeated overthrow of scientific theories and their replacement by better one or more satisfactory ones.[3]

Scientific knowledge grows in the same way that ordinary knowledge does, that is by the method of trial and error. We learn from our mistakes, says Popper.[4] Truth exists but human cognitive abilities prevent us from attaining absolute truth. We can never be sure when we have attained it. What we can attain is an approximation to truth, that is a progresssively better correspondence of our theories to the facts.[5] Popper often quotes the following extract from Xenophanes to illustrate the forever provisional and conjectural character of human knowledge:

> The gods did not reveal, from the beginning,
> All things to us; but in the course of time,
> Through seeking, men find that which is the better...
> These things are, we conjecture, like the truth.
> But as for certain truth, no man has known it,
> Nor will he know it; neither of the gods,
> Nor yet of all the things of which I speak.
> And even if by chance he were to utter
> The final truth, he would himself not know it:
> For all is but a woven web of guesses.[6]

It is in criticising their theories and submitting them to empirical tests that scientists can assess both the falsity and truth content of theories. Consequent upon the most severe tests, theories are accepted or rejected. "Science operates with conjectures,"[7] Popper tells us, and theories are only tentative hypotheses. Scientists attempt to falsify their theories and to replace them by better ones, closer to the truth. A new solution creates a new problem because, in answering some questions, new problems are engendered. Popper states:

> Yet perhaps even this picture of science - as a procedure whose rationality consists in the fact that we learn from our mistakes - is not quite good enough. It may still suggest that science progresses from theory to

> theory and that it consists of a sequence of better and better deductive systems. Yet what I really wish to suggest is that science should be visualised as *progressing from problems to problems* - to problems of ever increasing depth.[8]

Problems

> ...as a rule arise from the clash between, on the one side, expectations inherent in our background knowledge and, on the other side, some new findings, such as our observations or some hypotheses suggested by them.[9]

It is the continual overthrow of new solutions to problems by criticism and falsification that embodies the logic of science.

What is specific to science is that it has a criterion of progress. This "criterion of relative potential satisfactoriness" allows the evaluation of theories even before they are submitted to empirical testing. We can then know whether a theory represents an improvement over other theories. Popper writes:

> It [the criterion of relative potential satisfactoriness] characterises as preferable the theory which tells us more; that is to say, the theory which contains the greater amount of empirical information or *content*; which is logically stronger; which has the greater explanatory and predictive power; and which can therefore be *more severely tested* by comparing predicted facts with observations. In short, we prefer an interesting, daring, and highly informative theory to a trivial one.[10]

Thus, this criterion identifies the best theories as those with the highest degree of testable empirical content. This criterion guides scientists in their choice of the better theory.

The above characteristics of the growth of knowledge are explained through the schema of an evolutionary process of a natural selection of theories where the fittest survive. Popper states:

> ...the growth of our knowledge is the result of a process closely resembling what Darwin called "natural selection"; that is the *natural selection of hypotheses*:our knowledge consists, at every moment, of those hypotheses which have shown their (comparative) fitness by surviving so far in their struggle for existence; a competitive struggle which eliminates those hypotheses which are unfit.[11]

The goal of the tree of human knowledge is not survival per se, but the elimination of unfit theories, replaced by more satisfactory explanations. What survives are the best theories. The evolution of knowledge is also differentiated from the evolution of species in that, Popper agrees with Herbert Spencer, "it is largely dominated by a tendency towards increasing integration towards unified theories".[12] While the problems science has to face increase and become more differentiated, scientific knowledge grows more integrated.[13] It is through the method of critical rationalism that scientific knowledge will attain that ultimate goal. Criticism is essential to growth; for without it science ends. Popper writes:

> I assert that it is the method of critical discussion and the critical attitude... which makes progress in science possible - that is, the choice of better theories. This critical method constitutes the rationality of science. One may also say: if scientific knowledge ceases to grow, if science ceases to progress, it will lose its rational and its empirical character; for the rational and empirical character of science lies in the way it makes progress - and this means, the way in which we discriminate between theories and choose the better theories.[14]

* * *

Scientific knowledge is to be distinguished from other types of knowledge. Popper defines science in terms of the logical consequences of its aims, which consist of

> ...ever discovering new, deeper, and more general problems, and of subjecting our ever tentative answers to ever renewed and ever more rigourous tests.[15]

The aim of science is both theoretical (in its explanations) and practical (in its predictions and application). Popper remarks that it is science viewed as a rational activity that conveys the theoretical aim "to find *satisfactory explanations*, of whatever strikes us as being in need of explanation".[16] A scientific explanation has two components: the explicandum and the explicans. The explicandum is "a set of statements by which one describes the state of affairs to be explained".[17] The explicandum is usually known to, or assumed to, be true. The explicans consists of "explanatory statements". They represent the explanation itself of the explicandum. As the explicans is what is looked for, it is not known to be true, but it ought, at least, not to be known to be false. This description illustrates what Popper means when he writes that a scientific explanation is "*the explanation of the known by the unknown*".[18] The explicans has to be *independently testable* from the explicandum; if not, the explanation is circular or ad hoc. In order to achieve this independence, explanations must be universal statements rich in content, allowing a variety of different tests.[19] A satisfactory explanation is thus

> ...an explanation in terms of testable and falsifiable universal laws and initial conditions. And an explanation of this kind will be the more satisfactory the more highly testable these laws are and the better they have been tested.[20]

An explanation can be analysed in terms of a logical deduction. It is

> ...a deduction whose conclusion is the *explicandum* - a statement of the thing to be explained - and whose premisses consist of the *explicans* (a statement of the explaining laws and conditions).[21]

We can deduce what happens from the explicans with the use of universal laws as a part of the explicans. For instance, we may want to find the explanation of the death of a rat. The explicandum will state what we know and want to explain: "This rat here has died recently." Then, we will put forward an hypothesis from which the explicandum can be deduced. It could be: "This rat has eaten some bait containing a large dose of rat poison." But this statement would not be a satisfactory explanation because it is not specific enough. A good explicans would have two premisses: universal laws and initial conditions. The universal law will be: "If a rat eats at least eight grains of rat poison it will die within five minutes." The initial conditions may be put like this: "This rat ate at least eighteen grains of rat poison, more than five minutes ago." From the premisses of the explicans, the initial condition and the universal law, we are now able to deduce that the rat died quickly.[22] This example illustrates the need of both universal laws and initial conditions to formulate valid and satisfactory scientific theories.

The second aim of scientific activity, this one more directly pratical, consists in the formulation of predictions and their technical application. It is analysed within the same logical framework. Predictions are the logical consequences of the explicans, but not immediately observable. Predictions deduced from the explicans serve to test the explicans in comparing it with an observable situation. So, besides their practical role, predictions are of prime importance in the procedures to test a theory. Popper remarks:

> I have in later years (from 1950 on) made a sharper distinction between the theoretical or explanatory and the practical or 'instrumental' tasks of science, and I have stressed the logical priority of the theoretical task over the instrumental task. I have tried to stress, more especially, that predictions have not only an instrumental aspect, but also, and mainly, a theoretical one, as they play a decisive role in testing a theory.[23]

The explicans is falsified if the prediction does not agree with observation. A test does not reveal whether it is the universal laws or the initial conditions, or both, that are false. The corroboration of the prediction, does not, however, mean that the explicans is verified, as a true prediction may be deduced from a false explicans.[24]

* * *

What best characterises scientific activity is its conjectural nature. Knowledge is provisional and will remain so. With Popper, science is not viewed as a "body of knowledge" known for 'certain', (as it is with empiricists and positivists), but, on the contrary, it is a system of hypotheses", that is:

> ...a system of guesses or anticipations which in principle cannot be justified, but with which we work as long as they stand up to tests, and of which we are never justified in saying that we know that they are 'true' or 'more or less certain' or even 'probable'.[25]

In proposing hypotheses scientists aim at a true description of the world, that is at the explanation of observable facts. They can never be certain whether their theories are true, Popper constantly reminds us, but they may sometimes have good reasons to think that their findings are false.[26] The conjectural nature of scientific knowledge is illustrated by a simile:

> Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being.[27]

This "temporary" knowledge provides science with a new definition. It is the objectivity and the rationality provided by the method and logic of science that give its character as scientific knowledge. Science is an empirical theoretical system in that scientific theories are tested by experience. With Popper, experience becomes a method of demarcation in that it serves to test statements. It is that testability that gives science its empirical character.[28]

The method of science then, which is the same for all sciences according to Popper, rests on criticism and aims at falsifying theories. Positivists, Popper writes, are mistaken in demarcating science by the *meaningfulness* of statements defined through the inductive method. For positivists, what is meaningful is reducible to "elementary statements of experience." Statements which do not conform to that requirement are considered metaphysical, that is, meaningless. Positivists claim that observation *verifies* statements and ascertains conclusively the truth or falsity of statements. If this were so, Popper argues, statements would have to take a form that is both verifiable and falsifiable. While scientific statements ought to be falsifiable, however, no theories can be empirically verifiable in the strong sense intended by logical posivitists.[29] Popper writes:

> If we renounce this requirement [of conclusive verification and falsification of statements] 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.[30]

* * *

A distinction has to be made between falsifiability which, according to Popper, is "a criterion for the empirical character of a system of statements", and falsification, which Popper refers to as the "conditions a system is to be regarded as falsified".[31] The falsifiability of theories is the criterion of demarcation for science. It stipulates that:

> ...statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations.[32]

It is this that gives science its empirical character and distinguishes it from metaphysics. For Popper, metaphysical theories, that is non-testable theories, remain meaningful and sometimes useful, but have no interest for science for the time being, although they may become a part of science in the future.[33] Popper justifies his criterion of falsifiability by arguing that absolute knowledge is inaccessible and that we never have any fully compelling reasons to be certain of our knowledge. Past experiences are no guarantee of future occurrences. In solving Hume's problem of induction, Popper is able to endorse some modified version of the principle of empiricism because observation serves, not to *justify*, but as a criterion relevant to the acceptance or rejection of scientific statements. While the truth of a statement cannot be fully rationally justified, its falsity can sometimes be inferred from empirical evidence. This inference is not inductive but purely deductive.[34] Falsifiability restores a properly conceived empirical dimension to scientific statements.

The objectivity of empirical statements does not depend on the sincerity or honesty of individual scientists. Rather, it lies in the ability of the statements to be logically criticisable, that is, "in the fact that they can be *inter-subjectively tested*". Popper writes that a statement is objective "if in principle it can be tested and understood by anybody."[35] The more statements are presented in a way that they are testable and criticisable, the more they are objective.[36] Objective knowledge belongs to what Popper calls World three. It is the world of objective thoughts, that is the logical content of scientific theories as expressed, for example, in books, and thus detached from their authors. It includes criticism and rationality and all the consequences of theories, such as problems and discussion engendered by them. Even though it is produced by and can be affected by the intellect, World three is considered largely autonomous because it can grow without external contact. For instance, new discoveries are possible within the theories contained in World three. Objective knowledge, however, grows mostly through our interaction with World three. Popper writes:

> ...nobody, neither its creator nor anybody who has tried to grasp it [a theory], can have a full understanding of all the possibilities inherent in a theory; which shows again that the theory, in its logical sense, is something objective and something objectively existing - an object that we can study, something that we try to grasp.[37]

Theories are detached from their authors because even authors do not understand their own theories fully. In fact, it is in part that detachment that conveys its scientific character to a theory in making it 'available' to the criticism of the scientific community and linking it to the scientific background knowledge in order to assess it.[38] That is why Popper states that World three contains:

> ...knowledge without a knowing subject. [39]

But the judgment of the scientific community is not absolute. It may always be challenged in the future. What is important is that the rejection or acceptance of a theory is reached by rational decisions of scientists guided by methodological rules. World two has no part in it. World two represents conscious experiences and mental states. It is the world of conscious subjective knowledge, including psychological states. World two is said to depend on World three because all our activities depend on our theories. For Popper, the study of World three can aid in understanding World two, but this latter is irrelevant to the study of scientific knowledge.[40]

There is always a possibility, however, of escaping from the consequences of the falsification of a theory, by adapting it to make it fit a contrary observation. The purpose of Popper's first methodological rule is precisely to avoid falling into this conventionalist mistake. Some statements, called auxiliary hypotheses, can be added to a theory but only if they increase the falsifiability and testability of a theory. They then make the theory stronger in, for instance, predicting new events which necessitate more tests and possibilities of falsifications. Auxiliary hypotheses replace the conventionalist ad hoc hypotheses.[41] What distinguishes ad hoc hypotheses is that they are not and cannot be independently testable.[42] The second methodological rule refers to definitions in an axiomatic system. Popper advises using explicit definitions and avoiding changing them. In science, axioms are not considered conventions, but hypotheses. One should therefore be aware that changing definitions means changes in the system. The system must then be reviewed and looked at anew. Finally, conventionalists may always employ the tactic of questioning the honesty or competence of an experimenter and thus reject the possibility of ever falsifying a theory. But, with Popper's theory, the reliability of individuals is not in question:

experiments are inter-subjectively testable. Scientists may opt for more tests if they judge it necessary. But it is their decision that has the final word, not the experiment itself.[43]

Scientific decisions are considered 'conventions' by Popper but they are to be dissociated from the views generally referred to as 'conventionalism'. According to Popper, conventionalism refers to the views that all knowledge is the result of "our own creation".[44] The laws of natural science do not reflect the physical world because they are seen as mere conventions and arbitrary creations. Consequently, theories in natural science take the form of logical constructions determining the properties of the physical world. These artificial frameworks are the only possible means of understanding nature. Theories are, therefore, considered conventions, unfalsifiable and untestable, because they are only the product of the mind.[45] Popper's views on science and its conventions are of a very different nature. Science does not 'create' laws, but discover them. Popper recognises that all our theories may be false and that we cannot possibly recognise the truth. Absolute certainty is, consequently, not possible but conjectural and provisional knowledge is. Theories are considered to reflect reality. With Popper, conventions, which refer to methodological rules and scientific decisions, do not mean arbitrary decisions but, rather, a common decision reached after sometimes long deliberations.[46] Popper explains the meaning of conventions by using the analogy of the verdict of a jury which he compares to scientific decisions. The verdict is reached following some procedural rules but it is not, however, considered true. Like scientific decisions, the verdict can be questioned and rejected. The jury accepts or rejects, by agreement, the facts presented to it. The questions raised, the facts presented and the decisions made are situated within, and depend on, the legal context. Similarly, scientists assess basic statements in relation to methodological and theoretical considerations and initial conditions.[47] The criterion of falsifiability can be considered a convention in the sense that it is a requirement for the demarcation of scientific statements.[48] Methodological rules are also conventions as they are required to ensure scientific objectivity.[49] What distinguishes Popper's views from those of the conventionalists is that scientific decisions are not agreements upon universal statements (whole theories as argued by conventionalists), but upon singular statements. Scientists accept or reject the relevance of basic statements used to test a theory.[50] In Popper's own words:

> From a logical point of view, the testing of a theory depends upon basic statements whose acceptance or rejection, in its turn, depends upon our *decisions*. Thus it is *decisions* which settle the fate of theories... [T]he convention or decision does not immediately determine our acceptance of *universal* statements but..., on the contrary, it enters into our acceptance of the *singular* statements - that is, the basic statements.[51]

Popper's conventions may be said to have an element of arbitrariness in the sense that decisions are not logically derivable from the basic statements themselves or directly from the experiment.[52] Scientific decisions are not, however, arbitrary in the sense of a mere personal preference, as they are always rationally justifiable.

As a criterion of demarcation, falsifiability necessitates some important logical requirements. First, theories have to be strictly universal statements in order to be submitted to empirical tests. Universal statements themselves are not tested, but singular cases deduced from them are. Strictly existential statements can be included in a theory because they may bring more content and therefore increase the testability of the theory. However, isolated existential statements do not have a scientific status because their lack of empirical testability makes them unfalsifiable. Strictly existential statements are not empirical for the same reason that universal statements are not verifiable, which is that

> We cannot search the whole world in order to establish that something does not exist, has never existed, and will never exist.[53]

Or, for that matter, that something exists. The first characteristic of scientific theories is to *prohibit* the occurrence of some events. It is because of the possibility of a contrary existential statement that theories are falsifiable. The occurrence of what is prohibited would falsify the theory. Even when a scientific statement takes the form of a strictly existential statement, it always, directly or not, denies the existence of some event. For instance, Popper gives the example of the atomic numbers of elements in physics. It is an existential statement to assert a specific atomic number attaches to given element. But, Popper explains, this number was not determined by simple observation or only by uncovering its existence. Its discovery was made possible by deducing its characteristics and existence from a more general scientific theory which prohibited the existence of some events. Also, the existential statements about atomic numbers are not isolated, but are part of a universal theory to which they give content.[54]

Now that the criterion of demarcation has been established, the process of falsification can be analysed more directly. As stated previously, it is not the whole theory itself that is directly tested because each theory is composed of a number of universal statements. It is the basic statements that are tested. A basic statement is "a statement which can serve as premise in an empirical falsification; in brief, a statement of a singular fact."[55] Basic statements are deduced both from universal statements and initial conditions. They do not have to be falsifiable themselves because that would lead to infinite regress. Initial conditions are singular statements directly derived from the universal statements, and they describe "what is usually called the *cause* of the event in question." For instance, Popper explains, a universal law could be stated as:

> Whenever a thread is loaded with a weight exceeding that which characterizes the tensile strength of the thread, then it will break.[56]

The initial conditions would then be:

> The fact that a load of 2 *lbs.* was put on a thread with a tensile strength of 1 *lb.* was the 'cause' of its breaking.[57]

The basic statement, "this thread will break", is deduced both from the hypothesis and the initial conditions.[58] Basic statements assert that "an observable event is occurring in a certain individual region of space and time".[59] They take the form of singular existential statements. Basic statements can falsify a universal statement, and vice versa. However, the statement falsifying a basic statement cannot logically be a basic statement itself as this contrary statement would be deduced only from the universal statement itself, which is a strict "non-existence statement". Also, basic statements must be testable by observation and therefore inter-subjectively testable.[60] For a theory to be falsifiable, all basic statements derivable from it (in conjunction with the initial conditions) must be either potential falsifiers or permitted statements. Potential falsifiers are the basic statements the theory prohibits. They are essential for the theory to be falsifiable. Permitted statements are those that the theory "permits". A theory asserts the falsity of potential falsifiers but says nothing about the permitted statements and especially not that they are true.[61]

A single counter-example is however not sufficient to falsify a theory: to be valid, the refutation has to be reproducible. The degree of testability of theory is given by the number of its potential falsifiers. The more numerous the potential falsifiers are, the more opportunities the theory has to be refuted. A high degree of testability makes a theory more easily falsifiable as it has more empirical (basic) statements deduced from it and few permitted statements. This is one of the characteristics of the best theories. For Popper, the aim of scientific activity is precisely to produce that kind of theory, i.e., those that say "more about the world of experience" and that "rule out a larger class of basic statements." If such a theory survives the most severe tests, then it will have the highest degree of universality and will describe the world in the most precise possible way.[62]

The falsification of a theory is considered a success by Popper not only because false knowledge is thus eliminated, but also because in identifying new problems the growth of knowledge is stimulated. If we are, however, to talk of progress of science, some corroborations are needed.[63] Popper states:

> Earlier I suggested that science would stagnate, and lose its empirical character, if we should fail to obtain refutations. We can now see that for very similar reasons science would stagnate, and lose its empirical character, if we should fail to obtain verifications[64] of new predictions.[65]

The fact that a theory has been corroborated does not mean that it is true, whatever the number of corroborations. It only means that, so far, no tests have been able to refute it. The degree of corroboration is dependent on the degree of testability of a theory, the richness of its content and its explanatory power.[66] Corroboration refers to "the degree to which an hypothesis has stood up to severe tests and thus 'proved its mettle'".[67] Popper writes:

> ...it is not so much the number of corroborating instances which determines the degree of corroboration as *the severity of the various tests* to which the hypothesis in question can be, and has been, subjected. But the severity of the tests, in its turn, depends upon the *degree of testability*, and thus upon the simplicity of the hypothesis: the hypothesis which is falsifiable in a higher degree, or the simpler hypothesis, is also the one which is corroborable in a higher degree.[68]

A corroborated theory does not mean either that it is probable. On the contrary, the best scientific theories are the *least* probable because, ruling out a great number of events, they have less chance of being corroborated. The degree of corroboration increases not only with the number of corroborations but with the variety of tests to which it is submitted. If a theory is tested several times in one area, future corroboration will not affect very much the degree of corroboration because the theory is already considered corroborated. But if a theory is tested in new fields of application and still resists refutation, then its degree of corroboration is increased significantly. This indicates that a theory with a higher degree of universality has a higher degree of corroboration. That is to say that a theory is assessed by its logical relation with the basic statements deduced from it (and from the initial conditions). It is thus closely related to its degree of falsifiability; the more falsifiable a theory is, the higher is its degree of corroboration (if the theory is not refuted by test). The logical tie between a theory and its basic statements not only serves to assess the degree of corroboration but also its limits. We can never say that a statement is itself corroborated, but rather that a statement is "*corroborated with* *respect to some system of basic statements* - a system accepted up to a particular point in time."[69] It is thus important to remember that a corroborated theory has been subjected to a specific kind of corroborations - which means that all possible corroborations have not been exhausted. Therefore, corroboration cannot convey a truth value.[70]

Testing could go on for ever. When, then, do we stop testing a theory? It is a question of decision agreed upon by scientists. Popper states:

> This procedure has no natural end. Thus if the test is to lead us anywhere, nothing remains but to stop at some point or other and say that we are satisfied, for the time being...[W]e are stopping at statements about whose acceptance or rejection the various investigators are likely to reach agreement... If some day it should no longer be possible for scientific observers to reach agreement about basic statements this would amount to a failure of language as a means of universal communications. It would amount to a new 'Babel of Tongues': scientific discovery would be reduced to absurdity.[71]

Popper does not give any specific recommendations as to what to do in practice with theories that have been falsified. As a basic principle, a theory falsified by rigourous tests has to be rejected. Popper writes:

> If the outcome of a test shows that the theory is erroneous, then it is eliminated; the method of trial and error is essentially a method of elimination.[72]

This is consistent with Popper's view on the logic of critical rationalism and the evolutionary growth of knowledge. Scientific knowledge grows because better theories replace previous ones demonstrated to be false. If the number and variety of theories are sufficient to challenge previous knowledge and if these theories are submitted to tests numerous and severe enough, Popper writes:

> ...we may, if we are lucky, secure the survival of the fittest theory by elimination of those which are less fit.[73]

But, he continues, while we generally prefer to retain and use unfalsified theory, we may sometimes choose not to reject a falsified one. Although false, such a theory may still be useful. Popper explains:

> ...false theories often serve well enough: most formulae used in engineering or navigation are known to be false, although they may be excellent approximations and easy to handle; and they are used with confidence by people who know them to be false.[74]

Another reason why scientists keep using falsified theories is that sometimes the whole theory has not been falsified, but only some of its basic statements. The rest of the theory is not necessarily affected and can still be used.[75] It may be added that, although Popper does not always specifically say so, a falsified theory is in fact rejected when a better theory is discovered and replaces it.[76]

* * *

Critical rationalism thus rests on the falsifiability of theories, but the inconclusive nature of observation raises some problems as to the empirical basis of knowledge. Popper's epistemology challenges the commonsense theory of knowledge. According to Popper, this theory rests on the mistaken view that all knowledge comes from our senses. The theory assumes a passive absorption of knowledge. This empiricist approach to knowledge, perpetuated by logical modern positivists, is based on a quest for certainty. For Popper, the *sources* of our knowledge are numerous and not important. "[T]here is no such thing as a logical method of having new ideas"[77], states Popper. For him, not only do we make mistakes in our perceptions but, even more importantly, "there is no uninterpreted empirical basis" because "facts [are] interpreted in the light of theories".[78] For Popper, theories precede observation - to observe, one needs something to observe. He states:

> Observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem.[79]

Contrary to the commensense theory of knowledge, Popper argues, the role of theories is to guide our observations.[80] It has to be noted that Popper distinguishes between perception and observation. Perceptions are physiological activities. An observation, however, is not accidental. Here Popper refers to a scientific observation, a test in other words, which is planned and prepared. We "make" an observation, Popper remarks, whereas we "have" a sense-experience. There is a conscious purpose to an observation. That is why

> An observation is always preceded by a particular interest, a question, or a problem - in short, by something theoretical.[81]

Theories, however, can never be free from assumptions because theories always presuppose expectations.[82]

Popper argues that scientific knowledge cannot logically be considered certain knowledge. Knowledge is evaluated by testing propositions against reality. Even though tests follow rigourous objective procedures, it remains that tests are set up within theoretical frameworks which also delineate the observation itself as well as the assessment of the results of observation.[83] Results of empirical testing are always inconclusive because tests can only check single instances. Tests can be repeated, but the corroboration or falsification of an hypothesis cannot rationally have a universal application. Indeed, observation cannot provide conclusive assessment that could logically hold for the future because we have no more *rational* justifications to believe in the certainty of the outcome of a test than we have to believe in the repetition of a singular event in the future. The rationality of the scientific method provides objectivity, but not certainty.

With Popper, the empirical basis of science plays a secondary role in the growth of knowledge in the sense that the results of testing must always be interpreted, judged or balanced. It is in that sense that Popper insists that theories are accepted by convention, that is that scientists decide to accept a theory or to subject it to more tests whatever the outcome of the previous tests. Tests are not expected to 'verify' knowledge claims but only to give inconclusive information as to the correspondence of hypotheses with reality. Consequently, knowledge ought to be considered provisional.

A.J. Ayer disagrees with Popper on the importance of the role played by empirical facts and rationality. He sees an inconsistency in, on the one hand, trusting observation enough to use it to test our knowledge but, on the other hand, in not trusting observation enough to rely on the outcome of tests. He strives to demonstrate that logic and rationality are secondary to experience. Ayer's stand is a modern restatement of the tradition of British empiricists. For him, knowledge has its source in sense-experience, but not uniquely. Indeed, Ayer makes a distinction between the origin of knowledge and the way it is verified. He agrees with Popper that hypotheses are not necessarily inspired by observation; when scientists formulate hypotheses, they may begin with an intuition, without yet having any observations to justify the intuition. Theorising may depend on deduction as well as on induction. The verification of our hypotheses is, however, a different process: knowledge *is* certified, and only by observation.[84] Ayer argues for the possibility of conclusive verification for some empirical propositions.[85] His arguments are based on his solution to the problem of the logical asymmetry in the relationship of universal and singular propositions. He identifies one kind of empirical proposition that can be directly and conclusively verified. The validity of universal propositions can be established indirectly through the assertion of the truth or falsehood of singular propositions referring to a given event. In their turn, these singular propositions are themselves verified by the verification of propositions referring directly to material things. The truth or falsehood of these singular propositions referring to sense-data (called basic propositions) can then be determined directly by observation, and so be conclusively verified.[86] Ayer's principle of verification can be described as follows: the truth or falsehood of a meaningful statement (which is not analytic)[87] is conclusively verified in a direct manner by observation-statements that record an actual observation, or indirectly by observation-statements that record a possible observation.[88] Basic statements are thus verified by "the occurrence of the experience to which they uniquely refer".[89]

For Ayer, the influence of a theoretical framework does not inhibit certainty. His insistence on the reliability of verified hypotheses is grounded in his empiricist trust in sense-data. He argues that despite the fact that a proposition referring to sense-data contains no guarantee of future occurrences, its "agreement" with reality ensures the probability of its repetition.[90] It remains that

> ...to doubt the truth of such a proposition is not merely irrational but meaningless; for it is only significant to doubt where there is a logical possibility of error.[91]

For Ayer, our reasons for accepting the verification of the correspondence between basic statements and facts are found in our experience. To have an experience means to have sense-data, and we know that we have sense-data because we sense them.[92] In this way, Ayer postulates that basic propositions may be considered incorrigible. He holds that basic statements are incorrigible if

...what is meant by their being incorrigible is that it is impossible to be mistaken about them except in a verbal sense.[93]

Ayer points out that one cannot be mistaken in describing the content of one's experience, which is what basic statements do. One might use an incorrect word in referring to a material thing. In that sense, it is possible to make a linguistic error. But the point is that we cannot be incorrect in the description of our sense-data because they are the effect of a perception. Here, it is irrelevant whether what one perceives really exists or not.[94] What counts is that the perception occurs and produces sense-data that cannot be mistaken. Sense-data being what we sense by a perception, they have, by definition the properties of what is perceived.[95] It is in that sense that basic statements are considered incorrigible. Even though we may sometimes make an error of fact, we can still rely on our senses because they are right most of the time.[96] Ayer rejects what he believes to be the sceptical consequences engendered by the denial of the possibility of certain knowledge because, for him, its claims are not reasonable. "[W]e must be content with what we have",[97] he writes. About the sceptic who claims an absolute proof, Ayer remarks:

We can say that he is irrational; but this will not worry him; our standard of rationality is just what he objects to. Our only resource is to point out, as we have done, that the proof that he requires of us is one that he makes it logically impossible for us to give. It is, therefore, not surprising that we cannot furnish it: it is no discredit to the proofs which we do rely on that they do not imply that we can achieve the impossible; it would be a discredit to them, rather, if they did.[98]

This last quote embodies Ayer's ambivalence about certainty. It implies that truth is ascertained by the corroboration of hypotheses, even though there is no rational justification for being certain. For Ayer, it is irrational to be sceptical. His definition of rationality, however, takes the form of a notion of 'reasonability'. Indeed, he insists, to be certain is not so much a question of rationality as a question of accepting the limitations of what is possible. We are thus entitled to say that sense-experience verifies conclusively the truth or falsehood of an hypothesis because asking more than that is not reasonable. [99]

For Popper, however, rationality comes first. Contrary to Ayer, he denies the supremacy of sense-experience as the source of knowledge, and can hence dispense with induction which he considers a myth. Hume had made induction appear irrational. In order to reinstate the rationality of empirical tests, Popper argues that not only is induction not necessary in scientific theories but, even more, that it does not exist.

For Popper, Hume's account is psychological in the sense that Hume explained induction in terms of custom or habit. Hume tried to give a causal explanation of how we acquire beliefs. Hume was mistaken, according to Popper, in several ways. First, the repetition of an action does not generally lead to a belief because a belief is first necessary to justify the repetition. With time, following Hume, repetition should increase belief but, in fact, Popper notes, it is usually the opposite that occurs: belief decreases or becomes unconscious with time. For instance, the special concentration first needed to play a difficult passage on the piano is no longer necessary after repetition has made the execution automatic. Learning to ride a bicycle is another example. At first, the beginner is very much aware of the possibility of falling. The "belief" that certain movements of the steering mechanism will prevent falling becomes, however, redundant with practice. The process has become unconscious. Secondly, the repetition does not engender habits because while the repetition of an act may be called a habit, the habit is already present before any repetition, (such as with speaking or walking). On the other hand, as Hume admits, the single occurrence of a striking observation may create a belief or an expectation. Contrary to Hume's views, Popper concludes that an assumption, an expectation or a belief must precede repetition in order for the observer to make a link between the observed events. Also, Hume neglected discrepancies between different instances of a given event. Repetitions are never identical. Consequently, Popper states, a point of view preceding the observation is logically necessary in order to see a similarity between different, (even if only slightly), occurrences. Popper agrees with Hume that we are born with expectations. Contrary to Hume, these expectations do not, however, make us believe what we see, according to Popper. The role of expectations is restricted to making us see a similarity between repetitions. We try to discover similarities in repeated events, but far from being content with them, we interpret our observations in light of present theories. The conclusions we reach remain conjectural and susceptible to refutation. Viewed in this way, scientific beliefs are neither dogmatic nor irrational beliefs, but only a "critical and tentative acceptance of scientific theories."[100]

The above discussion underlines the significance of Popper's criterion of demarcation. He agrees with Hume that the truth of a statement describing observed instances or unobserved instances (such as explanatory universal theories) cannot be rationally justified by observation. Its falsity may, however, be rationally inferred from the observation of contrary instances. Hume's problem of induction is therefore solved by Popper's concept of falsification, which dispenses with inductive reasoning.[101]

Ayer discusses the problem of the rationality of induction and causality in focussing on the limits of rationality. He agrees with Hume and Popper that inductive reasoning is rationally unjustified.[102] He argues, however, that it is a misunderstanding of Hume's intent to conclude that induction must be rejected and that scepticism is consequently unavoidable. For Ayer, Hume's demonstration that inductive reasoning cannot be rationally justified serves only to indicate the limitations of rationality as a means of explaining induction. It is true that there is no logical link between "what is" and "what will be" - an effect cannot be deduced from the cause itself. However, since our past experiences show us a link between a given cause and a given effect, Ayer argues that it remains rational for us to expect a similar occurrence in the future.[103]

Ayer explains why we think in terms of causality by discussing the logic of the succession of events. We come to think that an event is caused by another because, he explains, we know that past events existed, and what they were. We do not know future events, but we know that, logically, a non-existent event cannot have any effect on a past event. We see a causal direction of events, that is we think forwardly, because we know that past events come before future events. We know this from experience since past events are known to have already existed and not to be affected or changed by present events.[104] We are justified in believing in inductive reasoning because it usually works; we are usually accurate, Ayer states, in describing our observations.[105] In defending induction, Ayer does not wish to diminish the importance of deduction, but rather to reinstate induction as a valid means of acquiring knowledge and to identify the logical limits of our dependence upon rationality. He states:

> Of course, the fact that a certain form of procedure has always been successful in practice affords no logical guarantee that it will continue to be so. But then it is a mistake to demand a guarantee where it is logically impossible to obtain one. This does not mean that it is irrational to expect future experience to conform to the past. For when we come to define 'rationality', we shall find that for us 'being rational' entails being guided in a particular fashion by past experience.[106]

Ayer thus concludes that "success in practice" is sufficient justification to trust inductive reasoning.[107]

Popper's discussion of empirical testing raises conflicting issues. On the one hand, as argued by Ayer, Popper insists on the inconclusive character of testing but does not, however, rely enough on empirical evidence and, consequently, brings the specter of scepticism into epistemology. On the other hand, his principle of falsifiability and the stress given to falsification merited him the epithet of 'positivist' because it is here understood that Popper relies too heavily on empirical evidence. Could Popper be a sceptical positivist? In contrast with positivists, for whom observation *verifies* statements, Popper writes that the

> ...basic statements are not justifiable by our immediate experiences, but are, from the logical point of view, accepted by an act, by a free decision...- a decision reached in accordance with a procedure governed by rules. [108]

The important role of the empirical evidence is recognised by Popper:

> Only 'experience' can help us to make up our minds about the truth or falsity of factual statements. [109]

But, its role is limited to that of a guide:

> Experiences can *motivate a decision*, and hence an acceptance or a rejection of a statement, but a basic statement cannot be *justified* by them - no more than by thumping the table.[110]

Popper insists that agreement among scientists is the last word on the assessment of a theory, but not in a conventionalist sense. It is meant to stress the non-authoritative character of empirical evidence: observation does not provide certainty to knowledge. What is important is that knowledge is first tested by observation and, then, scientists evaluate the results of the test. Popper's reconstruction of scientific growth is based on logic, rationality, and criticism, that is on World three. And it is because of these three elements that science can be considered objective and, therefore, belongs to World three. The principle of falsifiability is a principle of testability. This requirement of a possibility of contact with World one, the physical world, can be seen as an advance in the philosoply of science as to the identification of scientific knowledge. Falsifiability gives science a means of assessing its propositions: to have scientific value, theories have to propose predictions that can be confirmable or falsifiable. This (at least potential) contact with the physical world demarcates science. If Popper agreed that empirical tests verified knowledge, then World one would prevail over World three, and he could rightly be considered a positivist. But he does not. The physical world serves as a support to human knowledge. The physical world does not provide knowledge; it tests propositions that will become knowledge if they are corroborated. The importance given to falsification is not a positivist stance because falsification has to be understood as the dynamic of scientific growth within Popper's logical reconstruction of that growth.

* * *

A scientific theory, Popper states, is not held because it is true. We never know when it is true; we only know, sometimes, that it is false. Even falsification is inconclusive, as the future may show some errors in the testing procedures and reinstate a previously falsified theory.[111] The aim of science is search for truth, Popper reminds us, but the word "truth" is subject to certain restrictions. Absolute truth is unattainabale. The only kind of truth accessible to us is an approximation to truth. This means that we cannot do otherwise than to accept that our theories contain (unrecognised) false statements and that new theories can only be better, closer to truth than previous ones. We can never say that a theory is true. Popper states:

> ...we *cannot* ever have sufficiently good arguments in the empirical sciences for claiming that we have actually reached the truth, we *can* have strong and reasonably good arguments for claiming that we may have made progress towards the truth; that is, that the theory T_2 is preferable to its predecessor T_1, at least in the light of all known rational arguments.[112]

Consequently, we cannot have a criterion of truth.

Ayer disagrees again with Popper. In his defence against scepticism, Ayer argues for the necessity of a criterion of truth. His early strong position on certainty alters in his later writings.[113] He still argues for 'verification', but the term no longer implies certainty for the future. He asserts that a general criterion of truth is possible only if the truth of basic statements does not have to be guaranteed for the future -

a condition that he accepts. He states:

> Of course, to the extent that an hypothesis is open to further tests, there can be no question of our having any guarantee of its truth; but it is now generally admitted that no such guarantee could be forthcoming.[114]

Ayer strives to salvage his criterion of truth by arguing for what can be seen as an 'immediate' truth. A general criterion of truth is thus replaced by a criterion of empirical truth which stipulates that basic statements can be considered true in the absence of counterexamples, but with no implication for the future. He writes:

> ...there seems to be no good reason why we should not regard our experiences as directly justifying, not only sense-data statements, but the sort of statements which Popper treats as basic. We cannot hold that they verify them conclusively; but this is not a bar to our holding that they give us an adequate ground for accepting them.[115]

Here, Ayer's thinking obviously converges with that of Popper as he clearly renounces his claim for conclusive verification.[116] He does not, however, address the question posed by Popper that such a criterion of truth could not be applied in the case of two incompatible but unrefuted hypotheses - both theories could not be true.[117] Popper's theory could allow for a solution in such a case as, with critical rationalism, the scientific community 'decides' which hypothesis is better, that is theoretically closer to truth and representing a greater achievement for science.

Inspired by Alfred Tarski's correpondence theory of truth, Popper proposes a criterion of progress towards truth, which he presents as a "regulative principle". A scientific theory is held not only because it has resisted tests, but also because it is "better" than another theory in solving a specific problem. The explanations offered by this new theory are more complete and more satisfactory.[118] The idea of truth refers to the "objective truth in the sense of correspondence to the facts."[119] Better theories are therefore theories "nearer than others to the truth - which correspond better to the facts".[120] Critical rationalism is the means for getting nearer to the truth by always criticising theories in order to eliminate their mistakes.[121] The notion of approximation to the truth is called "the notion of verisimilitude" by Popper. It is a logical construction composed of two notions; the notion of truth and the notion of the logical content of a statement. The logical content of every statement consists of all the statements that can be deduced from the original. The class of *all* and *only* true statements is a sub-content and represents the *truth content*. The class of only false statements represents the *falsity content*. True statements that are deduced from false statements are part of the truth content.[122] Popper first defined verisimilitude as follows:

> ...the *verisimilitude* of a statement will be explained as *increasing with its truth content* and *decreasing with its falsity content*.[123]

But he subsequently corrected his definition, in part because he recognised that verisimilitude cannot be satisfactorily explained only in terms of logic. The new description states that a theory is closer to the truth if and only if its truth content is higher than that of a competing theory and if this new theory corrects, at least in part, the falsity content of the other theory.[124]

From a logical point of view, a theory with greater content and, therefore, more explanatory power would have more verisimilitude, even before any tests have been performed. This is true as long as the falsity content of the theory is not greater than that of the competing theory. Verisimilitude is not an epistemological concept for Popper in the sense that verisimilitude does not serve to identify true statements. According to Popper, it is impossible to have a criterion of truth. Verisimilitude is thus a logical explanation of what we *mean* when we say that one theory is closer to the truth than another. In its application, verisimilitude is limited to theories that can be compared, and more specifically to competing theories. Verisimilitude is not itself measurable, but Popper does endeavour to find a way to estimate the truth and falsity contents of theories. Verisimilitude follows the logic of critical rationalism which seeks better theories by trying to identify false statements.[125] Popper states:

> This assertion forms the logical basis of the method of science - the method of bold conjectures and of attempted refutations. A theory is the bolder the greater its content. It is also the riskier: it is the more probable to start with that it will be false. We try to find its weak points, to refute it. If we fail to refute it, or if the refutations we find are at the same time also refutations of the weaker theory which was its predecessor, then we have reason to suspect, or to conjecture, that the stronger theory has no greater falsity content than its weaker predecessor, and, therefore, that it has the greater degree of verisimilitude.[126]

We do not assess verisimilitude. We guess it, as we never know for sure that we have attained the truth.[127] Indeed, Popper asks:

> How do you know that the theory$_2$ has a higher degree of verisimilitude than the theory$_1$?[128]

He answers:

> I do *not* know - I only guess.[129]

Yet, some logical aspects of a theory do serve to indicate greater verisimilitude. Popper lists six of them.

a. T_2 makes more precise assertions stand up to more precise tests.

b. T_2 takes account of, and explains, more facts than T_1.

c. T_2 describes, or explains, the facts in more detail than T_1.

d. T_2 has passed tests which T_1 has failed to pass.

e. T_2 has suggested new experimental tests, not considered before T_2 was designed; and T_2 has passed these tests.

f. T_2 has unified or connected various hitherto unrelated problems.[130]

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When then, can we say that a theory is closer to the truth?

First of all, a theory must meet certain requirements to be considered scientific. A scientific theory aims at solving a problem. That is where the rationality of a theory lies. Consequently, the first step consists in evaluating the extent to which the theory serves the purpose for which it was designed.[131] Not only does a theory aim at explaining, but a better theory has the greatest explanatory power. This means that the explanations are given with more precision because it is not only "truth" that science seeks but "interesting truth".[132] This is achieved by the rich empirical content of a theory. Science wants this high degree of explanation because it wants to give significant answers to the problems it faces. Popper writes:

> For our aim as scientists is to discover the truth about our problem; and we must look at our theories as serious attempts to find the truth. If they are not true, they may be, admittedly, important stepping stones towards the truth, instruments for further discoveries. But this does not mean that we can ever be content to look at them as being *nothing but* stepping stones, *nothing but* instruments:... Thus we ought not to aim at theories which are mere instruments for the exploration of facts, but we ought to try to find genuine explanatory theories.[133]

A scientific theory also provides predictions, usually in specifying what cannot happen. This is so because

> ...a theory tells us the more about observable facts the more such facts it forbids.[134]

It is because of this richness of empirical content that the best theories are the least probable, as they are at great risk of being contradicted by facts.[135] A good theory is thus always risky. Finally, a scientific theory must be falsifiable. Falsifiability does not require immediate testing; technology may not be advanced enough at the time of the creation of a theory to empirically test it. But the possibility of testing a theory, at least potentially, is an essential aspect of science. However, a theory has to be immediately criticisable, subject to probable refuting arguments. Popper states:

> ...what cannot (at present) in principle be overthrown by criticism is (at present) unworthy of being seriously considered; while what can in principle be so overthrown and yet resists all our critical efforts to do so may quite possibly be false, but is at any rate not unworthy of being seriously considered and perhaps even of being believed - though only tentatively.[136]

Falsification identifies the non-correspondence with the facts. From a logical point of view falsification is stronger than corroboration. Corroboration "says nothing whatever about future performance, or about the 'reliability' of a theory",[137] but a falsification may clearly show the weakeness of a theory. However, at the practical level, falsification is no more secure than corroboration because there is always the possibility of future experiments contradicting the outcome of previous tests. "[B]ut since our quest is not for certainty, this does not matter", Popper tells us.[138] Science proceeds by bold conjectures: "all our theories remain guesses, conjectures, hypotheses".[139] Popper continues:

> Although we have no criterion of truth, and no means of being even quite sure of the falsity of a theory, it is easier to find out that a theory is false than to find out that it is true... We have even good reasons to think that most of our theories - even our best theories - are, strictly speaking, false; for they oversimplify or idealize the facts.[140]

Does this conclusion lead to radical scepticism? Not for Popper. He does not propose any specific criterion for the choice of a better theory because this choice is always unpredictable. Yet, the notion of verisimilitude coupled with the characteristics of what constitutes a scientific theory, do serve as a guideline for choosing. Here again, the aim is not to find an absolutely true theory, but to try to identify and to eliminate the false theories. Popper states:

> ...science has nothing to do with the quest for certainty or probability or reliability. We are not interested in establishing scientific theories as secure, or certain, or probable. Conscious of our fallibility we are only interested in criticizing them and testing them, in the hope of finding out where we are mistaken; of learning from our mistakes; and, if we are lucky, of proceeding to better theories.[141]

Popper refers to a theoretical preference for the choice of a better theory. It is based on agreement between scientists as to the result of tests the theory has been submitted to. The theory that resists the most rigourous tests "proves itself the fittest to survive".[142] From a theoretical point of view, unrefuted theories are preferred because "some of them *may* be true."[143] Theories are evaluated in comparison with other competing theories. A better theory will explain the successes and failures of a previous theory, as a new theory contains the previous one in approximation. A better theory also has to have "very high degrees of boldness" [144] if it is not only to explain but also to correct a previous theory. A theory is risky because "we do not wish to learn only that all tables are tables",[145] Popper writes. We want to acquire interesting and challenging knowledge. He states:

> ...we are not simply looking for truth, we are after interesting and enlightening truth, after theories which offer solutions to interesting *problems*. If at all possible, we are after deep theories.[146]

A better theory is thus not only the best testable but also the boldest, the best tested and, it is to be hoped, the best corroborated. It is a rational choice.[147] In this way, a scientific theory can engender the most unexpected problems, and, therefore, contribute to the growth of knowledge.

It is often argued that Popper's emphasis on falsification and on the inconclusive character of testing confer a negative, and even destructive, view of science. In that approach, scientists ought to distrust all theories and have as their only goal the falsification of theories. This extreme interpretation of Popper's views misunderstands Popper's endeavour to describe the logic of the process of scientific growth. Popper does not mean that scientists create theories only in order to reject them but, rather, that it is because scientists critically assess any scientific propositions that scientific growth occurs. Consequently, the theories that survive scrutiny can be trusted. It is not just any propositions that are accepted by scientists, but only those that convey the character of objective knowledge. Growth of scientific knowledge, with Popper, does not mean only the accumulation of more knowledge, but an increase of better, truer knowledge. Science grows in quality, not only in quantity. Consequently, science has no reason to be immobilised by a spirit of scepticism that would make all knowledge claims so doubtful that the effort to discover more would seem useless. Popper's essential message is that science grows by replacing false knowledge with truer knowledge and that while scientific knowledge can be trusted because of objective means of acquisition, it cannot have a dogmatic authority since there is no rational justification for dogmatism. Popper argues that progress in science can be explained in terms of logic because science is a rational process. It is from a rational point of view that results of tests are inconclusive and that scientists accept or reject the relevance of a test.

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Yet Popper's concept of verisimilitude raises some questions. According to his theory, hypotheses with higher falsity content are theoretically discarded. When two hypotheses are in competition, it is the hypothesis closer to the truth that is retained. In that sense, falsification does lead towards truth. There is some ambiguity in Popper's theory here. First, a falsified theory is not necessarily rejected[148] but, if it is, it is not clear *at what point* a falsified theory is actually rejected. Secondly, the concept of verisimilitude, as defined by Popper, is applicable only to the comparison of *two* theories. Verisimilitude is useless in the case of an isolated theory.

This point is important because it questions the justification of falsification as a criterion towards progress. If falsified theories continue to be used, then more specific methodological procedures are necessary to determine when a falsified theory is to be rejected. Progress occurs only insofar as refuted theories are in fact rejected. Yet, Popper cannot specify under what conditions rejection occurs. It might be argued that Popper implies that a theory is rejected when a better candidate is presented. Popper remains ambiguous on this point.[149] On the other hand, it could also be argued that, even if falsified, a theory may have a higher degree of truth content, and a lesser degree of falsity content than another theory, but with a degree of falsity content high enough to be refutable. Popper may have something like that in mind when he remarks that:

> I think it is important to make clear in what sense Einstein, who to the end of his life remained convinced that General Relativity was not true, claimed that this theory was 'better' than Newton's: that it was a better approximation to truth.[150]

It is noteworthy that verisimilitude expresses an *approximation* to truth, and not truth. This may explain why the degree of falsity content of refuted but still used hypotheses may sometimes be considerable.

A misunderstanding on the part of Ayer makes him reject Popper's concept of verisimilitude as useless. Ayer mistakenly assumes that verisimilitude serves to assess progress towards truth. Ayer confuses Popper's concept of what a better hypothesis means with a criterion of progress towards truth. For Ayer, truth is assured by the verification of hypotheses through observation.[151] Ayer seeks truth through confirmation, whereas Popper refers to an approximation to truth through refutation. For Ayer, it is through the discovery of true hypotheses that progress occurs, while, for Popper, it is through identifying false hypotheses. Popper argues for falsification because it is rationally justifiable to infer the falsity of an hypothesis from counterinstances.[152] Ayer argues for inconclusive 'verification' that asserts an 'immediate truth' because it would be irrational not to trust empirical evidence.

Both Ayer and Popper agree on the provisional nature of knowledge. Ayer comes to agree with Popper on the inconclusive nature of both falsification and corroboration. The fundamental difference between them is that Popper seems more consistent as he tends (although still giving more weight to falsified hypotheses) to accept the rational limitations imposed by the logical asymmetry of the relation between singular and universal hypotheses. Ayer recognises the problem of the asymmetry of universal hypotheses, but still maintains that we are entitled to rely on our experience when the strict application of rationality is too restraining. We can thus trust the future to be similar to the past because it is usually the case. The evidence given by the confirmation of a singular event can, therefore, logically be considered truthful, even if this judgment is not rationally grounded. Nevertheless, the primordial role Ayer attaches to experience cannot be reconciled with Popper's conviction that it is the decision of scientists that prevails. For Ayer, any means of validating an hypothesis other than observation is arbitrary.[153] Popper, for his part, cannot accept Ayer's principle of verification, even if 'inconclusive', because it associates knowledge with its empirical source. For Popper, the means of furthering progress towards truth is the testing of knowledge, with the "help" of experience. Falsification is essential but not wholly decisive. Ayer's concentration on sense-experience does not involve a discussion of the participation of the scientific community in the choice of hypotheses. It is not that he rejects participation as irrelevant, but he rather ignores the issue. He only alludes to the role of scientists in discussing the choice of basic statements. He agrees with Popper that there comes a time when basic statements are considered sufficiently tested so that

> ... the process may continue until we reach a statement which we are willing to accept without a further reason... It is just that at a certain point we decide that no further reason is required.[154]

In his plea for reliance on sense-experience, Ayer ignores other factors influencing observation, such as the expectations of the observers or simple technical or procedural mistakes. Popper does not. While Popper argues as strongly as Ayer against scepticism, he never forsakes the fundamental principle underlying his theory that as along as we do not know whether our hypotheses are true or not, we are not entitled to say that they are. For Popper, to doubt our knowledge is a protection against a greater danger than scepticism. Indeed, overconfidence in our knowledge endangers progress towards truth, since to believe our hypotheses true would make the questioning of knowledge pointless, and progress would cease. That is why, for Popper, epistemology ought not to seek to ground knowledge upon secure foundations; rather, it should articulate a method for evaluating and testing claims to knowledge.

NOTES

1. Popper, 1981, p.71.

2. 1968a, pp.120-153.

3. Ibid., p.215.

4. Ibid., p.216.

5. Ibid., p.226.

6. Ibid., pp.152-153.

7. Ibid., p.53.

8. Ibid., p.222.

9. 1981, p.71.

10. 1968a, p.217.

11. 1981, p.261.

12. Ibid., p.262.

13. Ibid., pp.256-265.

14. 1962, p.285.

15. 1968b, p.281.

16. 1981, p.191.

17. Ibid.

18. Ibid.

19. Ibid., pp.191-197.

20. Ibid., p.193.

21. Ibid., p.349.

22. Ibid., pp.349-351.

23. Ibid., note 9, p.356.

24. Ibid., pp.352-357. This last point will be developed later in the discussion of the concept of falsification.

25. 1968b, p.317.

26. 1968a, pp.114-115.

27. 1968b, p.111.

28. Ibid., pp.39-40.

29. Ibid., pp.35-42.

30. Ibid., p.42.

31. Ibid., p.86.

32. 1968a, p.39.

33. Ibid., pp.25-27.

34. Ibid., pp.54-55.

35. 1968b, p.44.

36. Ibid., pp.43-47 and 98-111.

37. 1981, p.299.

38. 1968b, pp.296-301.

39. 1981, p.109.

40. Ibid., pp.73-74 and 106-150. The last world referred to by Popper, World one, represents the physical world.

41. 1968b, pp.81-83.

42. 1974a, p.986.

43. 1968b, pp.78-84.

44. Ibid., p.79.

45. Ibid.

46. 1974b, pp.1110-1111.

47. 1968b, pp.106-111.

48. Ibid., p.54.

49. Ibid., pp.53-56.

50. Ibid., pp.108-109.

51. Ibid.

52. 1974b, pp.1110-1111.

53. 1968b, p.70.

54. Ibid., pp.62-70.

55. Ibid., p.43.

56. Ibid., p.60.

57. Ibid.

58. Ibid., pp.59-62.

59. Ibid., p.103.

60. Ibid., pp.100-103.

61. Ibid., p.86.

62. Ibid., pp.112-121.

63. 1968a, pp.242-244.

64. It is surprising that Popper uses the word "verifications" which he always attaches to the positivists' concept of verification. But as Popper dislikes quarrels about words, this use has probably to be considered a slip of the pen and should be understood as meaning "corroboration" or "confirmation".

65. 1968a, p.244.

66. Ibid., p.58.

67. 1968b, p.251.

68. Ibid., p.267.

69. Ibid., p.275.

70. Ibid., pp.251-276.

71. Ibid., p.104.

72. 1968a, p.313.

73. Ibid.

74. Ibid., p.56.

75. 1968b, p.72.

76. In his early writings, Popper insists more on the rejection of theories. Later (and more clearly in his article "Ayer on Empiricism and Against Verisimilitude", 1974b), he refers more often to the idea that a false theory is rejected when a better one is discovered. In fact, this idea is assumed in the discussion of verisimilitude. Verisimilitude refers to theories in competition, compared to each other. An isolated theory has no verisimilitude.

77. 1968b, p.32.

78. 1968a, p.387.

79. Ibid., p.46.

80. 1981, pp.60-73.

81. Ibid., p.342.

82. Ibid., pp.342-347.

83. 1968a, pp.110-111.

84. Ayer, 1946, pp.136-138. This discussion refers to Ayer's early position which was inspired by the logical positivists of the Vienna Circle. Ayer then referred to himself as a logical empiricist. However, it will later be shown that a shift in the direction of his thoughts concerning the authority of the empirical basis of knowledge can be detected as early as in *The Foundations of Empirical Knowledge* (1963) and more clearly in *The Problem of Knowledge* (1965). In later writings ("Truth, Verification and Verisimilitude", 1967), Ayer admits, although with some reserve, that the knowledge acquired by observation is "inconclusive".

85. For Ayer, there are only two types of propositions that can be considered meaningful, that is that they have sense-content. Generally, empirical propositions belong to one of these, the synthetic propositions, and are considered empirical hypotheses. The negation of a synthetic proposition makes it false. So, synthetic propositions are not absolutely certain. Their truth can be

tested by observation, but their confirmation only inconclusively increased their probability and, consequently, their reliability.(Ayer, 1946, pp.90-100.)

86. Ayer, 1936, pp.228-229.

87. Analytic propositions are the other type of meaningful propositions identified by Ayer. (See note 85.) Analytic propositions are tautologies. They are a priori propositions because they have no factual content; they take the form of definitions. Because of their nature, they are necessarily true and are the only propositions to be absolutely certain. Their negation would be contradictory.(Ayer, 1946, pp. 78-93.)

88. Ayer, 1946, p.10.

89. Ibid., pp.10-13.

90. 1936, p.228.

91. 1963, p.83.

92. 1936, pp.235-243.

93. 1946, p.10.

94. For Popper, "what really exists" is exactly what is at stake. When a theory is tested by observation, the problem is not to describe a perception accurately, but to be able to rely on it. Because the fallibility of human senses does

not allow a certain reliance, the results of testing remain inconclusive. Even more, Popper rejects any attempts to reduce knowledge or truth to sense experience.(Popper, 1974b, pp.1113-1114.)

95. Ayer, 1963, pp.21-30.

96. 1965, pp.56, 71-73.

97. Ibid., p.253.

98. Ibid., pp.184-185.

99. 1963, pp.109-112.

100. Popper, 1968a, pp.42-53.

101. 1981, pp.3-13.

102. Hume, 1955, pp.54-68.

103. Ayer, 1946, pp.49-55.

104. 1965, pp.192-198.

105. Ibid., pp.72-73.

106. 1946, p.50.

107. Ibid., pp.49-55.

108. Popper, 1968b, p.109.

109. 1981, p.12.

110. 1968b, p.105.

111. Ibid., p.50.

112. 1981, pp. 57-58.

113. Ayer, 1967.

114. 1967, p.686.

115. Ibid., p.689.

116. Ibid., pp.686-687.

117. Popper, 1974b, pp.1105-1108.

118. 1968a, p.226.

119. Ibid., p.27.

120. Ibid., p.226.

121. Ibid., pp.224-231.

122. Ibid., p.233 and pp.392-393.

123. 1981, p.48.

124. Ibid., pp.371-372.

125. Ibid., pp.52-58.

126. Ibid., p.53.

127. 1974b, pp.1101-1103 and 1981, pp.47-52.

128. 1968a, p.234.

129. Ibid.

130. Ibid., p.232.

131. 1981, p.288.

132. 1968a, p.229.

133. Ibid., p.245.

134. Ibid., p.385.

135. Ibid.

136. Ibid., p.228.

137. 1981, p.108.

138. 1968a, p.238.

139. 1981, p.13.

140. Ibid., p.318.

141. 1968a, p.229.

142. 1968b, p.108.

143. 1981, p.14.

144. Ibid., p.16.

145. Ibid., p.55.

146. Ibid.

147. Ibid., pp.13-23.

148. 1968a, p.56.

149. See note 76.

150. 1974b, p.1101.

151. Ayer, 1967, pp.690-691.

152. Popper, 1974e, pp.1020-1021.

153. Ayer, 1967, p.687.

154. 1965, p.74.

CHAPTER II

THE SOCIAL CONTEXT OF SCIENCE: T. KUHN

Thomas S. Kuhn's theory sets out to demonstrate that the greater part of scientific practice is incompatible with the critical rationalism postulated by Popper. Falsification and rejection of theories are, for Kuhn, two distinct processes not directly responsible for scientific progress. The fundamental difference, in Kuhn's view, is that, critical rationalism, if given too great an importance, would prevent science from progressing. The relative absence of criticism allows scientific research to expand and, in doing so, to reach a point where new discoveries are inevitable.

Scientific progress is portrayed by Kuhn as a cycle consisting of a long period of 'normal science' followed by a period of interruptive revolution and, then, a return to a long period of 'normal science'. The role and nature Kuhn assigns to 'normal science' determine the basis for the logic of scientific discovery. 'Normal science' refers to the daily activity of scientists and is characterised by stability, commitment to views generally accepted, and work aimed at expanding the application of theories that are not questioned. Kuhn advances the notion that because 'normal science' describes what scientists do most of the time, it also describes science.[1] The problems scientists deal with in 'normal science' are identified by a framework that defines which problems are worth scientific attention, i.e., those that are solvable, as it is only within the framework that solutions can be found.[2] The solution of such problems is not considered to be the discovery of something new, nor is the solution necessarily considered interesting or important since it has been predicted by recognised theories.[3] Kuhn calls these problems "puzzles" because (like ordinary puzzles) their resolution tests the skill and ingenuity of scientists but does not test a theory. If scientists fail to find a solution, the failure is attributed to their lack of ability to apply a theory. The theory, like a puzzle, is not considered defective.[4] On the other hand, if solutions cannot be foreseen for some problems, these problems are not considered puzzles. At least for the time being, they are rejected, ignored, or considered metaphysical.[5] 'Normal science' is essentially a puzzle-solving activity. It is not in its nature to challenge theories.[6] The aim is

> ...the steady extension of the scope and precision of scientific knowledge.[7]

Activities of 'normal science' are based on knowledge which is considered secure. Scientists work with, and extend, the application of theories that have been proven reliable. In Kuhn's words:

> ...'normal science' means research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice.[8]

For Kuhn, the primary aspect of scientific activity is stability. Revolutions do occur in science, but they are exceptional occurrences. Kuhn's concept of paradigm explains what the nature of scientific activity is and why scientific theories are not ordinarily challenged. Kuhn's articulation of 'normal science' in *The Structure of Scientific Revolutions* has, however, engendered much confusion due to his use of the term "paradigm" to refer to distinctly different concepts. He subsequently modified his terminology.[9]

Taken in a general sense, paradigms define science: they determine both scientific behavior and the nature of scientific activity. Paradigms constitute the source of strong commitment on the part of the members of a scientific community and, consequently, the very existence of the community.[10] Each area of science has its own paradigm and some paradigms are shared by more than one area.[11] Paradigms refer to what is taken for granted during ordinary scientific activities.[12] They are both restrictive and productive. They are restrictive because they identify the puzzles science is to be concerned with, and the procedures and rules to be followed both in the way to approach a puzzle and in the way to look for its solution.[13] In that sense, paradigms define science in identifying the instrumental and methodological aspects of research (that is, its legitimate laws and explanations) and its metaphysical views (that is, what the universe is constituted of or what it is not).[14] Paradigms are also productive because, by confining scientific activity within strictly defined limits, they force scientists to concentrate their efforts on specific problems. As a consequence, puzzles are studied in a depth and detail that could not be achieved without the restrictions. As will be seen later, it is this deepening of problems during 'normal science', (referred to by Kuhn as 'articulation of a paradigm'), that leads to new discoveries during the revolutionary period.[15]

In a more specific sense, Kuhn identifies two categories of paradigms. In its more sociological aspect[16] a paradigm can be viewed as a set of

> ...beliefs, values, techniques, and so on shared by the members of a given community.[17]

It represents what Kuhn identifies in other writings as "the disciplinary matrix". 'Disciplinary', Kuhn specifies, because "it is common to the practice of a specified discipline". 'Matrix', because "it consists of ordered elements which require individual specification."[18] More specifically, the disciplinary matrix refers to

> ...shared elements [that] account for the relatively unproblematic character of professional communication and for the relative unanimity of professional judgment.[19]

The disciplinary matrix encompasses the objects of the shared commitment of scientists. The objects may be theories expressed in symbolic generalizations (i.e., unquestioned symbolic form or axioms), heuristic models (such as analogies), beliefs (which are the metaphysical parts of a paradigm, such as "heat is the kinetic energy of the constituent parts of bodies"),[20] values (such as the accuracy of prediction) which guide the evaluation of whole theories, and, finally, exemplars (standard examples of solved problems).[21] The commitment made possible by the disciplinary matrix enables scientists to indulge in puzzle-solving.[22] The disciplinary matrix provides the "rules of the game" which scientists must respect in their choice of solutions and the means of obtaining them. These rules are not written rules but "established viewpoints" or "preconceptions"[23] that identify the puzzles. Through the disciplinary matrix scientists are confined to a restrictive framework that is conceptual, theoretical, and instrumental as well as methodological.[24] In 'normal science', rules prescribed by the matrix are not always identifiable. They often consist of knowledge scientists have acquired during their formal training by applying solutions to classical problems in science. Those rules have become 'tacit knowledge' because scientists now apply them in their daily activities without consciously realising it. Scientists often rely on that background knowledge to evaluate puzzles and their solutions.[25] Taken in its entirety, the disciplinary matrix comprises all the shared commitments of the scientific community.

Exemplars are the other, and in fact the more important, category of paradigms.[26] They are:

> ...concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.[27]

Exemplar-paradigms may be theories used as examples of solved problems, such as those problems found at the end of chapters in science textbooks and on which science students are tested.[28] They are "shared examples of successful practice" within a scientific community and are, therefore, essential to the conduct of research that Kuhn considers generally unproblematic.[29] In that sense, exemplar-paradigms represent a subset of the disciplinary matrix because they refer to a particular kind of commitment.[30] Indeed, as all scientists learn the same 'problem-solutions' within a given field, the exemplar-paradigms serve to reinforce the fundamental tenets of that field. As a consequence, Kuhn asserts that they create and maintain a general consensus *essential* to 'normal science'.

In Kuhn's description of paradigms, one idea is paramount: paradigms are the source of, and explain, the general agreement among scientists practising 'normal science'. This agreement is fundamental to Kuhn's 'logic' of science. It is the *submission* of scientists to the restrictions of their paradigm that permits 'normal science' to progress rapidly. The submission is based on trust because it is taken for granted that the paradigm can solve puzzles. Indeed, that was the reason for its acceptance in the first place.[31] Consequently, inexplicable phenomena or counterinstances are ignored because they are not consistent with the predictions of the theory-paradigm. In fact, they are often not perceived for what they are.[32] That is to say, 'normal science' is not a time for novelty nor for controversy. It is characterised by rather untroubled and 'routine'-activities - 'routine' since scientists do not normally engage in innovative projects. Scientists develop instruments, vocabulary, skill and concepts permitted by the paradigm and necessary to its application.[33] This concentrated effort serves, ultimately, to enable the paradigm to reach the outermost limits of its possibilities and, by doing so, engender a crisis. Then, and only then, will scientists have grounds for challenging the worth of their paradigm.[34]

* * *

In contrast to the stability of 'normal science', scientific crisis is a time of turmoil and uncertainty. It arises because the possibilities of the present paradigm are exhausted. Puzzle-solving activities are disrupted, as it is more and more obvious that the paradigm cannot accommodate certain anomalies. The trust which scientists placed in their paradigm is shaken and they are forced to question its fundamental tenets. Anomalies, often known for a long time, can no longer be ignored. They are now looked upon as new puzzles unsolvable by the present paradigm. Scientific research takes new directions in its search for a more effective tool for solving puzzles. Then, science becomes revolutionary. It now resembles science as described by Popper, in the sense that new theories are proposed and severely tested. The general consensus breaks up and gives way to doubt and questioning. New competing theories challenge the paradigm. Paradigmatic elaborations as well as fundamental theories are revised. This revolutionary period leads to the emergence of a new theory-paradigm. It takes a long time for a new paradigm to create a new consensus. A theory-paradigm is first accepted because it can solve the anomalies of the challenged paradigm and is potentially able to account for new questions raised with the crisis. The new theory-paradigm gains popularity because it appears that it "fits the facts *better*"[35] than other competitive theories.[36] It is not that it solves all problems immediately but, rather, Kuhn remarks, that it is perceived as

> ...a promise of success discoverable in selected and still incomplete examples.[37]

The following period of 'normal science' will then realize these promises with a new viewpoint, new method and new direction in research.

* * *

Contrary to Popper's theory which assigns a primary role to rational criticism in science, Kuhn denies its relevance, except during the rare periods of revolution. This clash between the views of Kuhn and Popper is to be understood through their respective conception of 'scientific progress'. First, it has to be remembered that, according to Popper, science progresses towards truth. Verisimilitude stipulates that, as a theory that replaces a previous one is a better approximation to truth,[38] then with each new theory science gets closer to the truth. Truth is the ultimate goal, but we do not need to know what it is to move towards it. Kuhn rejects this approach because, he argues, the concept of truth implies that we are in a position to know that theories are "statements about what is really out there."[39] Kuhn argues, however, that there is no way of knowing this. So, if one does not know what truth is, not only can one not talk about getting closer to it, but neither can one define progress (if progress means to get closer to truth).[40] We can say that science grows because more and more theories are produced and articulated, and because there are more discoveries about nature.

Kuhn's concept of progress in science, however, is not very clear and may sometimes be inconsistent. Kuhn tends to prefer the word "development" to refer to the growth of science in general, and to reserve the word "progress" for 'normal science'.[41] However, he still refers often to progress of science. 'Development' more accurately describes Kuhn's views because it takes into account his evolutionary approach.

Kuhn argues that, in order to be functional, scientific textbooks create the illusion that science progresses in the sense of cumulative knowledge. In science, history is written backward. Textbooks select only the scientific achievements necessary to illustrate exemplars and often distort them. As a result, science is wrongly pictured as a continual effort to solve the same kind of problems within a similar framework.[42] In fact, each new paradigm replaces the previous one in its new view of the world, its definition of problems and its instruments. Yet, revolutions are not identified in science textbooks. Consequently, one cannot identify past paradigms shift. That is why development in science is perceived as linear and cumulative.[43] For Kuhn, the shift of direction in the investigations that accompanies a new paradigm makes scientific development a process similar to Darwinian evolution. There is an increase in specialization and the change is irreversible, but "there is no ontological process".[44] Proliferation of theories does not equate with accumulation of knowledge. Kuhn states that

> Unlike discoveries of new natural phenomena, innovations in scientific theory are not simply additions to the sum of what is already known... [T]he acceptance of a new theory demands the rejection of an older one. In the realm of theory, innovation is thus necessarily destructive as well as constructive.[45]

The fact that new knowledge replaces old knowledge, (and not ignorance),[46] makes Kuhn wonder about progress. He asks:

> Is it not possible, or perhaps even likely, that contemporary scientists know less of what there is to know about their world than the scientists of the eighteenth century knew of theirs?[47]

Despite all this uncertainty about the nature and even the existence of progress in science, Kuhn still uses a concept of progress as the criterion of demarcation between science and what is not science. It has to be remarked that Kuhn's definition of science applies only to the natural sciences.[48] The statement is somewhat circular as, Kuhn admits,

> To a very great extent the term 'science' is reserved for fields that do progress in obvious ways.[49]

These 'ways' are, in fact, those described by Kuhn as defining 'normal science'. His theory of transition from immature to mature science is based on the idea of progress. A science is considered immature when it lacks a puzzle-solving paradigm because this lack curtails its progress. This pre-paradigmatic period is characterised by the confrontation of various schools of thought which causes science to stagnate. Social sciences, for example, are in such a condition. Only a paradigm can ensure the commitment necessary to advance,[50] that is to acquire

> the sort of progress that we now generally refer to when distinguishing the natural sciences from the arts and from most social sciences.[51]

The "sort of progress" refers to the efficient and rapid puzzle-solving of 'normal science' which allows a detailed investigation of problems and an extended articulation of the reigning paradigm.[52] Kuhn does not address the question of the unity of method for all sciences. Yet, his discussion on the need for a puzzle-solving paradigm essential to progress implies that any field of research that possessed such a paradigm would be considered scientific. Clarification of his concept of scientific progress would be necessary better to comprehend the role of puzzle-solving paradigms. In a sense, for Kuhn, scientific progress is the acquisition of new knowledge consequent upon the adoption of new paradigms. In another sense, it may also mean the more profound understanding of a paradigm, acquired in 'normal science' with the progressive articulation of the paradigm. It is not clear, however, whether Kuhn would accept these two definitions of scientific progress as being on a par. Progress remains a blurred concept with Kuhn. For Popper it is unequivocal. Progress is defined through the logic of scientific discoveries, that is in getting closer to truth through the falsification and rejection of theories. Kuhn's and Popper's different approaches to progress touch the central point of the incompatibility of their views.

*   *   *

For Popper, critical rationalism is at the heart of the logic in science. Kuhn argues that criticism is absent from 'normal science' because it is contrary to the very concept of 'normal science'. A new theory-paradigm emerges during the period of crisis. Once accepted, it generates and inspires the work done in 'normal science'. A new theory-paradigm means much more than a single theory. It implies all the changes its acceptance brought on, such as new ways of looking at the same phenomena, new instruments, and new direction of research. When a new theory-paradigm is accepted, the extent of all its possibilities is not yet known. It is still "very limited in scope and precision."[53] The acceptance of a new paradigm (which may be a long process) is only the beginning of a new era of enquiry. 'Normal science' is then occupied in discovering the applications of the new paradigm. Kuhn states that, once a paradigm is accepted,

> Mopping-up operations are what engage most scientists throughout their careers... [T]hat enterprise seems an attempt to force nature into the preformed and relatively inflexible box that the paradigm supplies. No part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all... [N]ormal-scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies.[54]

In 'normal science', scientists are not critical, Kuhn states, in the sense that they do not try to test their paradigm. It is taken for granted and never challenged. The tests undertaken in 'normal science' aim only to discover applications of the paradigm. Tests do not bring into doubt the credibility of the paradigm even if counterinstances are encountered, for the reason that counterinstances are 'expected'. They are considered problems that scientists cannot solve for the time being. Either these problems are put aside for future research, or scientists create ad hoc explanations.[55] It cannot be otherwise as, Kuhn argues, no paradigm can ever explain everything. They are thus *expected* to be incomplete.[56] On the other hand, psychological reasons account, in part, for the 'lack' of concern scientists demonstrate when faced with counterinstances. Because they trust the paradigm to be fundamentally useful, they are more interested in concentrating their efforts on solving the puzzles which are immediately solvable. The rest can wait. In fact, because there is not always a clear distinction between puzzles and counterinstances, Kuhn concludes that

> ...either no scientific theory ever confronts a counterinstance, or all such theories confront counterinstances at all times.[57]

To remain scientists, scientists have to learn to live with counterinstances. It is part of "the 'essential tension' implicit in scientific research."[58]

It appears that Kuhn understands critical rationalism only in terms of the falsification followed by the rejection of a paradigm. His claims that science starts where critical discussion stops does not take into account the scientific aspect of 'normal science'. Indeed, even in 'normal science', scientists must still exercise their judgment and evaluate the implications of tests and experiments they undertake. What is really at stake here is the different views Kuhn and Popper have about theories. A clear distinction of the nature of scientists' activities as respectively described by Kuhn and Popper is not always possible because they are both ambiguous about the exact nature of the theories they refer to (and Popper more so than Kuhn). With Popper, theory or hypothesis sometimes refers to a single theory of lesser importance. This would correspond to Kuhn's theories aiming at solving 'puzzles' during periods of 'normal science'. At other times, Popper refers to major theories (such as that of Newton) that would correspond to Kuhn's theory-paradigm. Popper does not, however, generally specify what kind of theory he has in mind. The distinction is crucial as Kuhn's concept of 'normal science' seems to refer to the creation of less important theories implementing the recognised theory-paradigm, while the period of crisis would refer to major and more consequential theories later developed in 'normal science'. With Kuhn, theory-paradigms refer to an extended definition of paradigm, including all the changes brought on by a new paradigm. The lower level theories are hypotheses characteristic of 'normal science' and of 'normal science' only. They serve to make a paradigm better understood. They complete the theory-paradigm in reinforcing it.[59] When Kuhn claims that there is no place for critical rationalism, he refers specifically to the other type of theory, the theory-paradigms. Theory-paradigms are not submitted to criticism because they are not considered hypotheses, but recognised and *accepted* theories. That is to say that they are not questioned during the practice of 'normal science'. The lower level theories derive from the theory-paradigm. They are its applications. If they withstand tests, it means that a new puzzle has been solved or that a new discovery (predicted by the theory-paradigm) has been made. Lower level theories are, nevertheless, tested, falsified and rejected, following the precepts of critical rationalism, if they do not fit the predictions or the constraints of the paradigm. Kuhn's use of the word 'theories' is confusing. He sometimes uses the word 'paradigm' to refer to a specific theory or to part of a theory that can be a theory-paradigm, a lower level theory or an exemplar. At other times, it implies all the changes and the consequences that are brought on by the acceptance of a new theory-paradigm. When Kuhn says that paradigms are not questioned in 'normal science', he seems to refer to the extended definition of paradigm because it is all these aspects of a theory-paradigm that are being explored and added to during its articulation. It now becomes clearer that, for Kuhn, tests have two different functions. In 'normal science', they serve to apply the theory-paradigm until it can solve no more puzzles. In periods of crisis, they serve to test the competitive theory-paradigms themselves in order to identify which is to become the new paradigm.[60] Scientists' commitment to a paradigm can be compared to Popper's rules of methodology that guide the *decision* of scientists in their choice of a theory.[61] Popper states that the choice is based on theoretical preference:

> ...the convention or decision does not immediately determine our acceptance of *universal* statements but..., on the contrary, it enters into our acceptance of the *singular* statements - that is the basic statements.[62]

Kuhn cannot but agree with this statement. However, his analysis goes further. During the testing of lower level theories, the kind of 'convention' Popper refers to can be observed. The theory-paradigm is the parameter of 'normal science' activities. It is because scientists share the same criteria that they can agree about the rules of procedures and choice. To be evaluated, a theory-paradigm has to have been applied sufficiently to discover which instruments and which tests are necessary to compare the theory with nature.[63] That is what Kuhn means when he says that

> Frameworks must be lived with and explored before they can be broken.[64]

This knowledge is fully acquired only in later stages of the articulation of the theory-paradigm. Typically, measurements correspond with a theory because the instruments are produced by the theory. Kuhn states:

> ...the relevant analytic techniques are based upon the very theory they are said to confirm... They are self-fulfilling prophecies in the physical as well as in the social sciences.[65]

When a theory-paradigm can no longer solve puzzles, the commitment of scientists breaks down and the theory-paradigm is questioned. It gives rise to the emergence of competing would-be paradigms. It is only in such a situation of crisis and proliferation of theories that testing occurs in Popper's sense. Kuhn states that

> In the sciences the testing situation never consists, as puzzle-solving does, simply in the comparison of a single paradigm with nature. Instead, testing occurs as part of the competition between two rival paradigms for the allegiance of the scientific community.[66]

On the other hand, Kuhn argues that even though falsification and rejection may sometimes occur together, they are essentially two independent processes and, consequently, cannot account for progress. Theory-paradigms are not rejected because they have been tested and falsified, but rather because their ability to solve puzzles has been exhausted.[67] On the other hand, falsification does not necessarily lead to rejection, as Popper admits. Furthermore, a new theory-paradigm is not accepted *only* because it has refuted the previous one. In fact, scientists do not "accept" a new theory-paradigm, but are rather progressively convinced of its ability to solve a new category of puzzle. The first encounter with a theory-paradigm is not and, in fact cannot, be convincing for most scientists because it has not had the time to be subjected to experiment and prove itself. Why then are new paradigms "accepted" in the first place? Why are they given the chance to prove themselves if it is not by the evidence of their confirmation?

* * *

During 'normal science' the disciplinary matrix and exemplars account for scientific behavior. Rules of conduct need not (and often cannot) be explicitly articulated because scientists rely on the background knowledge they have acquired during their training. As long as the paradigm is considered efficient in solving puzzles, rules are not necessary.[68] In time of crisis, however, the paradigm no longer ensures consensus. What then guides scientists in their choice of a new paradigm? To that question Kuhn answers that logic and experiment do play an important role, but that they cannot, alone, account for the decision. Psychological reasons are also responsible for scientific judgment.[69] Kuhn argues that epistemology must include in its analysis reference to subjective factors influencing the choices made by scientists if it is to understand

> ...what sort of thing knowledge is, what it is all about, and why it is that it works the way it does.[70]

In other words, scientific knowledge cannot be understood without understanding scientists' behavior. A formalised theory based only on rational conduct would not be adequate. However, to include psychological and sociological factors does not mean scientists are irrational, Kuhn argues. It means that their choice is not guided by rationality alone.[71] It must be remarked that Kuhn restricts this discussion mostly to the situation found in extraordinary science, as it is only there that scientists have to choose between theory-paradigms. In 'normal science', scientists have exemplars to guide them. Exemplars are useful for illustrating theories that are not questioned. They are not useful in times of crisis because, then, a choice has to be made between theories that are not paradigms yet.[72] The choice is always problematic for Kuhn because competing candidate paradigms are not simply right or wrong. To refer only to objective criteria in explaining the choice of theories reduces the analysis to only the positive aspects of an accepted theory. It simplifies the process of choice in ignoring the limitations of the winning theory (despite its acceptance) and the positive aspects of the rejected theory. It is often these very aspects that make the choice so difficult.[73] It is important to remember that, according to Kuhn, the reasons that make scientists reject a no longer acceptable paradigm are different from the reasons that make them not choose a candidate paradigm. Indeed, judging an almost unknown, but promising, theory requires criteria different from the judgment necessary to recognize that a known theory cannot answer some questions. On the other hand, subjectivity in science does not mean that scientists just assert that they like or dislike a theory for personal reasons. It means that they perceive and interpret objective criteria in different ways, and they do so because their personalities and professional backgrounds are different.[74]

The criteria of choice between would-be paradigms listed by Kuhn are similar to Popper's characteristics of a superior theory. A would-be paradigm is judged according to its capacity of being accurate, consistent (within itself and with other accepted theories), simple, fruitful of new research findings, and with a broad scope. These criteria are problematic for various reasons. The importance attached to criteria may change with time, and their applications may vary with time and within different fields. They are often imprecise and are interpreted differently by individual scientists. They may also conflict with one another - the simpler theory is not necessarily the most accurate.[75]

It is because of this ambiguity that Kuhn views criteria as values rather than as rules.[76] Personal preferences for some specific criteria-cum-values do sometimes play a decisive role in individual choice.[77] Whatever the objectivity of the criteria, these discrepancies cannot be avoided because the most decisive confirmations of a theory-paradigm occur only *after* it has been accepted and applied. Confirmations cannot be part of the process of selection as they are produced by 'normal science'. Later, they become those exemplars found at the "end of chapters in science texts".[78] Personal reasons, however, are not the only factors influencing choice. Individual professional experience, as well as social context and non-scientific beliefs, also affect the process. Thus, the choice of a theory-paradigm is influenced by both objective and subjective factors.[79]

Variability in values enables a crisis to emerge and, thus, new theory-paradigms to be produced and accepted. How can this be possible if the paradigm is so restrictive in 'normal science'? Here, sociological reasons are added to the analysis. According to Kuhn, by the time a crisis is recognised, scientists have been aware of problems for some time. Doubts about the paradigm are already present. Some scientists, usually young or new to the field, produce new theories. They can "disobey" their paradigm because, Kuhn explains, they are

> ...little committed by prior practice to the traditional rules of normal science, are particularly likely to see that those rules no longer define a playable game and to conceive another set that can replace them.[80]

Sometimes, the personality or the reputation of an innovator is influential in the recognition of new paradigms.[81]

* * *

A candidate paradigm is not accepted by all scientists when it is first proposed. It may take a long time for the entire scientific community to make its final choice. In fact, Kuhn argues, scientists do not really choose. They are progressively convinced. First, theoretical and experimental work are needed to present a theory sufficiently accurate and with a broad enough scope to be attractive to the scientific community.[82] The theory has to be partly tested to get a chance to become a candidate paradigm. The chances for success of a candidate paradigm are superior if it can solve the anomaly that has given rise to a crisis. Yet, Kuhn argues, this is not always the case and, even when it is, this success is not sufficiently convincing. The ability of a candidate paradigm to solve a large number of puzzles, and to make predictions is a more persuasive argument.[83]

One of the main elements of difficulty in the choice of candidate paradigms is a problem of communication. Candidate paradigms are derived from the old one. They use the "old" terminology but with a different meaning because each theory-paradigm represents a different way of looking at the same phenomena. They refer to different "worlds".[84] For Kuhn, paradigms are essentially incommensurable, albeit not forever incomprehensible. Once a candidate paradigm has attracted the attention of scientists (according to the criteria enunciated above), the scientists have first to be "willing" to be convinced and so accept to expose themselves to the candidate paradigm in order to understand it.[85] This first decision is "made on faith" because they hope the candidate paradigm will be efficient in puzzle-solving.[86] To understand a new theory-paradigm requires an effort of translation as scientists are already "conceptually and perceptually" conditioned by their old paradigm to *see* in a certain way.[87] For Kuhn, perception does not mean interpretation. A paradigm does not make scientists give a different meaning to what they see. It makes them *see differently*.[88] When scientists "accept" the necessity to think in terms of the new theory-paradigm, the change in their views is like a "gestalt shift", Kuhn explains. The new view is incommensurable with the previous one. As long as the shift has not occurred, scientists cannot really understand the arguments put forward by the proponents of a candidate paradigm.[89] The debate is between competing candidate paradigms that are at cross-purposes. That is why Kuhn insists that a

> ...paradigm choice can never be unequivocally settled by logic and experiment alone.[90]

The fundamental problem in the incommensurability of theory-paradigm is that, according to Kuhn, there is no scientific language that provides "the direct matching of whole words or phrases to nature".[91] As long as such a neutral language does not exist, it is impossible to compare the verisimilitude of competing hypotheses, as Popper assumes.[92] There may always be some "stubborn" scientists unwilling to make the shift. Kuhn doubts, however, whether those scientists who remain committed to a rejected paradigm can still be considered scientists.[93]

NOTES

1. Kuhn, 1970b, p.5.

2. Ibid., pp.36-37.

3. More specifically, by the paradigm. This concept will be analysed later.

4. 1965, p.817, note 6.

5. 1970b, pp.36-37.

6. Ibid., p.52.

7. Ibid., p.52.

8. Ibid., p.10.

9. Mainly in "Second Thoughts on Paradigms", pp.24ff, in "Reflections on My Critics", pp.271ff, and in the "Postcript-1969" to *The Structure of Scientific Revolutions*, pp.175, 182ff.
   Distinctions in the evolution of Kuhn's thought will not always be specified in the following analysis of his concept of paradigms.

10. 1974, p.294.

11. 1970b, p.50.

12. Ibid., p.145.

13. Ibid., p.60.

14. Ibid., p.41.

15. Ibid., p.24.

16. Seemingly following the suggestion proposed by Margaret Masterman in "The Nature of a Paradigm" (1970), pp.66-67.

17. Kuhn, 1969, p.175.

18. 1970a, p.271.

19. 1974, p.297.

20. 1969, p.184.

21. 1970a, pp.271-272.

22. 1969, p.271.

23. 1970b, p.39.

24. Ibid., pp.39-41.

25. Ibid., pp.43-47.

26. Kuhn admits in later writings (see note 9) that the concept of paradigm really refers to "exemplars". In his original description of paradigm (in *The Structure of Scientific Revolutions*), he confusingly also uses "paradigm" to refer to what he now identifies as "disciplinary matrix". The phrase 'exemplar-paradigm' will be used here, when

necessary, to avoid confusion with 'theory-paradigm' and with 'paradigm' used in a more general sense.

27. 1969, p.175.

28. 1970a, p.272.

29. 1974, p.318.

30. Ibid., p.294.

31. 1970b, pp.24-37.

32. A later discussion on perception will clarify this last point.

33. Ibid., pp.52-65.

34. Ibid., pp.24-37.

35. Ibid., p.147.

36. Ibid., pp.66-91.

37. Ibid., pp.23-24.

38. Popper, 1981, pp.44-57.

39. Kuhn, 1970a, p.265.

40. 1965, pp.813-814.

41. 1970b, p.163.

42. Ibid., p.138.

43. Ibid., pp.136-143.

44. Ibid., pp.170-173.

45. 1961, p.208.

46. Ibid., p.95.

47. 1965, p.814.

48. 1969, pp.160-166.

49. Ibid., p.160.

50. 1970b, pp.178-179.

51. 1959, note 3, p.231.

52. Ibid., pp.231-232.

53. 1970b, p.24.

54. Ibid., p.24.

55. Ibid., pp.81-82.

56. Ibid., pp.79-80.

57. Ibid., p.80.

58. Ibid., p.79.

59. 1965, pp.800-803.

60. 1961, pp.184-187.

61. Popper, 1968b, pp.108-111.

62. Ibid., p.109.

63. Kuhn, 1961, p.201.

64. 1970a, p.242.

65. 1961, p.196.

66. 1970b, p.145.

67. 1965, pp.804-805.

68. 1970b, pp.43-47.

69. 1965, pp.811-816.

70. 1977, p.512.

71. 1969, p.199.

72. 1973, pp.325-327.

73. Ibid., p.328.

74. Ibid., pp.336-337.

75. Ibid., pp.321-325.

76. Ibid., pp.330-331.

77. 1970a, pp.261-263.

78. 1974, p.301.

79. 1973, pp.324-325.

80. 1970b, p.90.

81. Ibid., pp.152-153.

82. 1973, pp.332-339.

83. 1970b, pp.152-155.

84. Ibid., p.149.

85. 1973, p.339.

86. 1970b, p.158.

87. Ibid., pp.124-126.

88. 1969, pp.195-196.

89. 1970b, pp.112-116.

90. Ibid., p.94

91. 1970a, p.271.

92. Ibid., pp.265-271.

93. 1970b, p.159.

CHAPTER III

FALSIFICATION AND REJECTION: I. LAKATOS

The basic concept of Lakatos's logic of discovery is the continual competition among *series* of theories, i.e., research programs. For him, the idea of evaluating scientific theories as if they were independent and isolated from other theories is misleading. Indeed, the scientific nature of knowledge is characterised by the progressive development of diverse research programs.[1] When one refers to a single theory, the dependence of that theory on a large body of connected knowledge is always understood, specifies Lakatos:

> Of course, there is nothing wrong in saying that an isolated, single theory is 'scientific' if it represents an advance on its predecessor, as long as one clearly realizes that in this formulation we appraise the theory as the outcome of - an in the context of - a certain historical development.[2]

By concentrating on common problems, a series of theories ensures continuity within a scientific program. It is in that sense that, through the successions of research programs, the growth of science is characterised by a continuity such as described in Kuhn's concept of "normal science",[3] but with the difference that competition between programs is always present.[4]

A research program is defined by Lakatos in terms of methodological rules, some of which are metaphysical in the sense of having no potential falsifiers. A program is characterised by a central theory (or "hard core", as Lakatos usually refers to it). It is comparable to Kuhn's concept of paradigms during 'normal science', in that the direction given to a research program creates and maintains a general consensus among scientists working within it. It thus prevents the research from deviating beyond the boundaries of what scientists identify as relevant to the program. Indeed, some rules inhibit development that would conflict with the hard core. They define the hard core and represent the negative heuristic of a program.[5] The hard core is accepted by convention and considered irrefutable by "provisional decision".[6] The other set of rules, the positive heuristic, is more flexible and serves to indicate the direction to pursue for the successful development of the program.[7] Because the hard core cannot be directly attacked, auxiliary hypotheses are created.[8] They form a "protective belt" around the core and they are what is challenged, tested and reformulated or replaced in order to maintain and strengthen the hard core.[9] If this process leads to new discoveries, the program is progressive. On the other hand, the positive heuristic serves as a guide when anomalies or inconsistencies are encountered. It anticipates anomalies as well as the order of research that will deal with them. The construction of the protective belt of auxiliary hypotheses is thus guided by the positive heuristic of a program, which latter Lakatos describes as follows:

> ...[it] consists of a partially articulated set of suggestions or hints on how to change, develop the 'refutable variants' of the research-programme, how to modify, sophisticate, the 'refutable' protective belt.[10]

Lakatos considers his methodology to be more tolerant than falsificationism because it regards anomalies, inconsistencies and ad hoc stratagems as being integral parts of scientific progress. On the other hand, he also regards it as being stricter because research programs must correctly predict new facts and because the

> ...protective belt of its auxiliary hypotheses should be largely built according to a preconceived unifying idea, laid down in advance in the positive heuristic of the research programme.[11]

* * *

Lakatos rejects the idea of a strict criterion of demarcation between science and non-science. All metaphysical statements cannot be excluded from science. For instance, the hard core contains spatio-temporally universal statements, essential for the continuity of science. That is why Lakatos always insists that it is whole programs together with their auxiliary hypotheses that are evaluated, and not single theories.[12] The aim of a proper scientific epistemology is not to eliminate bad "theories", but rather to be able to distinguish good and better theories from those that are less or no longer productive.[13] Lakatos's solution is thus a methodological evaluation of research programs. A research program is evaluated in terms of "progressive problemshifts" when it is successful, and "degenerating problemshifts" when it becomes unsuccessful.[14] What counts most is new knowledge. A progressive program constantly increases its empirical content by the progression of theoretical "problemshifts", that is new predictions. Theoretical progress can be "verified"[15] immediately by logical analysis.[16] Empirical progress cannot, however, be continuously "verified" because it is done only empirically, and corroborations are often only retrospectively recognized. It takes time because counterexamples may make predictions appear erroneous for a long period, until known facts are reinterpreted in their favor or failures explained by novel auxiliary hypotheses. Before these readjustments occur, the discovery of new facts (or an "empirical shift" as Lakatos calls it) remains unacknowledged.[17] This is why Lakatos states that the empirical content can and should be corroborated, but that corroboration usually occurs only at a later time. In Lakatos's words:

> ...at least every now and then the increase in content should be seen to be retrospectively corroborated: the programme as a whole should also display an *intermittently progressive empirical shift*. We do not demand that each step produce *immediately* an *observed* new fact. Our term '*intermittently*' gives sufficient *rational* scope for dogmatic adherence to a programme in face of *prima facie* 'refutations'.[18]

As long as the theoretical growth keeps on predicting new facts with success, a program is considered progressive.

All scientific research programs do not develop in the same manner, nor at the same speed; progress is neither a regular nor a constant process. Lakatos identifies three typical patterns of development in a research program. In one pattern, a program has a progressive problemshift characterised by an alternation of conjectures and refutations, similar to the concept of growth as explained by Popper. In the second pattern, Lakatos sees an exemplification of the "autonomy of theoretical science", that is that a research program can progress theoretically without any direct contact with the physical world. Here, the program does develop new empirical content but there is no attempt to subject it to any experiments. Scientists are careful not to expose their research to public scrutiny before they consider it developed enough to have successful corroborations. Finally, in the third pattern, there is no empirical progressive problemshift because the facts that the program explains are already known and explained by another program. This program serves only to explain the same phenomena but differently. In this case, the scientific value of the program is recognised only when new facts are produced. Lakatos remarks that the development of a research program is often dependent upon the development of mathematics, or even of other disciplines.[19]

A program enters its degenerative phase when its theoretical progress cannot keep up with either its own empirical growth or that of competitive research programs. It must have recourse to "post-hoc" explanations.[20] When the hard core of a program ceases to predict new facts, it loses its appeal and is abandoned, at least in principle.[21] In fact, a program is not abandoned because its positive heuristic has ceased to be productive. A program is put aside only when another competitive research program is more successful, even while the first program is still progressive. The idea of competition between research programs is paramount to Lakatos's views. Lakatos rejects Kuhn's concept of the dogmatic authority of a single paradigm. The fact that there is always more than one progressive program, which Lakatos refers to as "theoretical pluralism", ensures both the continuity of science and the dynamic of its growth.[22] Lakatos writes:

> The history of science has been and should be a history of competing research programmes (or, if you wish, 'paradigms'), but it has not been and must not become a succession of periods of normal science: the sooner competition starts, the better for progress.[23]

Kuhn's view on the incommensurability of paradigms must thus be rejected as scientists do work with rival programs especially in order to understand where the degenerating one has failed. This leads Lakatos to say that it is not for socio-psychological reasons that a program is abandoned, but for objective reasons. That is, a rival program is adopted because it

> ...explains the previous success of its rival and supersedes it by a further display of *heuristic power*.[24]

Lakatos admits that it is often difficult to evaluate the heuristic power of a competitive program. The appraisal depends on the definition of "factual novelty", and it sometimes takes a long time for a program to be "seen" as really predicting new facts. What are later considered new facts may, for a while, be seen as theoretical reinterpretations of known facts.[25]

A rival progressive program supersedes a degenerating program under three conditions: the rival program can produce new facts, it can explain what the degenerating program could and it can also solve some problems the other program encountered, such as anomalies or ad hoc hypotheses.[26] The defeated degenerating program does not, however, cease to exist.

It may recover a competitive place if it succeeds in producing new facts and corroborating some of them. (Here is another example of the possibility of working with different programs at the same time.)[27] A rejection is thus never definitive. In fact, a defeated program can continue for a long time if, Lakatos states, it keeps producing

> ...content-increasing innovations even if these are unrewarded with empirical success.[28]

On the other hand, a new research program should not be abandoned only because it cannot surpass a rival. Here, Lakatos's "methodological tolerance" suggests that a program should not be ignored when it would be seen as a progressive problemshift "supposing its rival were not there."[29] In order to eliminate scepticism, Lakatos strives to maintain a balance between a restrained but still very present concept of fallibility (off-set by corroboration), and a powerful but not destructive concept of criticism (guided by the richness of a program). Even a negative evaluation of a program does not lead to its rejection. In fact, Lakatos remarks, "elimination is a relatively routine affair".[30] A competitive program supersedes a degenerating program because it has "excess empirical content", that is that it can explain more facts, especially new ones, than its rival even if they are not entirely confirmed. No refutations decide this choice. Lakatos thus demonstrates that it is the progressive problemshift sustained by later corroboration that accounts for scientific progress, not falsification.[31] Consequently, scientific progress is possible *in spite of* anomalies or inconsistencies. This view of progress complements Lakatos's definition of science which stipulates that a program is scientific in so far as it produces "novel" facts.[32] He writes:

> We '*accept*' problemshifts as 'scientific' only if they are at least theoretically progressive: if they are not, we '*reject*' them as 'pseudoscientifc'.[33]

For him, what is important is not only that a theory be corroborated, but first of all that *new* facts be corroborated. Lakatos's "empirical criterion" that a scientific research program ought to produce new facts becomes a criterion of demarcation.[34]

* * *

With Lakatos the relation between rejection and falsification is almost accidental. He makes a distinction between "minor crucial experiments" and "major crucial experiments". Lakatos concedes that "minor crucial experiments" are very common - different versions of a research program are constantly rejected.[35] "Major crucial experiments", however, do not play a definitive role in rejection because they are rarely recognised as crucial at the time they occur. It is usually only *afterwards* that they are seen crucial, often when a superior program explains an anomaly, which then becomes a "brilliant refutation".[36] Sometimes, on the contrary, a refutation later becomes a strong corroboration with further development of the program. Consequently, an experiment never represents a serious challenge.[37] This point is fundamental to Lakatos's views. The

Popperian role of falsification which ensures scientific progress is denied, but not because of Kuhn's concept of crisis. With Lakatos, there is no crisis, but rather continual discovery of new facts. It is not that Lakatos considers falsification useless but rather that he assigns it a role different from the one Popper insists on. Falsifications are frequent but do not play any immediate nor decisive role. A falsifying experiment serves only to *identify* anomalies or inconsistencies *until a better research program is proposed to explain them.*[38] "There is no instant rationality", insists Lakatos.[39] A research program is not and cannot be conclusively challenged at each step of its progression. Evaluation is not an easy clear cut appraisal. That is why the real evaluation takes time and really comes later, when the program finally solves the problems identified by the experiment or after the theory has degenerated and been replaced by another one.[40] Even then, the evaluation is never conclusive. The abandoned program may enjoy a new progressive burst.[41] That is what makes Lakatos write:

> ...if falsification depends on the emergence of better theories, on the invention of theories which anticipate new facts, then falsification is *not* simply a relation between a theory and the empirical basis, but a multiple relation between competing theories, the original 'empirical basis', and the empirical growth resulting from the competition. Falsification can thus be said to have a '*historical character*'.[42]

The problem with "crucial experiments" and empirical tests in general is, according to Lakatos,

> ...the supreme difficulties of deciding exactly *what* one learns from experience, what it 'proves' and what it 'disproves'.[43]

Interpretation of the results of an experiment is not done in isolation, but within the expectations of competitive theoretical demands.[44]

A program is not falsified when it is replaced by a more progressive one. For Lakatos, falsification as a dynamic of scientific growth cannot operate because all scientific propositions, theoretical or factual, are fallible and conjectural. They can be neither proved nor disproved. That is why clashes between theories and facts are dealt with either by creating auxiliary hypotheses to explain anomalies, or by putting aside anomalies for the time being. To follow the precept of dogmatic falsificationism would lead to the rejection of science itself because, according to Lakatos, if not provable, scientific theories would then have to be considered metaphysical. On the other hand, if as with Popper it is accepted that facts cannot prove theories, then we are left with scepticism. But with the recognition that all scientific propositions are fallible, the need of conclusive corroboration or falsification disappears.[45] It remains, however, that even if not authoritative, an empirical basis is still necessary. Yet, corroborations are more important than refutations. They "keep the programme going, recalcitrant instances notwithstanding."[46] Lakatos writes:

> Our considerations show that the positive heuristic forges ahead with almost complete disregard of 'refutations': it may seem that it is the '*verifications*' rather than the refutations which provide the contact points with reality.[47]

Lakatos acknowledges that the problem of the empirical basis is never solved: basic statements have to be accepted or rejected. Nevertheless, he states,

> ...we can make our learning less dogmatic.[48]

To recognise that "observational theories" may be problematic and fallible means that our methodology ought to be more flexible and that some conventionalism is unavoidable.[49] Scientific progress is ensured by the

> ..interaction between the development of the programme and the empirical checks [which] may be very varied -which pattern is actually realized depends only on historical accident.[50]

In fact, Lakatos cautions, to give too much importance to refutation would be a "dangerous methodological cruelty" in the sense that it would prevent research programs from developing, or cause a program to take a much longer time to reach a testable stage, if it ever did.[51]

* * *

So far, our analysis of Lakatos's methodology may seem to limit the empirical basis to a neglected role. But this is not quite so. The superiority of corroboration over falsification being established, Lakatos also proposes a principle of induction which gives a new importance to corroboration. This principle stipulates that

> ...corroboration is a *synthetic* - albeit conjectural - measure of verisimilitude.[52]

The evaluation of growth in science is made possible by a correlation between degree of corroboration and degree of verisimilitude. This concept derives from Popper's theory of verisimilitude. Lakatos, however, reproaches Popper for refusing to admit that positive corroboration means "growth of conjectural knowledge".[53] Indeed, for Popper, corroboration is not a measure of verisimilitude, but only an indication.[54] To say that we are closer to the truth is, he states,

> ...a conjecture, a guess (and 'synthetic'); *not* the appraisal which states the degree of corroboration (which may be said to be 'analytic').[55]

Because Popper refuses to be more specific, Lakatos considers his theory to be without any epistemological value. It means that science can grow but we still do not *know* that it does. As a consequence, Popper's views lead to scepticism because, then, scientific growth means

> ...increased awareness of ignorance rather than growth of knowledge. It is '*learning*' without ever *knowing*. [56]

Lakatos, however, strives to define a criterion of truth because it is essential to identify the increase or decrease of the truth content of a theory if one wants to avoid scepticism.[57] Indeed, for him, without that criterion it is impossible to "*recognise* progress".[58] Lakatos wants more than to "guess that the better corroborated theory is also one that is nearer to the truth".[59] To hope to be closer to the truth is still only a belief -- a metaphysical belief that has nothing to do with rationality. While Popper solves the problem of induction negatively, (by eliminating induction), Lakatos proposes a positive solution, that is

> ...that the scientific game, as played by the greatest scientists, is the best extant way of increasing the

> verisimilitude of our knowledge, of approaching Truth; the *sign* of increasing verisimilitude is increasing degree of corroboration.[60]

Lakatos's theory of verisimilitude therefore allows scientific assessment to be "genuine epistemological appraisals".[61] Of course, Lakatos's theory remains speculative but, as basic statements are already accepted by convention, he sees no problem in also including a "conjectural weak inductive principle"[62] to scientific criticism. It adds more rationality to the logic of the growth of science. Falsification now being excluded from the aim of scientific activity, fallibilism can be accepted without scepticism.[63]

* * *

It may seem inconsistent of Lakatos to argue in favour of a diminished role played by observation and, at the same time, to give corroboration a stronger authority than falsification. His views on anomalies and the autonomy of theoretical science may, however, reconcile this seeming opposition. Lakatos wants to demonstrate two points: first, that theoretical science has more importance in the growth of science than empirical testing, and secondly, that anomalies and ad hoc hypotheses are consistent with progress.

Falsificationism is the main target. In his analysis of Prout's and Bohr's research programs,[64] Lakatos illustrates that theories are often more reliable and accurate than observation. Of course, the fact that observations are theory-impregnated is a part of the problem. What Lakatos wants to emphasize is that theories (in the sense of research programs) can correct erroneous observations. Theoretical science enjoys a "relative autonomy" because it does not need observation to advance. Lakatos states that

> ...it was clear that M_2 and M_3 [theoretical models] would have been developed within the research programme...without *any* stimulus from observation or experiment.[65]

Progress in science is generated by the force of abstract thinking, talent, and imagination, supported by, or even forcing, the development of mathematics. It does not lie in observation and even less in falsification. The role of the empirical basis is rather to identify the scientific character of research programs (prediction of new facts) and to corroborate what scientists "invent" in their theories. Lakatos fears "manic data collection" and too much precision because, at the extreme, they could prevent the initial development of speculative theories.[66] He writes:

> If a scientist (or mathematician) has a positive heuristic, he refuses to be drawn into observation. He will 'lie down on his couch, shut his eyes and forget about the data'... Occasionally, of course, he will ask Nature a shrewd question: he will then be encouraged by Nature's YES, but not discouraged by its NO.[67]

The second point Lakatos wants to demonstrate is that the positive heuristic of a program guides scientists in an "ocean of anomalies";[68] it provides an order of research. Indeed, a program predicts anomalies and dictates the order in which they will be dealt with because the theory of a research program can anticipate future refutations. Some anomalies will be resolved or explained by auxiliary hypotheses constructed following the expected order of development of the program. Unsolvable anomalies will be put aside.[69] This is why Lakatos insists that theoretical knowledge itself reveals when a program does not work and when changes are needed - observations are not necessary.[70] Problems are thus rationally chosen according to the positive heuristic of the program. Disagreeing with Kuhn, Lakatos rejects the idea that psychological factors affect that choice.[71] On the other hand, he agrees with Kuhn's notion of 'normal science', but only in the sense that anomalies are a normal part of research. Their occurrence does not upset a program. For Lakatos, to call anomalies a refutation is only a dramatization of the situation or a linguistic difference.[72] For Lakatos, anomalies are Kuhnian puzzles in the sense that they are problems that challenge a program. There are three typical ways of solving them. First, it is expected that an anomaly can be explained by the program. When it does, what was seen as an anomaly becomes an example confirming a theory.[73] For instance, the erratic movement of Uranus strengthened confidence in Newton's theory of gravity when Neptune was discovered. In the second solution, the anomaly is solved by a different independent program. The problem simply disappears. The remaining possibility is when an anomaly is solved by a rival research program. Then, the anomaly is seen as a counterexample.[74] As all programs constantly encounter anomalies, it is rational to work with a "refuted" program. Lakatos agrees with Kuhn that if a program were routinely to be abandoned because of anomalies, it would be the end of scientific progress. A period of tolerance is necessary in order to find out the reasons for the problems, and justified as long as the program is progressive. It is not that anomalies are totally ignored. They are recognised as problems but, if not immediately solvable, they are put aside in order to allow the program to keep on developing, always keeping in mind that one day these problems will have to be solved. Once the program has reached its limits of development and the heuristic power weakens, then scientists concentrate their attention on anomalies. If no solutions are found then, Lakatos admits, it would be irrational to keep on defending anomalies and refusing to admit failure.[75] An element of dogmatism toward a research program is thus fundamental to the logic of scientific growth. Lakatos explains:

> Thus the 'dogmatism' of 'normal science' does not prevent growth as long as we combine it with the Popperian recognition that there is good, progressive normal science and that there is bad, degenerating normal science, and as long as we retain the *determination* to eliminate, under certain objectively defined conditions, some research programmes.[76]

The idea of trial and error experiments must be abandoned.[77] It is not by their mistakes that scientists learn, but by their theories and their contact with reality given by corroboration.

* * *

With his methodology of scientific research, Lakatos endeavors to demonstrate that scientific progress is a rational process. He does not claim to present a comprehensive theory of the growth of science - he considers it impossible fully to reconcile the freedom and creativity of scientists with methodology.[78] But, he explains, his methodology can expose more of the rationality of scientific behaviour than other methodologies. For instance, the choice of working within an inconsistent theory can now be perceived as rational, and Kuhn's more socio-psychological approach can be accounted for rationally. On the other hand, because a methodology is a rational reconstruction, it is limited by the very fact that human beings are not completely rational.[79] To alleviate this inherent limitation of methodology, Lakatos adds an historical element which can account for Popper's three worlds and, especially, can clearly distinguish World two (feelings, beliefs, consciouness) from World three (objective knowledge articulated in propositions).[80] For Lakatos, a clear understanding of the interaction between these three worlds is essential to grasp the history of science.[81] A rational reconstruction always "cheats" on history. Many elements are left out and the order in which facts were discovered is not always respected.[82] With a concept of internal and external history, rational reconstruction can be dissociated from, while completed by, non-rational factors influencing scientists' behaviour.[83] Internal history belongs to World three. It is primary and represents the intellectual part of history.[84] It is normative and has nothing to do with subjective factors. It contains the philosophies to which scientists adhere and which guide them in their decisions.[85] But a reconstruction cannot contain everything. What is perceived as irrational is eliminated. There remains a "*selection* of methodologically interpreted facts" as well as their "*radically improved version*".[86] It is a selection because the historian cannot avoid theoretical bias.[87] Socio-psychological and, sometimes, political elements of history are, however, also essential to understand science's progress. For instance, Lakatos states the following example:

> No rationality theory will ever solve problems like why Mendelian genetics disappeared in Soviet Russia in the 1950's...[88]

That is why an external history is also needed. It is secondary and empirical. It represents social history and deals with subjective factors. When history differs from its rational reconstruction, external history provides empirical explanations of why it is different.[89] For instance, it

> ...explains why some people have false beliefs about scientific progress, and how their scientific activity may be influenced by such beliefs.[90]

External history, therefore, explains what a solely rational explanation cannot. In dissociating rational from irrational elements, Lakatos can thus reconstitute the rational dimension of scientific progress.

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Contrary to other methodologies, Lakatos's can be applied to evaluate methodologies, his own included.[91] For him, the role of methodologies is not to tell scientists what they should do but, on the contrary, to explain what they do in order to understand "*actual* scientific rationality".[92] In evaluating theories of rationality, Lakatos's methodology becomes a "methodology of historiographical research programs" because, for him, all methodologies can be seen as rational historical reconstructions of science, and thus as historiographical research programs themselves. Historical dimension is, however, sometimes underplayed in Popper's theory. As a consequence, the theory clashes with reality. Indeed, within the history of science, the actual behaviour of scientists is not consistent with that described by Popper. For instance, Lakatos remarks that scientists do not treat anomalies the way Popper describes, nor do they reject falsified theories because of counterexamples. If this is so, Popper's theory should be condemned according to his own standards of falsification. This point is important because Popper's rational reconstruction of scientific discovery cannot account for such behaviour as described by Lakatos. With Lakatos's methodology stressing theoretical pluralism, however, rejection because of falsification is not at stake. In fact, all methodologies can be falsified because

> ...no set of human judgments is completely rational and thus no rational reconstruction can ever coincide with actual history.[93]

What is more important is to evaluate them. Appraisals are based on the ability of a methodology to predict new scientific decisions or changes in established ones. For instance, Popper's theory superseded justificationist and inductivist theories in restoring rationality to scientific theories. It was, therefore, a progressive shift because it allowed more scientific activities to be considered rational and thus made it possible to interpret more actual scientific decisions. A methodology is not addressed to scientists but to philosophers. The influence that Popper's theory had on scientific judgment is an exception, as in that case methodology helped scientists see the negative influence of another methodology (inductivism).[94] The growth of methodologies is characterised by its progressive historiographical research programs; that is, by discoveries of new historical facts about the rationality of science and by the reconstruction of the growth of scientific knowledge within its historical development. Here progress means:

> ...the reconstruction of a growing bulk of value-impregnated history as rational.[95]

As with scientific programs, anomalies are present in methodologies because mistakes and failures in judgment are always possible. Lakatos considers that these characteristics of methodologies, which serve to evaluate them, also apply to his own methodology of research programs. Future research, he states, will corroborate the "progressive problemshift" of his methodology, that is that what is considered irrational choice in Popper's theory can now be considered rational within his own theory. When this new fact about science is recognised, then Lakatos's theory will supersede Popper's theory.[96]

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Lakatos's logic of the growth of science rests upon the possibility of constant competition between research programs stimulated by the imagination of scientists. The main issues raised by Lakatos are the identification of scientific progress and the role played by falsified theories. Gerard Radnitzky and Gunnar Andersson point out that the basic question is that of "cognitive progress" and wonder

> ...what we should mean by that term and how we can recognize whether or not in a concrete case there is such progress.[97]

This is Lakatos's first concern and he responds to it with his principle of induction. Lakatos's position is not that of an extreme inductivist. On the contrary, for him, the main source of knowledge is in the minds of scientists. What he does is to give more "concreteness" to the concept of verisimilitude by further developing the logic followed by Popper. Lakatos agrees with Popper that we do not know what the truth is, but that we have good reason to think that a better theory is closer to the truth, even if this theory is probably false itself. On the other hand, verisimilitude means that a theory closer to the truth has more truth content and less falsity content than a competitive theory. It is to be remembered that truth content refers to all and only true statements deducible from a statement being tested. Falsity content refers only to its false statements. True statements deducible from them are part of its truth content.[98] Lakatos puts forward that we are thus entitled to conclude that the corroboration of a better theory is a measure of truth content. It does not imply that the theory is true because the judgment is always made within a theoretical framework. It means that, in confirming what the theory predicted, corroboration attests the closeness of the theory to the truth. In other words, verisimilitude does serve to identify scientific progress. It is not a conclusive evaluation, but it is a more affirmative statement than Popper's instance on "guessing" the truth of a statement. Indeed, Popper states:

> ...to say that a theory has a greater verisimilitude than one of its competitors remains essentially a matter of guesswork.[99]

Another point of ambiguity is raised by A.E. Musgrave. He asks: What is meant by "rejected theories"? He identifies four meanings. First, a theory is rejected because it is recognised as false. Secondly, a theory is rejected because another theory is better. Thirdly, a rejected theory is one that is put aside. And, fourthly, a rejected theory is one that is eliminated. In the latter meaning, however, it is not clear exactly what "eliminated" refers to unless, as Musgrave suggests, it means "burning the books in which the theory is written."[100] With Popper, the fate of a falsified theory is left ambiguous since Popper's emphasis shifts from falsificationism as total rejection to the more tolerant thesis suggested by Kuhn and Lakatos. Lakatos states that, for scientists, the "pragmatic" meaning of rejection is the "decision to cease working on [a research program]".[101] It may, however, be understood from his analysis that scientists do not actually make that decision. Falsification is useful for identifying anomalies, but not for judging whole research programs since its relevance is recognised only retrospectively. It has to be concluded from Lakatos's discussion that scientists progressively lose interest in a degenerative theory. Theories fall into oblivion with time because scientists prefer to concentrate their attention on more rewarding research programs. It is not a steady and constant process; interest in a theory can be both lost and renewed from time to time.

What distinguishes Lakatos's views most sharply from those of Popper is his insistence that what scientists actually do must be a part of the answer. Not the whole answer, as Kuhn tends to think, but a source of information and checkpoints on the reconstruction of scientific work. That is why for Lakatos these fundamental questions can be answered only through studying scientists' behaviour; not their individual idiosyncracies, but their behaviour as a group indulging in a rational activity. Methodologies must take into account the actual decisions of the scientific community. Because scientists can be mistaken, however, their decisions must also be judged against a comprehensive rational methodology. Lakatos's argument that theoretical science is primary and autonomous implies that both questions and answers about nature come from World three.

NOTES

1. Lakatos, 1970, pp.118-119.

2. Ibid., p.119, note 2.

3. Ibid., p.132.

4. Ibid., p.183.

5. Ibid., pp.132-135.

6. 1969, p.248.

7. 1970, p.132.

8. Ibid., p.182. With Lakatos, auxiliary hypotheses are acceptable in so far as they follow the requirements of the positive heuristic. Auxiliary hypotheses are necessary to maintain progress, as they can deal with inconsistencies and anomalies. Without them, the research would be interrupted too early.

9. Ibid., p.133.

10. Ibid., p.135.

11. 1969, p.249.

12. 1971a, p.99.

13. 1971b, p.178.

14. 1971a, p.99.

15. It is to be noted that Lakatos uses the word "verification" not in the sense of conclusive proof but rather as "...a corroboration of excess content in the expanding programme...He specifies:"[a] 'verification' does not *verify* a programme: it shows only its heuristic power."(1970, p.137, note 2.)

16. 1970, p.116. Lakatos does not ignore the problem of agreement on whether a fact is really "new" or only differently interpreted. He is here referring to the situation in general, when the 'newness' of a prediction is not at stake.

17. Ibid., pp.133-134.

18. Ibid., p.134.

19. Ibid., pp.151-154.

20. 1971a, pp.100-101.

21. 1970, p.154.

22. Ibid., pp.154-155.

23. Ibid., p.155. (Italised in the text.)

24. Ibid.

25. Ibid., pp.155-156.

26. Ibid., p.154.

27. Ibid., pp.157-158.

28. Ibid., p.158.

29. Ibid., p.157.

30. Ibid.

31. 1971a, pp.100-101.

32. 1970, p.182.

33. Ibid., p.118.

34. Ibid., p.119.

35. Ibid., p.157.

36. Ibid., p.159.

37. Ibid., pp.158-173.

38. Ibid., p.119.

39. 1971a, p.101.

40. 1970, p.173.

41. 1971a, p.101.

42. 1970, p.120.

43. Ibid., p.168.

44. Ibid., pp.168-172.

45. Ibid., pp.99-103.

46. Ibid., p.137.

47. Ibid.

48. Ibid., p.131.

49. Ibid., pp.129-131.

50. Ibid., p.151.

51. Ibid.

52. 1969, p.256.

53. Ibid.

54. Popper, 1974e, p.1011.

55. Ibid.

56. Lakatos, 1969, p.254.

57. Ibid.

58. Ibid.

59. Popper, 1974e, p.1011.

60. Lakatos, 1969, p.255.

61. Ibid., p.256.

62. Ibid., p.261.

63. Ibid., pp.254-262.

64. 1970, pp.139-149.

65. Ibid., p.149.

66. Ibid., p.152, note 4.

67. Ibid., p.135, note 1.

68. Ibid., p.133.

69. Ibid., pp.134-136.

70. Ibid., p.151.

71. Ibid., p.137.

72. Ibid., p.182.

73. Ibid., p.159, note 1.

74. Ibid.

75. Ibid., pp.137-145.

76. Ibid., p.177.

77. 1971a, p.100.

78. 1971b, p.179.

79. 1971a, p.102.

80. Ibid., p.127, note 61.

81. 1970, pp.179-180.

82. 1971a, p.107.

83. Ibid., p.105.

84. Ibid., p.123, note 1.

85. Ibid., pp.105-106.

86. Ibid., p.106.

87. Ibid.

88. Ibid., p.102.

89. Ibid., pp.105-106.

90. Ibid., p.105.

91. Ibid., p.118.

92. 1969, p.252.

93. 1971a, p.116.

94. Popper, 1969, p.252.

95. Lakatos, 1969, p.251.

96. 1971a, pp.109-118.

97. Radnitzky and Andersson, 1978, p.17.

98. Please see pp.34 and 35 for the description of Popper's concept of verisimilitude.

99. Popper, 1974b, p.1102.

100. Musgrave, 1973, p.403.

101. Lakatos, 1970, p.157.

CHAPTER IV

CONCLUSION

The epistemological questions treated in this thesis are of particular relevance for the sociology of science since this sub-discipline has systematically neglected the role Popper's World three plays in scientific enquiry.

As we have seen, Popper argues that progress in science can be explained in terms of a 'logic of inquiry' because science is a rational process. The main questions Popper attempts to answer are: what is the character of scientific knowledge, how does science progress, and what is meant by scientific growth? For Popper, World two does not directly affect the process of acquiring knowledge because of the very nature of scientific knowledge. Scientific knowledge is a form of knowledge that can be considered objective because it is acquired through a method that is objective. When the means of acquisition are not objective, we are no longer talking about scientific knowledge. 'Scientific method' belongs to World three. Its aim is to identify propositions that have a scientific character and which are better than those currently held. The concept of "better" propositions means 'closer to the truth', since a new hypothesis chosen over another contains more true statements and less false statements than the latter. Scientific method hence ensures not only an increase in objective knowledge, but also an increase in truth content. But, while trustable because objective, scientific knowledge is also said to be provisional and conjectural. Popper then has to answer the question as to how we can claim to have knowledge we can trust if we can never know whether it is true or false and if all theories are probably false.

Popper explains why scientific knowledge cannot be considered certain knowledge. Knowledge is evaluated by testing propositions against reality. Even though tests follow rigorous objective procedures, they are set up within theoretical frameworks which affect the observation itself as well as the assessment of the results of observation. Results of empirical testing are always inconclusive because tests can only check single instances. Tests can be repeated, but the corroboration or falsification of an hypothesis cannot rationally be held to have universal application. Consequently, observation cannot provide conclusive assessment. The empirical basis of science plays a secondary role in the growth of knowledge in the sense that the results of testing must always be interpreted, judged or balanced. That is what Popper means when he insists that theories are, in one sense, accepted by convention, i.e., by rational decision. Since the acquisition of knowledge is not an inductive process but a process of trial and error, testing of theories can be seen as a rational activity. Indeed, tests are not expected to "verify" knowledge claims but only to give inconclusive information as to the correpondence of hypotheses with reality. Rationally, we have no way of knowing the future -

of being certain that future tests will have identical results to the present ones. That is why results of experiments ought to be considered inconclusive and, consequently, knowledge provisional.

Popper's theory is a rational reconstruction of how scientific knowledge grows, and is as such a descriptive theory. It has often been remarked, however, that Popper unfortunately uses prescriptive elements without always clearly distinguishing them from the descriptive part of his theory. His theory sometimes takes the form of an ideal to be reached or a guide to what scientists should do. As a consequence, Popper's rules of scientific behaviour have been misunderstood as to the extent to which they are applicable. Indeed, it is argued that scientists do not abide by the rigorous criteria enunciated by Popper. For instance, falsification, it is alleged, would not play a decisive role in science because it is often not considered canonical during ordinary scientific activities. This argument is frequently used in order to refute Popper's theory which states that science grows through the falsification and rejection of theories and their replacement by better ones. If Popper had set out specifically to describe scientists' daily activities, such would be the case; but he does not. According to Popper, the concept of falsification is part of the logic of scientific growth; it is the dynamic of that growth. The frequency of its use is irrelevant except in the limiting case, not in fact characteristic of science, where falsifications are wholly ignored. That is to say that scientists may not have to devote most of their time to trying to falsify their theories. For science to grow, what is necessary is a critical attitude to ensure that false theories are identified, rejected, and replaced by better ones at some time. We do not know exactly when. This does not mean that all false theories are rejected, but that the process is in action often enough or regularly enough for growth to occur. It is not a question of how many theories are replaced but, rather, that as long as some theories are replaced by better ones, science progresses. The *practical* details of scientific enquiry are not dealt with because Popper is concerned exclusively with the logic of the process.

The accusation that Popper's emphasis on falsification is destructive to the spirit of science, in the sense that scientists are understood to have as their only goal the falsification of theories, is not justified. This extreme interpretation of Popper's views misrepresents Popper's endeavour. Popper is not a naive falsificationist. He does not claim that scientists create theories only in order to reject them but, rather, it is because scientists critically assess any scientific propositions that scientific growth occurs. That is why the theories that survive scrutiny can be trusted. It is not just any propositions that are accepted by scientists, but only those that convey the character of objective knowledge. Growth of scientific knowledge, with Popper, does not mean only the accumulation of more knowledge, but also of better, truer knowledge, that is of verisimilitude. Science grows in quality, not only in quantity. Consequently, science has no reason to be immobilised by a spirit of scepticism that would make all knowledge claims so doubtful that the effort to discover more would seem useless. Popper's essential message is that science grows by replacing false knowledge with truer knowledge and that while scientific knowledge can be trusted because of its objective means of acquisition, it cannot have a dogmatic authority since there is no rational justification for dogmatism.

The role and nature of Popper's critical rationalism has been and is still the object of great controversy in the philosophy and the sociology of science. This thesis will argue that Popper's theory on the logic of scientific growth is not seriously challenged by Kuhn's views, and that Lakatos's analysis shows that Kuhn's views can in part be assimilated to those of Popper. The clash between Popper and Kuhn may be due to some ambiguity in Popper's theory but it is mostly to a misunderstanding of what Popper's theory fundamentally is, that is a rational reconstruction of the logic of growth in science. Whether Popper is right in arguing for a logic of scientific growth is not at the heart of the discussion. The issue is that if a logic of knowledge growth exists, scientists' subjective incentives and motives are not relevant to that logic except in so far as they may hinder its free application. That is to say that the logic of the progress of scientific knowledge can be understood within World three only.

Kuhn's views are generally perceived as irreconcilable with Popper's theory. In direct conflict with Popper, Kuhn argues for the incommensurability between theory-paradigms, stability and dogmatic commitment to paradigms and, more importantly, for the relative absence of critical rationalism in ordinary scientific practice. Kuhn also dissociates rejection and falsification, and denies falsification any important role in the choice between theories. He argues that it is not only critical rationalism but also psychological, sociological and other subjective factors that are responsible for the growth of science. Critical rationalism would wholly inhibit science if it became as important a contribution as Popper claims.

Kuhn's and Popper's opposition can be partially reconciled if the nature of their respective theories is clearly understood from the perspective of World three and World two. The theories are not situated at the same level. Kuhn's theory mainly analyses the practice of science in its daily activities. It is not so much a rational reconstruction of the logic of scientific growth as an empirical analysis of its practice. Kuhn's views may thus be seen as complementary to Popper's theory in that he analyses the details of the scientific activities left out in the rational reconstruction of Popper. For instance, Popper explains the objective characteristics of science in describing its method. He thus discusses how a new theory is evaluated by objective methodological procedures and how scientists decide on the value of these procedures. Popper, however, does not explain specifically how a new theory comes to be proposed or how a problem comes to be seen as a problem to be solved. Kuhn does this with his description of the development of a paradigm which leads to a crisis. When a theory reaches its limits of application and it can no longer solve problems, especially when the anomalies generated by the theory have been temporarily put aside, it then becomes apparent that a new theory is needed to explain those anomalies and to provide further 'puzzle-solving' opportunities. In other words, a new theory is needed because it is time to solve the problems ignored by the old paradigm. During its expansion a paradigm reveals some problems that it cannot itself solve. Kuhn insists that Popper's theory is mistaken in proposing falsification as a dynamic of scientific growth because scientists expect falsifying instances and do not reject their theories because of them. They rather put them aside. What is most important in Kuhn's analysis is the dissociation he sees between falsification and rejection. Both processes occur, but not in a causal relationship. Popper's ambiguity has created some confusion about his views on the fate of a falsified theory. In some of his writings, Popper insists that it is because a falsified theory is rejected that science progresses. This statement refers to the rational reconstruction of science. In other writings, Popper stresses that, *in practice*, a falsified theory is not always rejected. In his response to Lakatos, he finally clarifies his position:

> It is true that I have used the terms 'elimination', and even 'rejection' when discussing 'refutation'. But it is clear from my main discussion that these terms mean, when applied to a scientific theory, that it is eliminated as a contender for the truth - that is, refuted, but not necesssarily abandoned. [1]

Even though, in some specific cases, a falsified theory is not rejected, the logic of scientific growth is not challenged. Kuhn's description of "normal science" explains in more detail why falsification does not lead automatically to rejection. A paradigm needs time to prove itself and to attain its limits. In the meantime, scientists work within a theory that has encountered falsification. It is true that Kuhn insists that an attitude of trust towards a paradigm is more appropriate than a response of scepticism or doubt. A paradigm is described as having dogmatic authority. If this attitude prevailed, it would conflict with Popper's logic of science. But Kuhn specifies that paradigms come to be challenged and rejected. How soon or how frequently they are, does not affect the logic of science. Kuhn's and Popper's confrontation about the terms "commitment" and "dogmatic" takes the form of a quarrel over words. Popper does not reject what may be called a justified dogmatism. He insists that

> the dogmatic defence of a theory has a positive methodological role to play. [2]

"Dogmatic" here is very close to the meaning given to that term by Kuhn and it belongs to the same level of empirical analysis, not to the theory of the logic of science itself. Indeed, it refers to the actual practice of science and it demands of the scientists that they do not give up too early or too easily in the face of counter-examples. In fact if, as according to Popper, scientists come to an agreement as to the conclusion to be drawn from observation, then it is a logical consequence that these scientists share a trust for a corroborated theory. Popper argues that verisimilitude indicates a better correspondence to facts and that tests serve to identify false theories. There is, therefore, no reason to object to the scientists' commitment to a severely tested and corroborated theory such as 'paradigms' come to be. Scientists committed themselves to Newton's theory for a long time. Popper does not consider their commitment unscientific. He admires the genius of Newton who was able to conceive a theory that could furnish a general consensus within the scientific community for so long. The commitment may not be as strong or as common as Kuhn claims. It remains, and Popper agrees on this, that as long as a better theory has not been proposed, scientists cannot do otherwise than to work within the theoretical frameworks available, whether falsified or not. Popper does not disagree with the idea of the development of a paradigm during "normal science". What he objects to is that, for Kuhn, 'routine activity' characterises science.[3] Popper's objection refers to the practice of science. He dislikes "routinised" science and considers it likely to endanger the survival of science. Popper is, however, too extreme in his rejection of "routinised" science. Even if it were true that modern science has become in great part "routinised" because of the modern phenomenon of technological mass production,[4] routine does not necessarily endanger science. It is the contention of this thesis that the number of scientists engaged in routine activities may not be important when explaining the logic of scientific growth. Whatever their number, as long as some scientists continue to practice science following the precepts of critical rationalism, science should progress if the logic of its growth is that described by Popper. If all paradigms should cease completely to be challenged, then science would end when the last paradigm reached its limits.

Kuhn's sociological and psychological explanations of why a new paradigm is accepted by the scientific community also do not represent a definite challenge to Popper's theory. Kuhn argues that a theory is not accepted for reasons alone because the choice of criteria is influenced by subjective factors as well. For instance, Kuhn asserts that a theory is accepted, in part, because it is proposed or supported by reputed scientists and their judgment is trusted. Other scientists do not try to falsify or even to criticise the theory. If this is so, one may conclude that progress in science is ensured by only a small group of scientists, those who actually make decisions. But it must also be assumed that those scientists follow a method similar to that described by Popper since it is not simply any new paradigms that are chosen. Indeed, the choice is not as arbitrary as it first seems in reading Kuhn. A new paradigm is accepted, Kuhn specifies, because it "fits the facts better."[5] It solves at least some of the anomalies of the challenged paradigm and seems to be able to delineate and solve more problems than other competitive theories. It is seen as a "promise of success".[6] If, as Kuhn indicates, the choice is based on methodological rules similar to those proposed by Popper, then the choice cannot be said to be subjective. Whether the theory is proposed by a young scientist still not committed to a recognised paradigm, or whether professional background affects the interpretation of the methodological rules, does not challenge Popper's logic of science. Popper refers to methodological rules as conventions precisely because the criteria of choice cannot be specified entirely. Each test requires the evaluation of scientists. Popper agrees that the choice is always problematic. Kuhn argues that because scientists perceive and interpret objective criteria differently, the choice is not entirely objective. Their interpretation is affected by their personalities and their professional training. Popper agrees that criteria are not clearly defined, and that their interpretation may vary. Even if a scientist considers that the accuracy of a theory is the most important criterion because of a peculiarity of his personality, this reason does not deprive 'accuracy' of its objective character. What is most important for Popper is that objective criteria are being used to choose a theory, and they are objective because they are rationally justifiable. If Kuhn is right that subjective factors play an important role in the choice of a theory, then how can he explain that the chosen paradigm "fits the facts better"?[7] The fact that the new paradigm does have characteristics similar to a theory chosen under Popper's methodological rules cannot be accidental. If, on the other hand, the subjective factors are influential enough to make objective criteria vary enough to question their importance in theory choice, then Kuhn does not explain how the strong commitment to a paradigm can be possible despite the influence of subjective factors. There seems to be some inconsistency in Kuhn's views. For him, science is not characterised by the objectivity of its method, but by the consensus around a paradigm that identifies scientific knowledge. A scientific theory is, however, characterised by objective criteria. Kuhn does not demonstrate clearly enough how subjective factors can directly affect the objectivity of scientific choice.

Kuhn's views of the progress of science are the only elements of his theory obviously irreconcilable with Popper. For Kuhn, there is no progress in science but, rather, a juxtaposition of new and independent knowledge. In 'normal science', there is a form of progress in the sense that paradigms are developed. Only then does scientific knowledge expand. In times of crisis, however, the incommensurability of views between paradigms prevents progress - old knowledge is replaced by new. Consequently, Kuhn argues that one cannot talk about progress in science because there is no continuity in the growth of scientific knowledge. Even if there were progress, Kuhn specifies, there is no means of recognising it. Here Kuhn is more consistent than Popper in applying the precepts of rationality. Kuhn rightly argues that if we do not know what the truth is, then we are not rationally entitled to say that a better theory is closer to the truth. The concept of verisimilitude is the most vulnerable part of Popper's theory because it is an a priori assumption. Verisimilitude gives meaning to scientists' efforts to make sense of the physical world but, like rationality, it takes the form of a belief. If the truth is unattainable, it is consequently not a question of logic to assert that a better theory is more true. Without a means of defining it, truth remains an hazy ideal. If, as Popper states, all theories are probably false, how can there be a bringing closer to truth? Logically, the progressive increase in truth content and decrease of falsity content should eventually, however long it takes, end up in purely true theories if progress means getting closer to truth. Given the a priori unattainability of truth, Popper lacks grounds for associating progress with the truth.

Kuhn's strongest argument against Popper's theory is that not only is critical rationalism not prevalent in scientific growth but, if it were, science would end. Kuhn insists on the absence of criticism during "normal science". Since "normal science" characterises science for Kuhn, the absence of a criterion of critical rationalism is an important challenge to Popper's theory. It will be shown, however, that Kuhn's statement can be understood differently and that, in fact, critical rationalism can be seen as present in "normal science". Kuhn's arguments are based on his view that, in their ordinary practice, scientists do not try to challenge the paradigm they are working with. That is why he asserts that criticism is absent from "normal science". He rightly argues that if scientists tried to falsify theories too soon, they would be too easily rejected and, consequently, science could not progress. What Kuhn means, therefore, is that an excess of criticism would be detrimental to science. Kuhn, however, misrepresents Popper's concept of critical rationalism. Kuhn sees Popper as a 'naive falsificationist'. It has been demonstrated earlier that this is not the case. Kuhn seems to interpret 'criticism' only as falsification followed by the immediate rejection of a paradigm. That is why he argues that critical rationalism comes into play only during times of crisis, that is, when competing theories challenge a no longer satisfactory paradigm. What Kuhn does not seem to take into consideration, however, is that during the application and expansion of a paradigm the same procedural rules are used as during a crisis. Even if scientists do not intend to test their paradigm, some testing still has to take place for the paradigm to develop and make new discoveries. Other theories of lesser importance than a paradigm are produced, tested, corroborated or falsified and rejected during the process of expansion. Even though these theories are proposed within the framework of the paradigm, critical rationalism is, in fact, at work following the same objective specified by Popper's logic of science, but with a somewhat different end in view. The point is that critical rationalism does not address itself merely to fundamental theories. Popper's reference to them may create a mistaken interpretation of his view. Critical rationalism refers to an attitude of doubt towards any scientific hypothesis - doubt in the sense of questioning any scientific procedures in order to make sure no errors are missed, and in being motivated to test hypotheses in order to identify those which are mistaken. Even if the paradigm is not itself challenged, it does not in fact mean that critical rationalism is absent. A small challenge remains a challenge. In that sense, Kuhn is not justified in claiming that scientists do not exhibit a critical attitude. On the other hand, even if critical rationalism were not constantly at work, its occasional absence would not affect the logic of scientific growth because, once again, it is not the frequency of its use that makes science grow. Possibly, all scientists' activities may not have to be scientific in Popper's sense for science to continue to exist.

Lakatos's theory can be seen as a possible reconciliation between Popper and Kuhn because it suggests that the logic of scientific growth can be understood without any reliance on World two. A better understanding of some aspects of World two, however, Lakatos argues, would provide clarification of unexplained developments in the World three of science.

Lakatos's theory is a continuation of Popper's rational reconstruction of the logic of science. A part of Lakatos's theory, however, can be seen as an analysis of the practice of science. It is at that level that a reconciliation between

Kuhn's and Popper's theories is possible. Lakatos's views are often in agreement with Kuhn's. His concept of a progressive research program is comparable with Kuhn's development of paradigms. Also similarly, a research program reaches its limits when it can no longer solve problems. Lakatos's distinction is that, like Popper but more insistently, he characterises scientific activity as a continual competition between theories. Scientists are strongly committed to their programs, but they are not as constrained by them as Kuhn argues. With Kuhn, there is no competition in "normal science" because the incommensurability of paradigms prevents scientists working with different frameworks at the same time. Lakatos shows that this cannot be the case. For him, it is in fact the interaction between research programs that ensures continuity in science and the dynamic of its growth. He argues that scientists do work with different and opposing theories, especially during the degenerating phase of a program in order to understand how and why their program is failing. As with Kuhn, a research program is not challenged while it is still expanding. It is the auxiliary hypotheses that are subjected to tests and, in that sense, the program is protected. As with paradigms, the framework of a program guides the direction of research as long as it is productive.

Lakatos agrees with Kuhn that, in general, theories are not rejected because they encounter some falsifying empirical evidence. Lakatos shows, however, that scientists' commitment to a refuted theory is rational behaviour, independent of any subjective factors. This commmitment is necessary for science to continue. Popper specifies that the falsification and rejection of mistaken theories are the dynamic of the logic of scientific growth. Kuhn's and Lakatos's insistence that, in general, theories are not prematuraly rejected does not necessarily conflict with Popper's logic of science. Kuhn refers only to paradigms and specifies the conditions for their actual rejection. Lakatos, however, makes the distinction between research programs and auxiliary hypotheses. Auxiliary hypotheses, comparable to what has been referred to as "lesser" theories in the discussion of Kuhn's theory, are subjected to tests, corroborated, or falsified and actually rejected. The process is, however, different with research programs. There is no actual and immediate decision to abandon a degenerative program any more than there is to accept a new one. Lakatos's analysis brings more information to bear upon what scientists actually do in practice in order to clarify the process of rejection and acceptance of theories. It is a process characterised by the progressive conviction of the worth or the uselessness of a program. With Lakatos, it becomes a long process - one in which we cannot identify exactly at what point a program ceases to be considered useful by most scientists. Some scientists may take more time to realise or to admit the failure of their program. Some may never do so. There is no crisis and no instant decision. There is, therefore, a distinction in the role played by "lesser" theories and "important" theories in the growth of science. Whether some scientists remain committed to a paradigm rejected by the rest of the scientific community, or whether a paradigm is actually "rejected", is irrelevant to the logic of scientific growth. What is important is that research programs come to be considered ineffective (even if all scientists do not agree) and that other research programs are considered to replace them because they can solve problems unsolvable by previous programs and because they can produce new theoretical growth. That is to say that the application of falsification may vary. In "normal science", some "lesser" theories are falsified and rejected as a direct consequence of tests conceived to assess them. Paradigms or research programs are not considered to be subjected to tests conceived to assess them because, as Lakatos's analysis indicates, it is only when the programs have reached their limits of productivity that previous negative experiments are seen as significant evidence of their limitations. This acknowledgment takes the form of a crisis for Kuhn because it is described as an exceptional occurrence in science. With Lakatos, the competition between programs is always present. If a change of program is a crisis, then science is in perpetual crisis. Sometimes, however, a research program may not be actually confronted by a falsifying empirical observation. Then, the program comes, retrospectively, to be considered false, in the sense that its failure is recognised by its lack of productivity. Lakatos's approach makes falsification appear less authoritative, in practice, than with Popper. Corroboration is more in evidence than falsification since, in the practice of science, contact with the physical world serves to check the progress of a research program. With Lakatos, critical rationalism is always at play in the practice of science, but does not necessarily follow a strict pattern.

What is important with Lakatos is that, in agreement with Popper, his analysis demonstrates that scientific progress can be understood within World three. It is suggested that the choice of theories is explicable in terms of rationality alone. For instance, Lakatos explains in terms of rationality why scientists prefer to wait before exposing their theories to the scrutiny of the scientific community, and why working with a falsified program can be a rational choice. The subjective elements of World two are not relevant to an understanding of the logic of World three, but they are still important. They belong to the external history of science and may be useful in explaining the political and social conditions that characterise science within a specific context. They do not ensure progress in knowledge, but they may be important in understanding a particular direction in the development of scientific knowledge.

Lakatos's views do not solve all the issues involved in the divergence between Kuhn's and Popper's views on the growth of knowledge. Lakatos's theory can be seen, nevertheless, as an advance in the resolution of these issues in making a clear distinction between the rational elements of the internal history of science (World three) and subjective elements (World two). The role of non-rational factors in the development of knowledge can thus be better understood. It is incumbent on the sociology of science to study further the practice of science in order to bring more comprehension on the nature and the extent of the interactions between World three and World two. Future empirical research in the sociology of science needs to address World three problems, and findings need to be interpreted within that world. A merely 'behavioural' analysis of scientific institutions and of practising scientists is insufficient to explain this peculiarly rational dimension of intellectual life.

NOTES

1. Popper, 1974d, p.1009.

2. Ibid., p.1010.

3. 1970, p.51.

4. 1974c, pp.1144-1148.

5. Kuhn, 1970b, p.147.

6. Ibid., p.23.

7. Ibid., p.147.

BIBLIOGRAPHY

Ackermann, J.R. (1976), *The Philosophy of Karl Popper*, Amherst: University of Massachusetts Press.

Acton, H.B. (1951), "Comte's Positivism and the Science of Society", *Philosophy - The Journal of the Royal Institute of Philosophy*, Vol. 26, No. 99, October, pp.291-310.

Adorno, T.W. (1977), *The Positivist Dispute in German Sociology*, translated by G. Adey and D. Frisby, London: Heinemann.

Aron, R. (1931), *La Sociologie allemande contemporaine*, Paris: Quadridge/PUF.

Ayer, A.J. (1936), "Verification and Experience", in *Logical Positivism*, A.J. Ayer (Ed.), New York: The Free Press, 1959.

__________(1946), *Language, Truth and Logic*, New York: Dover Publications.

__________(1950), *Hume*, Oxford: Oxford University Press.

__________(1963), *The Foundations of Empirical Knowledge*, London: MacMillan and Co. Ltd.

__________(1965), *The Problem of Knowledge*, London: MacMillan and Co. Ltd.

__________(1967), "Truth, Verification and Verisimilitude", in *The Philosophy of Karl Popper*, P.A. Schilpp (Ed.), Vol. II, The Library of Living Philosophers, Lasalle, Illinois: The Open Court Publishing Co., 1974.

Barnes, B. (1974), *Scientific Knowledge and Sociological Theory*, London: Routledge & Kegan Paul.

Bartley, W.W. III (1962), *The Retreat to Commitment*, New York: Alfred A. Knoff.

__________(1968), "Demarcation Between Science and Metaphysics", in *The Problems in the Philosophy of Science*, (Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Vol. 3), I. Lakatos and A. Musgrave (Eds.), Amsterdam: North-Holland Publishing Co.

Belth, M. (1977), *The Process of Thinking*, New York: McKay.

Berkeley, G.(1957), *A Treatise Concerning the Principle of Human Knowledge*, C.M. Turbayne (Ed.), Library of Liberal Arts, New York: Bobby-Merrill Co. Inc.

Brodbeck, M. (1959), "Models, Meaning and Theories", in *Symposium in Sociological Theory*, L. Gross (Ed.), New York: Harper and Row.

Comte, A. (1852), *Cours de philosophie positive*, Tome 1, Paris: Borrane et Broz, Libraires.

Dixon, K. (1973), *Sociological Theory - Pretence and Possibility*, London: Routledge and Kegan Paul.

__________(1980), *The Sociology of Belief - Fallacy and Foundation*, London: Routledge & Kegan Paul.

Ferre, F. (1970), *Introduction to Positive Philosophy, Comte*, Indianapolis: The Library of Liberal Arts.

Feyerabend, P. (1975), *Against Method - Outline of an Anarchistic Theory of Knowledge*, London: NLB.

Frisby, D. (1972), "The Popper-Adorno Controversy: The Methodological Dispute in German Phisolophy", in *Philosophy of the Social Sciences*, Vol. 2.

Gellner, E. (1974a), *Legitimation of Belief*, London, Cambridge: University Press.

__________(1974b), "The New Idealism - Cause and Meaning in the Social Sciences", in *Positivism and Sociology*, A. Giddens (Ed.), London: Heinemann.

Giddens, A. (1974), *Positivism and Sociology*, London: Heinemann.

Gross, L. (Ed.) (1967), *Sociological Theory: Inquiries and Paradigms*, New York: Harper & Row.

Halfpenny, P. (1982), *Positivism and Sociology: Explaining Social Life*, Boston: George Allen and Unwin Ltd.

Hume, D. (1955), *Inquiry Concerning Human Understanding*, Charles W. Hendel (Ed.), Indianapolis: Library of Liberal Arts.

Keat, R. & Urry, J. (1980), *Social Theory As Science*, London: Routledge and Kegan Paul.

Kekes, J. (1977), "Rationality and Problem-Solving", *Philosophy of Social Sciences*, Vol. 7, pp.351-366.

Kolakowski, L. (1972), *Positivist Philosophy -- From Hume to the Vienna Circle*, translated by Norbert Guterman, Middlesex: Penguin Books.

Kuhn, T.S. (1959), "The Essential Tension: Tradition and Innovation In Scientific Research", in *The Essential Tension - Selected Studies In Scientific Tradition and Change*, T. Kuhn, Chicago: The University of Chicago Press, 1977.

__________(1961), "The Function of Measurement in Modern Physical Science", in *The Essential Tension - Selected Studies in Scientific Tradition and Change*, T. Kuhn, Chicago: The University of Chicago Press, 1977.

__________(1965), "The Logic of Discovery and the Psychology of Research", in *The Philosophy of Karl Popper*, P.A. Schilpp (Ed.), Vol. II, The Library of Living Philosophers, Lasalle, Illinois: The Open Court Publishing Co., 1974.

__________(1969), "Postcript - 1969", in *The Structure of Scientific Revolutions*, 2nd ed. (enlarged), Chicago: The University of Chicago Press, 1970.

__________(1970a), "Reflections On My Critics", in *Criticism and the Growth of Knowledge*, (Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Vol. 4), I. Lakatos and A. Musgrave (Eds.), Cambridge: Cambridge University Press.

__________(1970b), *The Structure of Scientific Revolutions*, 2nd ed. (enlarged), Chicago: The University of Chicago Press.

__________(1973), "Objectivity, Value Judgment and Theory Choice", in *The Essential Tension - Selected Studies in Scientific Tradition and Change*, T. Kuhn, Chicago: The University of Chicago Press, 1977.

__________(1974), "Second Thoughts on Paradigms", in *The Essential Tension - Selected Studies in Scientific Tradition and Change*, T. Kuhn, Chicago: The University of Chicago Press, 1977.

__________(1977), "Discussion", in *The Structure of Scientific Theories*, (Section VI), F. Suppe (Ed.), 2nd ed., Board of Trustees of the University of Illinois.

Lakatos, I. (1969), "Popper on Demarcation and Induction", in *The Philosophy of Karl Popper*, P.A. Schilpp (Ed.), Vol. I, The Library of Living Philosophers, Lasalle, Illinois: The Open Court Publishing Co., 1974.

__________(1970), "Falsification and the Methodology of Scientific Research Programmes", in *Criticism and Growth of Knowledge*, I. Lakatos and A. Musgrave (Eds.), Cambridge: Cambridge University Press.

__________(1971a), "History of Science and Its Rational Reconstructions", in *Boston Studies in the Philosophy of Science*, (Proceedings of the 1970 Biennial Meeting of the Philosophy of Science Association, Vol. 8), Roger C. Buck and Robert S. Cohen (Eds.), Dordrecht-Holland: D. Reidel Publishing Co.

__________(1971b), "Replies to Critics" in *Boston Studies in the Philosophy of Science*, (Proceedings of the 1970 Biennial Meeting of the Philosophy of Science Association, Vol. 8), Roger C. Buck and Robert S. Cohen (Eds.), Dordrecht-Holland: D. Reidel Publishing Co.

Lakatos, I. & Musgrave, A. (Eds.), (1970), *Criticism and the Growth of Knowledge*, (Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Vol. 4), Cambridge: Cambridge University Press.

Locke, J. (1964), *An Essay Concerning Human Understanding*, S.D. Woozley (Ed.), London: Fontana Library.

Madge, J. (1964), *The Origin of Scientific Sociology*, New York: The Free Press of Glencoe.

Magee, B. (1982), *Popper*, Glasgow: Fontana/Collins.

Masterman, M. (1970), "The Nature of a Paradigm", in *Criticism and the Growth of Knowledge*, (Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Vol. 4), I. Lakatos and A. Musgrave (Eds.), Cambridge: Cambridge Univesity Press.

Miller, D. (1975), "The Accuracy of Predictions", *Synthese*, Vol. 30, Nos 1 & 2, February/March, pp.159-191.

Mulkay, M. (1979), *Science and the Sociology of Knowledge*, Boston: George Allen & Unwin Ltd.

Musgrave, A.E. (1973), "Falsification and Its Critics", in *Logic, Methodology and Philosophy of Science*, (Proceedings of the Fourth International Congress for Logic, Methodology and Philosophy of Science, Bucharest, 1971, Vol. 4), Patrick Suppes et al (Eds.), Amsterdam: North-Holland Publishing Co.

O'Connor, D.J., (1967), *John Locke*, New York: Dover Publications.

O'Hear, A. (1982), *Karl Popper*, The Arguments of the Philosophers, London: Routledge and Kegan Paul.

Pears, D. (1970), *Ludwig Wittgenstein*, New York: Viking Press.

Popper, Karl (1962), "Some Comments on Truth and the Growth of Knowledge", in *Logic, Methodology and Philosophy of Science*, (Proceedings of the 1960 International Congress), E. Nagel, P. Suppes and A. Tarksi (Eds.), Standford: Standford University Press.

__________(1964), *The Poverty of Historicism*, New York: Harper, Torchbooks.

__________(1968a), *Conjectures and Refutations: the Growth of Knowledge*, Harper Torchbooks, New York.

__________(1968b), *The Logic of Scientific Discovery*, New York: Harper Torchbooks, 2nd ed.

__________(1968c), "Remarks on the Problems of Demarcation and of Rationality", in *Problems in the Philosophy of Science*, (Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Vol. 3), I. Lakatos and A. Musgrave (Eds.), Amsterdam: North-Holland Publishing Co.

__________(1970), "Normal Science and its Dangers", in *Criticism and the Growth of Knowledge*, (Proceedings of the International Colloquium in the Philosophy of Science, London, 1951, Vol. 4), I. Lakatos and A. Musgrave (Eds.), Cambridge: Cambridge University Press.

__________(1971), *The Open Society and Its Enemies*, Vol. I and II, Princeton: Princeton University Press.

__________(1974a), "Ad Hoc Hypotheses and Auxiliary Hypotheses. The Falsifiability of Newton's Theory", in *The Philosophy of Karl Popper*, P.A. Schilpp (Ed.), Vol. II, The Library of Living Philosophers, Lasalle, Illinois: The Open Court Publishing Co., 1974.

__________(1974b), "Ayer on Empiricism and Against Verisimilitude" in *The Philosophy of Karl Popper*, P.A. Schilpp (Ed.), Lasalle, Illinois: The Library of Living Philosophers Inc.

__________(1974c), "Kuhn on the Normality of Normal Science", in *The Philosophy of Karl Popper*, P.A. Schilpp (Ed.), Vol.II, The Library of Living Philosophers, Lasalle, Illinois: The Open Court Publishing Co., 1974.

__________(1974d), "Lakatos on the Equal Status of Newton's and Freud's Theories", in *The Philosophy of Karl Popper*, P.A. Schilpp (Ed.), Vol. II, The Library of Living Philosophers, Lasalle, Illinois: The Open Court Publishing Co., 1974.

__________(1974e), "My Solution of Hume's Problem of Induction", in *The Philosophy of Karl Popper*, P.A. Schilpp (Ed.), Vol. II, The Library of Living Philosopers, Lasalle, Illinois: The Open Court Publishing Co., 1974.

__________(1977), "The Logic of the Social Sciences", in *The Positivist Dispute in German Sociology*, T.W. Adorno et al, translated by G. Adey and D. Frisby, London: Heinemann.

__________(1981), *Objective Knowledge - An Evolutionary Approach*, Oxford: Clarendon Press.

__________(1982), *The Open Universe - An Argument for Indeterminism -From the Postcript to The Logic of Scientific Discovery*, W.W. Bartley (Ed.), London: Hutchinson.

__________(1983), *Realism and the Aim of Science-- From the Postcript to the Logic of Scientific Discovery*, Totowa, New Jersey: Towman and Littlefield.

Radnitzky, G. and Andersson, G. (1978), "Objective Criterion of Scientific Progress? Inductivism, Falsificationism, and Relativism", in *Progress and Rationality in Science*, (Boston Studies in the Philosophy of Science, Vol. 58), G. Radnitzky and G. Andersson (Eds.), Dordrecht-Holland: D. Reidel Publishing Co.

Rickert, H. (1962), *Science and History: A Critique of Positivist Epistemology*, translated from the German by George Reisman, Princeton: D. Van Nostrand Co. Inc.

Schilpp, P. A. (1974), *The Philosophy of Karl Popper*, Vol. I and II, The Library of Living Philosophers, Lasalle, Illinois: The Open Court Publishing Co.

Trigg, R. (1985), *Understanding Social Science - A Philosophical Introduction to the Social Sciences*, New York: Basil Blackwell

Urmson, J.O.(1982), *Berkeley*, Oxford: Oxford University Press.

Watkins, J. (1969), "Comprehensively Critical Rationalism", *Philosophy - The Journal of the Royal Institute of Philosophy*, Vol. 44, No. 167, January, pp.57-61.

__________(1971), "CCR: A Refutation", *Philosophy*, January, Vol. 46, No 175, pp.56-61.

Zeitlin, I.M. (1981), *Ideology and the Development of Sociological Theory*, 2nd ed., Englewood Cliffs, N.J.: Prentice-Hall.