Scientific progress is a fact of history. The instances of this fact can be traced from Ptolemy to Copernicus, Newton to Einstein, Wave theory of light to Corpuscular theory. Priestley to Lavoisier, etc. However, the fact of scientific progress entails an enquiry into the pattern of scientific progress which pattern further necessitates an examination of the position of truth in the onward march of history of science. For, understanding of the position of truth follows from the understanding of pattern of progress. The present topic: ‘Scientific Progress and the Problem of Realism’ is an attempt to understand the pattern of scientific progress and the status of truth in the changing scientific or theoretical discourse. However, the pattern of progress depends on the nature of theories. In other words, nature of theories indicates the pattern of progress and, in its turn, the pattern of progress indicates the position of truth in it.

Accordingly, the research embodied in this thesis is carried out on the views of five considerably different philosophers of science: Karl R. Popper, Thomas S. Kuhn, Imre Lakatos, Larry Laudan and Stephen Toulmin. The present work is comprised of the following eight chapters:

1. Introduction
2. Understanding Theory and Theory-change
3. The Aim of Progress and the Falsifiable Hypothesis.
4. The Status of Progress in Paradigm-shift
5. The Research Programme and the Pattern of Progress
6. The Research Tradition and the Character of Progress
7. The Structure of Progress in Evolutionary Change.
8. Critical Evaluation
In chapter-1, **Introduction**, we have tried to show how 'truth' problematises scientific progress. Though science is usually deemed to be objective, it possesses, especially at its fundamental level, the human elements also. The historical study of scientific activities reveals a different picture of science than that of its popular and widely prevalent conception.

In the second chapter, **Understanding Theory and Theory-Change**, we have discussed some elementary ideas about the nature of scientific theories and the different forms of scientific change. Here we have firstly dealt with the relation of truth to theory. A theory may be formulated with the aim of describing the natural world or predicting the future phenomena, or explaining natural facts and solving the problems arising thereof, etc. In this regard, there are two approaches to scientific theories: realism and anti-realism. According to realism the world is real and scientific theories have the capacity to exactly pinpoint its reality. In view of the same, different versions of realism have been brought out in this chapter to see the position of truth in scientific theories. On the other hand, various versions of anti-realism outlined in this chapter, hold that scientific theories do not aim at truth, but are rather conventions or fictions about nature. In essence they are instruments for predicting natural phenomena. Secondly, in this chapter, we have pointed out that there is a gulf between scientific rules and methods and the actual practice of science. Here the actual or historical way of doing science has convinced us that there does not exist any fact-independent theory or any such logic which necessarily culminates into a theory. This is a crucial finding about scientific theories entailing a foundational methodological shift. Thirdly, we have attempted to point out different terms and conditions of scientific progress. If $T_2$ better explains the phenomena than $T_1$, then it can be considered as progress. But there are instances of such revolutionary changes in the history of science when it is very difficult to understand as to whether the rejection of $T_1$ by $T_2$ is really progress or not. However, some elementary ideas of progress have been outlined in this chapter.
The third chapter of this thesis is titled **The Aim of Progress and the Falsifiable Hypothesis.** In this chapter, we try to understand 'Scientific progress and the problem of realism' on the basis of Popper's falsificationist view of scientific change. The core of Popper's view of scientific change is that a scientific theory is no more than a falsifiable hypothesis. Many a hypothesis are fielded in astronomy, physics or geology, or for that matter in sociology, economics, or political science. But only those hypotheses can be deemed to be scientific theories which are falsifiable. For Popper such a hypothesis can be produced from any source such as mythology, metaphysics, religion or folklore etc. If such a hypothesis is falsifiable, it is scientific.

In Popper's view, science can make progress through such hypotheses, by recourse to the method of 'trial and error'. Scientists put their hypothesis on trial and if any error is found out thereof, the theory is refuted. Thereafter, the scientists do conjecture up another hypothesis. His book 'Conjectures and Refutations' is addressed to expound this method. A competing hypothesis or \( H_2 \) proposes a crucial experiment to refute the earlier theory \( H_1 \). However, the falsifying hypothesis i.e. \( H_2 \) which refutes the falsifiable hypothesis, i.e. \( H_1 \), is deemed to be of greater relevance in the ongoing scientific inquiry. In this process of falsification science makes progress. For Popper, such a progress aims at truth. Through falsification we reduce falsity from science; and reduction of falsity logically means an increase in the truth-content. So every step of falsification entails that we are proceeding towards truth. Popper says that falsification is a ceaseless process. In this process, we do not know when, where and how we arrive at the truth. However, what is assured is that our search is directed at the truth. Even if some steps sometimes go wrong, our direction towards the truth remains unchanging.

In the fourth chapter, **The Status of Progress in Paradigm-shift**, we have discussed Kuhn’s view of scientific change. For Kuhn, a fundamental scientific theory is no more than a paradigm. A paradigm is the construct of a scientist’s imaginative mind. A paradigm is a grand theory which is first
accepted by a scientific community and then every scientific activity of that community is determined by the accepted paradigm. Thus a paradigm is a closed framework whose change brings a total change in science. Theory\textsubscript{2} becomes acceptable on the rejection of Theory\textsubscript{1}. Such a kind of radical change is christened as scientific revolution by Kuhn. For him, a revolution is just a replacement of one theory by another. So, in this transition, there is no increase of knowledge, it is only a shift of the outlook. Paradigms are incommensurable. So, the question of understanding progress through paradigm-shift does not arise at all.

However, the efficacy of a paradigm depends on its followers. Kuhn exemplifies this with the tools of a carpenter. The tools may work well if a carpenter is skillful. Paradigms are, likewise, tools in scientists’ hands by which they solve the puzzles about the natural world. It indicates that a paradigm does not aim at truth, rather it addresses itself to solving of scientific puzzles.

In the fifth chapter, The Research Programme and the Pattern of Progress, we have taken up ‘research programme’ view of science advocated by Lakatos. The phrase ‘research programme’ indicates that fundamental scientific products are not isolated theories, but rather a series of theories which Lakatos calls a ‘research programme’. This view is different from that of Kuhn and Popper. For Kuhn, there occurs a revolution and as a result the earlier theory is rejected. Popper holds that with falsifying hypothesis the earlier hypothesis is refuted and rejected. But Lakatos holds that there is no such instant rejection of the old theory. Every theory has its ‘relative autonomy’. A crucial experiment may declare anomalies of a theory, but the theory may not be rejected by the scientists. For Lakatos, auxiliary hypotheses make a protective belt which prevent the refutation of the core hypothesis. It is further strengthened by the heuristic power of a ‘research programme’. So, scientific change occurs not in the form of an instant revolution. It is rather a slow process. For Lakatos, the ‘degenerating problem-shift’ of the earlier theory and
‘progressive problem-shift’ of the later theory constitute the real reason of or
ground for scientific change. However, in this view, a theory may discover
facts, but those facts are oriented under the influence of a particular ‘research
programme’. So, increase in discovering of facts does not necessarily mean the
increase of truth-content.

In the sixth chapter, The Research Tradition and the Character of
Progress, we have deliberated upon research tradition view of scientific
change. The phrase ‘research tradition’ indicates that our discussion is based
here on Laudan’s view that fundamental scientific theories are no more than
‘research traditions’ which determine all our scientific activities including all
the problems, even if they be metaphysical or worldview problems. According
to Laudan, since science is a problem-oriented enterprise, scientific change
occurs on the basis of ‘problem-solving-effectiveness’ of the traditions. A
research tradition with greater ‘problem-solving effectiveness’ supersedes that
tradition which is of less ‘problem-solving-effectiveness’. So, it is an obvious
progress of science. But this progress is not committed to realism. For, a theory
aims at ‘problem-solving’, not at truth; a research tradition is evolved for the
purpose of solving the problems. In this regard, Laudan holds that the solution
of the problems does not necessarily mean attainment of truth. A solution is a
solution of our problem which is totally a human product.

In the seventh chapter, The Structure of Progress in Evolutionary
Change, we have worked out Toulmin’s view that a theory-change is like an
evolutionary change. Every theory is an organism and scientific discipline is a
species. Every theory endeavours to survive in the intellectual environment. In
evolutionary change, a theory changes in terms of development from lesser to
greater perfection. So, there is a continuity in science – the new theory emerges
out of the old. This kind of change is an obvious progress. The goal of this
progress is not transcendental truth, but rather the truth that is conceived of in
the process of evolution of concepts. This progress is directed at the intellectual
ideals which are again changeable in the course of conceptual evolution.
In the eighth chapter, Critical Evaluation, we have made a critical evaluation and found that the ideas of science developed through the discussion of different views of five philosophers are quite unlike the traditional view of science. According to the traditional view of science, scientific theories are produced out of inductive generalizations. A hypothesis is formulated to account for the observed facts. From this general hypothesis we deduce a particular conclusion which is then tested through a crucial experiment. In this view, scientists are seen as piling up facts, generalizing them into laws, and again piling up more facts step by step in the laboratory. If you can infer the laws from the accumulated facts, you can deduce the facts again from the laws, and the content of the laws is nothing but the facts. For traditional view of science, science ultimately aims at discovering the truth. Successive generations of scientists have filled in more and more parts of the complete true story of the world. This is a cumulative process of progress.

Our present study of the five philosophers of science leads us to doubt the traditional concept of scientific progress. Firstly, the new ideas oppose the concept of cumulative progress. Popper, Kuhn, Laudan and Toulmin all recognize the phenomenon of scientific revolution. Revolution, as opposed to accumulation, rejects the past achievements, and thus does not add to the piling up of facts. Secondly, scientific progress does not occur methodically, especially not in the way of inductive method. In Modern Physics, theories like quantum theory, relativity theory, theory of subatomic particles etc. are not formulated by way of inductive generalization. They are created by scientist’s imaginative mind. History of actual science reveals that science makes progress through the violation of the prevailing methods. If scientists obeyed the absolutistic mechanism of Newton, they could not have had the relativistic one of Einstein.

Another important part of scientific method is crucial experiment. As opposed to traditional concept of theory-change, our present understanding is that a crucial experiment cannot be decisive with regard to the acceptance or
rejection of a theory. There are several historical examples to support the contention that theories were not given up by the scientists despite the fact that crucial experiments brought out their anomalous character. The so-called facts are given to us in the light of our theoretical orientation. This is so mainly because observational facts on the basis of which an experiment is undertaken have lost their objective status. The theory-ladenness of facts leads us to the incommensurability of theories. As a result, we are not in a position to make a rational or methodical judgment on theory-change.

For the same reason, it is difficult to accept a realistic account of scientific change. Unlike scientific realism, it is quite untenable to maintain that the unobservable entities that theoretical terms indicate do exist. Many such theories were proved false and rejected by scientists. Even the most lenient version of realism asserting that science at least aims at truth though not achieving it, is also difficult to maintain in view of Kuhn's discovery of sociopsychical elements in scientific activity. For him and others like Laudan, Lakatos and Toulmin, scientific theories are but instruments for predicting the natural phenomena or for solving the problems experienced by us. And therefore, better theory means a better instrument. Realism, on the other hand, maintains that a better theory leads us to better description of the world.

From the discussion of different views of different philosophers we have concluded firstly that scientific progress is negotiated through the actual science of the past. Kuhn may not recognize 'better' theory, but his 'dominant' paradigm means some progress registered by the scientific community. He calls it sociological progress. Secondly, we see that in no version of scientific progress, are we bestowed with truth as such. Popper may show that reduction of falsity logically means progress towards truth. But until we understand the truth itself, it does not mean much. In falsification we can only know as to what is not true without knowing as to what is true. Thirdly, we see that scientific theories are the constructs of imaginative human minds. This account of science is radically unlike the popular conception of science, which maintains
that scientific theories are discovered by some sacred methods. This study helps us to realize that it is not scientific method, which guides men, but it is men who guide scientific research, i.e., scientific research is guided according to human thought. Fourthly, we see that scientific theories are not ideology-free. Kuhn’s ‘paradigm’, Popper’s ‘falsifiable hypotheses’, Lakatos’s ‘research programme’, Laudan’s ‘research tradition’ and Toulmin’s ‘intellectual ideals’ — all are but ideologies of scientific research. So, scientific theories are ideologically loaded constructs, not detached and neutral discoveries by rational calculations. Fifthly, we see that scientific progress occurs regardless of everything. Science dictates its own ways of change and progress. Standards of appraisal, logic of discovery; nothing is strictly obeyed by science in its onward march. Finally, when the theory changes, thereafter methodologists think over as to why and how the change has occurred. This postdictory thinking indicates that there is freedom in scientific research also.
SCIENTIFIC PROGRESS AND THE PROBLEM OF REALISM

THESIS
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IN
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Md. Abdul Mannan

Under the Supervision of
Mr. Mohammad Muqim

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2005
Dedicated

To

Dr. Mir Akramuzzaman
(Professor of Physics, J.N.U., Dhaka, Bangladesh)

Who made me what I am
CERTIFICATE

The present thesis entitled, "Scientific Progress and the Problem of Realism" is a research work carried out by Mr. Md. Abdul Mannan under my supervision.

The topic has been assigned to Mr. Mannan by the Committee for Advanced Studies and Research, Faculty of Arts, in its meeting held in the month of October, 1999.

I recommend that he may be allowed to supplicate for the award of Ph.D.

Mohammad Muqim
(Supervisor)
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Chapter – 1

Introduction

Traditional understanding of science as objective study of nature and traditional understanding of scientific progress in terms of better and better description of the natural world, are compatible with realism. But in this thesis entitled ‘Scientific Progress and the Problem of Realism’, an attempt is undertaken to show that such an understanding is not easily maintainable; rather it is quite problematic – there are subjective elements in scientific enterprise.

This problem can be traced to the very character of science. On the simplest level, science is knowledge of the world of nature. This knowledge is constituted by recognizing the regularities in nature. Therefore, recognition of regularity in which, for example, the Sun and the Moon periodically repeat their movements, is a scientific knowledge. However, the mere recognition of regularities does not exhaust the full meaning of science. Regularities may be constructions of the human mind. Human mind normally likes to explore regularities and uniformities and does not like to see chaos in nature; so it may construct regularities even when none objectively exists. For instance, one of the astronomical laws of the Middle Ages was that the appearance of comets presaged a great upheaval, as the Norman Conquest of Britain followed the comet of 1066 (Encyclopedia Britannica, 1993, vol. 27, p.32).

Some insist that the genuine understanding of regularities demands explanations of the causes of the laws. However, in this realm where there is wide-spread disagreement. While Modern biology thrives on causal chains, modern quantum mechanics has given up the causal quest. Even if causation and explanation are admitted as necessary, there is little agreement on the kind of causes that are permissible, or possible in science. Causes once appealed to, have been rejected by modern scientists. If the entities which were once considered as real and now are considered as unreal, then the entities which are now considered as real may be considered as unreal in future. Therefore science is to be considered as knowledge of regularities that is subjected to some degree of skeptical reasoning (Encyclopedia Britannica. 1993, p.32).
Consider the line of development from Aristotelian mechanics through Cartesian to Newtonian to Einsteinian one. The salient feature of Aristotelian mechanics is that the ultimate cause of all motion is a prime, or unmoved, mover (God) that stood outside the cosmos. In addition to this the understanding of Physics and biology posits teleology in the cosmos. However, the Cartesian. Cartesian ‘matter’ and ‘motion’ are ateleological. Nor did Descartes’s system seem to need the active participation of a deity. The Cartesian cosmos was like a watch that had been wound up at the creation and continues ticking to eternity. This understanding of the world is also totally different from the Newtonian world. Newton’s three laws of motion and the principle of universal gravitation suffice to regulate the cosmos. Absolute space, for Newton, was essential, because space was the “Sensorium of God” and the divine abode must necessarily be the ultimate coordinate system. This world is again different from Einstein’s mechanics which redefines Physics as the study of relations between observers and events, rather than of the events themselves. What was observed, and therefore what happened, was now said to be a function of the observer’s location and motion relative to the other events. Therefore, no longer could physicists speak with confidence of physical reality, but only of the probability. Now the question arises as to which of the understandings represent the reality of the world, Aristotelian, Cartesian, Newtonian or Einsteinian?

The problem can be seen in another way. The understanding of physical science may be broadly divided into laws and theories. A law may be described as an empirical generalization that we accept as true. Each of these laws refers to single thing, property, process, etc., that is observable under specified experimental conditions. On the other hand, a theory is not an empirical generalization. It contains theoretical terms such as ‘electron’, ‘field’, ‘ether’, etc. These theoretical terms do not refer to things, properties, processes that are directly observable. It is because of such terms that theories cannot be put to direct test in the ways that laws can. This accounts for the fact that individual laws may survive when one theory is discarded and replaced by another.

Let us exemplify this distinction. The following statement is Boyle’s law: At a constant temperature, the volume of a given quantity of any gas is
inversely proportional to the pressure of the gas. This law is explained in terms of a theory of perfect gases. The theory of perfect gases postulates that an ideal gas would consist of perfectly elastic molecules. Moreover, it assume that the volume occupied by the actual molecules and the forces of attraction between them is zero or negligible. If we examine Boyle's law, we find that the terms refer to what is observable under specified experimental conditions. We can actually measure and therefore see the pressure of a gas. But if we examine the theory of perfect gases, we realize that we can not isolate a molecule in order to see if it is perfectly elastic. For this reason Boyle's law can be directly tested, but no such straightforward test exists for the theory about molecules.

However, there are linking principles which serve to relate the theoretical terms to the observational laws. But such a relation is not one-to-one. We do not necessarily have one, and only one, linking principle for each theoretical term. Sometimes there is one; sometimes there are many; sometimes there is none. For example, there is a linking principle for "mean kinetic energy of the totality of molecules", but there is no linking principle for "kinetic energy of an individual molecule" (Brody, 1968, pp. 5-6). At this point, the theoretical terms that do not refer to anything we can observe become so mysterious. Therefore, it can be asked whether or not the things referred to by the theoretical terms are real or mythical?

The nature and character of 'experiment' may also lead towards an understanding of problem of realism. Most people agree that knowledge is the aim of science; that all knowledge starts with experience. We know that observation and experiment furnishes the basis upon which all science is built. Experiment is the method of modern science. An experiment is designed according to hypothesis and carried out under some special conditions in a laboratory; it is more restricted and will organised than what we ordinarily call experience. But the mere collection of facts is not yet science; it is the systematic and ordered account of our experiments that represents science. We wish to connect various facts we have accumulated in a particular field, and so we need to create theories to interpret our experiments. Thus theoretical language is intertwined with the experiment and we cannot separate what is the contribution of language and what is contribution of experiment. In this way a situation may arise when a false theory may become quite successful. Hence,
scientific progress consists not merely in new experiments, but also in the adoption of theoretical terms (Hutten, 1956, pp. 11-13).

The problem lies also in the character of scientific theory. A physical theory consists of a formal calculus and of an interpretation. But the relation between calculus and interpretation is in fact not unique. A single calculus may be interpreted in terms of various concepts, moreover, we often have several calculi that may be interpreted by the same set of concepts. It follows that we may have various ways of speaking about our experiments. We can construct several theories covering the same field of experience. Nature allows us a wider latitude for its description than is usually believed. For example, the ‘wave-language’ and the ‘particle-language’ represent equivalent descriptions of light. No possible test by experiment is satisfied by the one system which is not also satisfied by the other. Both are equally acceptable in physical science. This far-reaching freedom in the choice of the concepts by means of which we describe nature is characteristic of modern science (Hutten, 1956. pp. 170-171).

Every description of nature is also an interpretation whenever we use language to describe our experience, whenever we take words to refer to some events or other, it is for us to decide what we want our words to mean. For, the events do not speak for themselves, and language is the instrument we have invented in order to describe them. The way we speak reflects our attitude towards the events described, and the language we adopt thus expresses a certain interpretation (Hutten, 1956, p. 176). For example ‘Atom’ may be interpreted according to the mechanical model in kinetic theory, or according to the planetary model or to the vector model in quantum theory; or in terms of energy states, of charge-clouds, and of probability distributions, in quantum mechanics. We have multiple interpretations for a given expression (Hutten. 1956, p. 202).

Interpretation has a role also with the experiment. An experiment may have been carried out correctly according to accepted procedures, but it does not say anything unless we make it speak. It is experiment only if it is designed to prove or disprove a hypothesis, we must formulate what we intend to do and what we mean to prove by so doing – otherwise the experiment says nothing. Experiment is controlled experience. Experience by itself is ‘silent’ and it
requires a hypothesis to give it voice. Nature is not a book, and certainly not an open one, which we have merely to read when we do science. Things, events, and facts do not speak, the scientist does (Hutten, 1956, p. 223). Moreover, new theories may introduce new methods for experimentation. Also there are many psychological problems and questions which creep into any explanation of scientific method (Hutten, 1956, p. 222). These are among the conditions that ground the problem of realism.

This problem of realism is related to scientific progress. Progress always presupposes a goal towards which it makes its forward march. What is that goal? Is that goal the reality of the world? Or is that goal something other, for example, instrumental effectiveness? If the aim of the progress is reality, how does science through its theory-change get nearer to the reality? If theory change in science results in the rejection of the earlier theory then how do we conceive of the later theory. Is it better and therefore an advancement over the earlier one? If in some sense or the other, it makes such a progress, then how do we know that the progress has occurred towards reality at all? To understand progress, we need some regulative standard by which the merits of successive theories can be judged. In the philosophy of science the debate is not only about the kind of standard but also about the very possibility of finding any standard at all. According to Popper, truth content is such a standard by which successive theories can be judged. For him, if the later theory falsifies the earlier one, we have eliminated some falsity-content from our previous knowledge. Therefore, the later theory can be said to be nearer to truth. However, Popper is opposed by Kuhn. For him, there is no such regulative standard to judge the merits of successive paradigm, for they are all mutually incommensurable. Acceptance and rejection of successive theories are dictated by various socio-psychical factors, not by any logical considerations. So, through such a theory-succession even progress cannot be conceived, let alone the truth. Lakatos tries to reconcile between the standpoints taken by Popper and Kuhn. For him, scientific decision can not be irrational. The rationality of theory-change is “progressive vs degenerating problem-shift”. However, rationality is maintained at the cost of truth as aim of science. In maintaining rationality he goes against Kuhn and favours Popper, but in rejecting truth he goes against Popper and favours Kuhn. For Laudan, the standard of measuring the merits of successive theories is ‘problem-solving
effectiveness’, which again gives up truth as the goal of progress. For Toulmin, there is no such static or absolute unchangeable rationality. Rather reason too undergoes the process of evolution and the concept of truth as well. Rejection and acceptance of a theory is decided by ‘the struggle for survival’. In this evolutionary process, progress is again conceived at the cost of truth.

While propositions are deemed to be true or false, physical objects or metaphysical entities are deemed to be real or unreal. A proposition, such as, ‘this is a white pen’ is true if an object, white pen, really exists out there. In other words, if the object ‘white pen’ is real, the proposition ‘this is a white pen’ is true; or if the proposition ‘this is a white pen’ is true, the object ‘white pen’ is real. Therefore, ‘truth’ and ‘reality’ are two sides of the same coin. ‘Reality’ qualifies or characterizes an object or entity whereas truth qualifies or characterizes a proposition. Scientific theories are propositions or conjunctions of propositions. When we say that a scientific theory is true, it means that the entity that is described in this theory really exists. Realism about scientific theories asserts that the entity that is described by the theory really exists, and therefore the theory is true. However, realism in the content of present thesis is not merely concerned with the nature of scientific theories asserting that theoretical entities really exist. Here we are primarily concerned with realism as linked to the phenomenon of ‘scientific progress’, a relation of successive scientific theories. Realism about scientific theories explores as to whether the theory aims at describing reality, or the entities described by the theory really exist or not. On the other hand, realism about scientific progress explores as to whether a better theory is really a better description of the world, or later theory is closer to reality than an earlier one i.e. whether theory₂ is closer to reality than theory₁. So, realism with regard to progress is concerned with theory change, when one theory is replaced or superseded by another theory and, both theories are presumably trying to describe the reality.

We are not concerned with the realist stand with regard to external world which is challenged by idealism. For realists the world is independent of human consciousness or subjectivity. For idealists the external world is somehow dependent on human consciousness. This philosophical or ontological disagreement is not germane to our thesis. Our concern here is with the status of scientific theories. Do the entities assumed by higher science
really exist? The realist responses here will be affirmative whereas the instruments will forward a negative response. Furthermore, we are dealing with the question of the status of progress in the ongoing march of theory-change. How is a later theory superior to or better than the earlier one? In which sense is later theory an advancement over the earliest theory and advancement wherefo, in which direction. The realists will maintain that long drawn-out theoretical succession is geared towards reality. The instrumentalist as against the realist will deny any such direction of the scientific progress.

Realism about progress views the relation of the two theories where both describe reality and one is superseded by another. Realism about progress assumes that though theories describe reality, descriptions are not complete, and scientific progress through its gradual change tries to reach that complete description, which may be called complete theory, or absolute theory. So, scientific change aims at complete true theory, which will not, in course of change at all. Such a scientific change is called real scientific progress. However, analysis of such progress depends on the analysis of theories and other scientific activities which are associated with successive theories leading towards progress. The present thesis is an attempt to analyse the nature of scientific theories and also nature of the relation of the successive theories with a view to understand scientific progress and its impact over realism.

A question about the relation of the successive theories to be asked is: what makes a later theory better than an earlier one? The question can be answered in different ways. For example, it can be said that a later theory is better because it is a better description of the world. This answer presupposes that a scientific theory is nothing other than a description of the world, and therefore that description comprised of propositions (theory) can be either true or false. But how can one judge that a later theory is a better description of reality? We do not have any model or picture or map of reality to make a comparison between theory and reality. This can be done only by an analysis of the theory and theory-change.

The question as to what makes later theory better than an earlier one has also been answered on the grounds of simplicity, elegance and economy. There are many philosophers of science who claim that any later theory can be better
not for its greater truth-content, but for its greater simplicity or elegance or economy. Instead of being a better description of the world, a later theory may also be deemed to be having greater problem-solving effectiveness. However, these different perspectives on theory-change and scientific progress are assumed in a antirealistic framework may be called instrumentalism.

There are some such higher level theories which do not have any such connection with the observable world. Such theories are composed of the propositions which are hypothetical; again such propositions are composed of the terms which are hypothetical; even the words which define those terms are also hypothetical. Moreover, to observe the empirical consequences, we require some other theories between those higher level theories and factual states of affairs. In this way, such theories remain so distant that any direct connection with the observable world becomes impossible of detection or articulation. Therefore, such theories belong to very different realm than those theories which have some direct connection with the observable world. The logic of such theories becomes entirely different – different in their mode of judgement, standard of appraisal, criterion of meaning, and their structure and nature. The most important feature of their nature is that they are metaphysical in their character. They are pure products of scientific imagination. Every assertion, at this level is a theoretical construct in the sense that one theory is considered by another theory, and that theory is considered again by another theory etc. The understanding is caught in a theoretical circle. Only after elaborate deductions are we able to establish a semblance of connection with the observable or empirical world. This level of theoretical science is called revolutionary science. Scientific theories at this level are designated by different philosophers in different terms, such as, conjecture, paradigm, research programme, research tradition etc.

Regarding the theories at the level of revolutionary science, there is a widely accepted view that scientific change or progress is directed at goals and many such goals have been ever more successfully realized. Successive generations of scientists have been filling in more and more parts of the complete true story of the world. There may have been mistakes and false steps here and there, but there is an overall trend towards accumulation of truth, or.
at the very least, of better and better approximations to truth. There is also a modest view that science is directed at discovering truth about those aspects of nature that impinge most directly upon us. According to this idea, all this is achieved by a sacred method – Scientific Method. There are objective canons of evaluation of scientific claims. Methodologists should articulate these canons, so that we can forestall possible misapplications and extend the scope of scientific method into areas where human inquiry typically falters (Kitcher, 1993, p. 3).

But, if history is viewed as a repository for more than anecdote or chronology, it could produce a decisive transformation in the image of science (Kuhn, 1977, p. 1). The practice of science, past and present, is seen as raising doubts about whether the aims have actually been attained. For, history shows that there are numerous historical phenomena which indicate that the new theory has been accepted on the ground of the rejection of the old. Not only old theories are rejected, but old mode of appraisal, the old way of looking at the world too are rejected. For instance, in 1905, special theory of relativity and quantum theory were developed in drastic violation of all existing philosophies of science (Suppe, 1977, p. 10). These then serve as the basis for wondering whether the aims are attainable or not. This concern is further reinforced by adding perspectives of the biologist, the cognitive scientist, and the sociologist. If scientists, like other people, are recognized as biological entities, who have evolved under selection pressures; if scientists, like other people, are cognitive systems with identifiable limitations and deficiencies; and if scientists are embedded in complex networks of social relations; then the chances that their activities will result in the attainment of truth can seem to be decreasingly low (Kitcher, 1993, p. 6).

This realization essentially comes from the study of history of science. There are two aspects of it: internal history and external history. Internal history is all the cognitive relations: methods, standards, rules, theoretical schemes or empirical investigations; and external history is all the external relations: sociology, psychology, ideology, etc. Mary Hesse says that internal history of science considers science a history of rational thought about nature evolving according to its own inner logic; and external history of science considers science a social and cultural phenomenon inseparable from the whole social life. The former searches for reasons admitted consciously by scientists
in their work, the latter for causes which affect the development of science and usually operate subterraneously. Both these approaches are historically significant, for science is a function of both reasons and causes (Krajewski, 1977, pp. 115-118). The recognition of external–internal factors entails the recognition of two different approaches to science: history of science and philosophy of science. The historians deal with facts and data, seeking to arrange them into a convincing and coherent tale about how scientific ideas have evolved. Philosophy of science, by contrast, is commonly perceived as a normative, evaluative and largely a priori investigation of how we ought to proceed. On this view, the gap between history and philosophy of science is as broad as the divide between matters of facts and matters of value. However, although they are radically different approaches, they are complementary to each other.

In his “Towards an Historiography of Science” J. Agassi shows how much the historical writings about science are laden with implicit philosophical assumptions - assumptions which decisively determine the character of the history that is produced. To take an overly simple example, if a historian is convinced that experiments can be the only decisive grounds for abandoning a theory, then history will tend to focus exclusively on so-called crucial experiments. The thesis is not merely that philosophical assumptions have influenced historical scholarship, but that they must do so. For, history has no neutral data, and treatment of any particular historical episode is influenced to some degree by one’s prior philosophical conceptions of what is important in science. History and philosophy of science are thus correlative. The aim of philosophical inquiry is the production of a set of norms, but any philosophical theory of science, which fails completely to square with the history of science, would be deemed unacceptable. Moreover, confronted with a philosopher’s account that entails that the whole of the history of science is irrational, we would tend to view that as a reductio ad absurdum of the theory of rationality rather than as a demonstration that science itself has been a sequence of entirely irrational preferences. Thus, history of science presupposes a philosophy of science, and philosophy of science is then to be authenticated by its capacity to lay bare the rationality implicit in the history of science (Laudan, 1978, pp. 155-156).

Despite being dependent upon each other, history of science and philosophy of science have their independent nature. For this reason, we see
that all the available philosophies of science can do scant justice to history of
science. There are multiple instances of disagreement amongst them. If history
could evolve just according to a particular philosophy, then such a situation
would not have arisen — history and philosophy of science would have only
been in accord with each other. In this connection, we should maintain the
distinction between two types of history of science: HOS\(_1\) and HOS\(_2\). History
of science, HOS\(_1\) can, at a first approximation, be regarded as the
chronologically ordered class of beliefs of former scientists. Historians describe
just discoveries and inventions and successive formulations of laws and
theories. On the other hand, history of science, HOS\(_2\), is writing about the
history of science - the descriptive statements which historians make about
science. It tries to discover regularities in the growth of science and in the
change in its methods. Ronald Giere argued that the philosopher must not
become a slave to HOS\(_1\) because one of his primary roles is to criticize the
theories of the past. However, philosophical theses cannot be completely a
priori; they must capture some of our pre-philosophical hunches about which
theories are rational and which are not. If these hunches do not come from
HOS\(_1\), where do we obtain them? This position of history and philosophy of
science raises the normative-descriptive paradox (Laudan, 1978, p. 158).

According to Laudan, there is a possible way out of this paradox. For
him, within HOS\(_1\), there is a subclass of cases of theory - acceptance and theory
rejection, about which most scientifically educated persons have strong
normative intuitions. For example, (1) It was rational to accept Newtonian
mechanics and to reject Aristotelian mechanics by, say, 1800; (2) It was
rational by 1890 to reject the view that heat was a fluid; (3) It was rational for
physicians to reject homeopathy and to accept the tradition of pharmacological
medicine by, say, 1900; (4) It was rational after 1920 to believe that the
chemical atom had no parts; (5) it was irrational to believe after 1750 that light
moved infinitely fast; (6) It was rational to accept the general theory of
relativity after 1925; (7) It was irrational after 1830 to accept the biblical
chronology as a literal account of earth’s history. However, this sort of
intuition Laudan calls pre-analytic intuition (PI) about scientific rationality. So,
philosophy of science aims to explicate the criteria of rationality implicit in our
pre-analytic intuition about certain cases within HOS\(_1\). And the authentication
of any philosophical model requires careful research in HOS$_2$ in order to assess the applicability of that model to the cases that constitute pre-analytic intuition. Thus, the normative-descriptive paradox is resolved (Laudan, 1978, pp. 160-161).

This position of inquiry about scientific change may be called the Rational reconstruction of history of science. This is a joint mode of history and philosophy that is thought to be capable of dealing with the fact that the norms of rational evaluation change through time. Any theory of norms, if it is to be applied historically, must take into account the fact that previous scientists had norms of their own that cannot be ignored while explaining their cognitive stances with respect to the theories of the day ((Laudan, 1978, p. 168). This type of enterprise is, however, not science; it is a non-scientific, epistemological formulation. This enterprise seeks the solution of three questions here: One, how do the new theories emerge?, how do the scientists, who are brought up in the old theoretical framework, transcend the old and think of the new?, two, why do scientists become convinced to accept the new theory in the face of the old theory, whereas there is no such a historical standard to make a comparison between the old and the new?, and three, what is the status of this shift from the old theory to the new?, is it advancement towards reality? However, these three questions are the three aspects of one single issue – pattern of scientific progress that, in turn, lies in the process of scientific change. This issue will be discussed here on the basis of the views of Karl R. Popper, Thomas S. Kuhn, Imre Lakatos, Larry Laudan and Stephen Toulmin.

In chapter-1, Introduction, we have tried to show how ‘truth’ problematises scientific progress. Though science is usually deemed to be objective, it possesses, especially at its fundamental level, the human elements also. The historical study of scientific activities reveals a different picture of science than that of its popular and widely prevalent conception.

In chapter-2 Understanding Theory and Theory-change we have outlined some essential preliminaries about the nature of scientific theories and scientific progress. In this chapter we will see two views: realism and antirealism. Realism holds that the entities assumed by a theory really exist, but
antirealism denies this. According to antirealist view such entities are mere conventions on fictions. Here we will also discuss the general outlook regarding the nature of progress. Progress is a relational term; which kind of relation of the successive theories is called progress, is discussed here.

According to Popper, scientific theories are hypotheses only. He shows that scientific change is scientific progress. For Popper, as every new theory falsifies the old one, it means that the new theory is better than the old one. Although hypotheses in their nature are human constructions, they undergo severe tests, and therefore, the decision about them is objective. Such a decision about theory change may indicate an increasing approximation to reality. Nevertheless, when and at what point would we reach the truth we do not know. This position is discussed in chapter-3, The Aim of Progress and Falsifiable Hypothesis. The relation of ‘truth’ with ‘progress’ is discussed here in terms of the ‘aim of progress’ and the phrase ‘falsifiable hypothesis’ indicates that our analysis is carried out on the basis of Popper’s view that fundamental scientific products are only hypothetical or conjectural.

However, Kuhn’s account is radically or altogether different from that of Popper. According to Kuhn, a scientific theory is a ‘paradigm’ in its nature. A paradigm is constructed by a scientific genius. Scientists accept the new and reject the old which makes a shift or replacement of paradigms. Therefore, such a change cannot be progress. He argues that since both the old theory and the new one are human products, such change cannot claim to have got nearer to reality. How Kuhn establishes such a skeptic position is discussed in chapter-4, The Status of Progress in Paradigm-shift. The relation of ‘truth’ with ‘progress’ is discussed here in terms of the ‘status of progress’ and the phrase ‘paradigm-shift’ indicates that our issues are debated on the basis of Kuhn’s view that scientific theories are no more than paradigms.

Lakatos tries to work out a reconciliation between these two positions. According to Lakatos, the fundamental scientific construct is not a single theory, but rather a series of theories which he calls Research programme. The advancement of research programme depends on how many facts are discovered and explained by the programme. But, in the final analysis, since these facts are human constructions, they cannot mirror the truth. This position is outlined in chapter-5, The Research Programme and the pattern of
Progress. The relation of ‘truth’ with ‘progress’ is discussed here in terms of the ‘pattern of progress’ and the phrase ‘research programme’ indicates that our issues are carried out on the basis of Lakatos’ view that the fundamental scientific constructs are not isolated theories, but rather a research programme.

However, Laudan goes one step ahead of Lakatos by holding that fundamental scientific theories are not a programme, but a tradition. This research tradition according to Laudan is rational. Laudan holds that science is a problem-solving activity. For him, ‘Research tradition’ rather than a single theory is the unit of scientific change. Research tradition determines which are the problems to be dealt with and what are the solutions to be accepted. Therefore, these are also human constructions. Moreover, he says, solutions do not necessarily mean truth. This position is discussed in the chapter-6, The Research Tradition and the Model of Progress. The relation of ‘truth’ with ‘progress’ is discussed here in terms of the ‘model of progress’ and the phrase ‘research tradition’ indicates that our issues are carried out on the basis of Laudan’s view that scientific theories are research traditions.

Toulmin goes yet another step farther from Laudan in maintaining that the standards of rationality too undergo the process of evolutionary change. Stephen Toulmin recognizes the time factor in scientific change. He holds that not only theory and concepts, but also their standards of appraisal and the concept of truth all are dynamic – nothing remains static. But at the same time, there is continuity in change. A scientific theory, like an organism, at every step of its development is quite different one, yet it remains the same. Such a pattern of progress is the process of evolution. Evolution is a sort of progress. But whether this progress is towards reality is difficult to say. For, even the concept of reality undergoes the process of evolution. This is shown in the chapter-7, The Structure of Progress in Evolutionary Change. The relation of ‘truth’ with ‘progress’ is discussed here in terms of the ‘structure of progress’ and the phrase ‘evolutionary change’ indicates that the analysis is carried out on the basis of Toulmin’s view that scientific theories are like organisms whose development occurs in the process of evolution.

In the chapter-8 Critical Evaluation we have shown that scientific progress does not occur in the traditional way of accumulation. And progress is not methodical, but creative. Finally we have shown that such a progress does not assure us with the attainment of truth.
Chapter – 2

Understanding Theory and Theory Change

2.1. Nature of Scientific Theory

*What is theory?*

Natural science deals with natural phenomena. A rational enquiry and explanation of these phenomena fructify into a theory. A theory is that which connects the observed phenomena and traces them back to a single pure phenomenon, thus bringing about logical arrangement. Physical theories provide patterns within which data appear intelligible. They constitute a conceptual gestalt. A theory is not pieced together from observed phenomena; rather it is what makes it possible to observe phenomena; they are built up retroductively. Theories put phenomena into systems. A theory is a cluster of conclusions in search of a premise.¹

However, a scientific theory is essentially a hypothesis. It is so because a theory is never conclusively or definitively proved. There is always the theoretical possibility that new evidence will be discovered which conflicts with some of the observational statements inferred from the hypothesis. Such an eventuality often leads to the rejection of the hypothesis. The history of science records many instances in which once accepted hypothesis was subsequently abandoned in the light of adverse evidence.²

According to O. Neil scientific theory is the supposition of the matters which have not yet been observed but which might have been observed if circumstances had been favorable. Take for example, Harvey’s theory of the motion of the blood. If he had possessed a microscope as Malpighi did, (some thirty years after Harvey published his supposition of the pores in the flesh linking the arteries and the veins, Malpighi using an early microscope saw them.), he would have been able to observe the fine capillaries joining the end branches of the arteries and of the veins. But as it happened, he was not favored by such circumstances and so his only resort was to suppose that there were such linking pores in the flesh.³

O’Neil says that he did this not simply because he was dissatisfied with the facts he had, and not at all because he was too impatient to wait for other
facts to come to him or too lazy to look for them. But rather he was puzzled by the incomplete set of facts he had. He thought that he could detect the general outline of the picture of which the known facts constituted some of the parts. He had some of the pieces of the jigsaw that are set in the right places, but he had no more pieces with which he would fill the remaining gaps. He proceeded then to imagine what the suspected missing pieces were like so that he could present a whole picture that made sense. Earlier he was in a puzzle because of the shortage of the facts, now he assumed additional items of ‘information’ in order to remove that puzzlement. Since this move is merely an act of imagination, acceptance of his known facts does not necessarily follow the acceptance of his suppositions. If his evidence had been inconsistent with the theory, the acceptance of the evidence would commit one to the rejection of the theory.\(^4\) Hempel says, scientific theory is essentially a hypothesis, for such a theory is never proved beyond any possible doubt. The possibility of new evidence dislodging a time-honoured theory can never be ruled out. The history of science is replete with examples of established theories being abandoned in the face of contradicting data.\(^5\)

O’Niel says that different theories may imply the same observed facts. Therefore, there arises the problem of deciding which of the alternative theories should we adopt. At this point, several criteria of choice might be offered. Other things being equal, the simplest theory is to be preferred. This criterion is sometimes called the canon of parsimony and sometimes Occam’s razor. William Oceam is credited with dictum: theoretical entities should not be multiplied beyond necessity. Copernicus propounded a simpler theory than Ptolemy. His principal economy consisted in accounting for the whole of the sun’s apparent motion and a component of the apparent motion of Mercury. Venus, Mars, Jupiter and Saturn by means of the single assumption that the earth was in motion around the sun. However, there is potentially no end to the alternative theories that would do equally well. Thus a theory is not necessarily the right one simply because it is the only one which one can think of to explain the facts relevant to it. There is always the possibility that some other theory will do as well or even better. Quite often alternative theories have been propounded differently from one another in ways other than
simplicity or economy of assumption. They may differ in respect of the range of the phenomena they explain or in the precision with which they explain the relevant phenomena. Furthermore, one may imply a state of affairs which is contrary to what observation reveals, whereas the other implies a state of affairs which is in accord with data supplied by observation.  

O’Neil mentions various types of theories: firstly, a theory that assumes the things that could be observed if circumstances were favorable; secondly, a theory that assumes the things that play a role like that played by any other important relevant fact; thirdly, theories which do not supply missing pieces of the jigsaw puzzle but which provide another sort of rationale for what has been observed. They perform this role by providing bases from which observed or observable situation may be inferred. Fourthly, there is another type of theory even better represented by Mendeleyev’s periodic table. It is not much the assumption of any given observable fact. However theories could be judged from two angles: truth-value and validity. Truth is judged through the factual consequences and validity through implication. False theories may formally imply conclusions, which are true, and true theories may imply those which are false.

Description, Prediction and Explanation

What is the function of a scientific theory? There is no readymade response which is perfectly congruent to the full import and load of this crucial methodological/ideological question. There can be multiple responses to this foundational query. For O’Neil, scientific theories are trifunctional, viz., they either describe or predict or explain a situation or a given state of affairs under consideration. The meaning of these three terms may be expressed as follows: (1) a description says what an object or a situation is, (2) a prediction is a fore saying about its occurrence, its features, etc., in advance of its being observed to occur or to have such and such features etc. and (3) an explanation shows how, or why, it is so or has become so.

Description of some object involves setting down its features and properties. Thus we may describe hydrogen as a gas at normal temperatures, as having an atomic mass of 1.008, a valance of 1, as being highly
inflammable and as having one proton in its nucleus and one electron in orbit around its nucleus. The first four items in this description of hydrogen have been derived from its observation whereas the fifth component is derived from theorizing aimed at a rationale for some observed properties of hydrogen and the other elements. Thus though descriptions are usually thought of as being statements about the observed characteristics of situations and objects, it may be necessary to recognize hypothetical descriptions. Adams and Leverreir offered some hypothetical descriptions of Neptune before its observation and Mendeleyev offered quite detailed hypothetical descriptions of scandium, gallium and germanium. Granted that a description is not confined to those statements derived directly from observations, descriptions may be explanations, and may be predictions also. Bohr’s description of sub-atomic structure of the elements explains Mendeleyev’s periodic table, and Newton’s description of the forces operating on particles of matter and of their modes of operation explains the planetary motions as conceived by Kepler.⁹

O’Neil says that the word ‘explanation’ is derived from the Latin verb ‘explanare’ which means ‘to spread out’, ‘to make clear’. There are two views of what constitutes explanation. One usage refers to giving the meaning of words or of the concepts signified by words. Thus, we explain ‘mammal’ by saying that it refers to an animal which suckles its young. This is sometimes providing better-understood words equivalent to less well-understood words. Though the process is carried out through the medium of words, it is more connected with what the words signify than with them as words in themselves.¹⁰ It is like providing referential or dictional meaning. The second one refers to some state of affairs wherein we can show that it follows logically from premises which include some general rules, principles, laws; and which have not been shown to be untenable. That is, the explanatory premises should imply the facts to be explained and should not be contradicted by some facts other than those being explained. It may even be said that premises including a general rule, principle, or law do provide an explanation; whether it is tenable or not is another matter. The third stresses insight, intuition and even mystical revelation. It is illustrated by Kepler’s
appeal to regular solids for an explanation of the spacing of the planets in the solar system and to a less degree by Newton’s assimilation of the periodicity of the elements to the periodicity of the tonal scale.¹¹

Finally, prediction in science for O’Neil, is a foresaying before the observation of the event - but not necessarily before the occurrence of it. There are three kinds of predictions in science. The first kind of predictions arises from the rounding out of a descriptive theory. An example of the case is Mendeleyev’s anticipation of unobserved elements or the anticipation of the basis of Bode’s law of planets between Mars and Jupiter. The second kind of predictions arises from the provision of additional premises needed to enable an explanatory theory to explain known facts. An example is the Adam’s and Leverreir’s anticipation of a planet roughly corresponding to Neptune later observed by Galle. The third arises as the implications of a theory formulated to explain other facts. Example is the implication of Slellar’s parallaxes by the heliofocal theory.¹²

Realist Approach to Science

What is the aim with which a theory is formulated ? Does a theoretical formulation describe the reality of the world, or is it designed merely for predicting (or explaining) the natural phenomena ? In this regard, there are two different views : Realism and Anti-realism.

According to realism the essential aim of a theory is to describe the reality, in other words, the theoretical formulation refers to the reality. However, this position of science depends on the question as to whether the world is real or not, and whether human scientific research is capable of knowing the reality or not. This position is summarized in the following lines:

Metaphysical Realism: Metaphysical realism is a doctrine upholding that there are truths about the actual structures of the real world that do not depend on the cognitive capacities of human investigators.¹³ This view asserts that there is exactly one true and complete description of ‘the way the world is’. Giere says that metaphysical realism is not a thesis about any theory now known, it is a thesis about some possible theory. This possible theory could not consist of a family of related models. It would have to be
just one big model. Every aspect of this model would have to correspond to a feature of the world, and there could be no feature of the world left out. The model would have to be unique. No other model could do an equally good job—this model is not only unrestricted, but also complete and perfect.\textsuperscript{14}

\textit{Commonsense Realism:} Commonsense realism asserts the existence of the observable entities of science and commonsense in the sense that our commonsense is able to observe the reality. This concerns not simply observed entities, but rather observable entities. Most observable entities have never been observed, yet in ordinary life and in science it is common to infer the existence of such entities. We observe the footprint in the sand, and infer the existence of a person. We observe the movements of Uranus, and infer the existence of Neptune. Commonsense realism does insist that such macroscopic objects, as Moon or Sun that we perceive do exist independently of our perceptions of them. All perceivers might perish without their perishing.\textsuperscript{15} In the same way, it also asserts the existence of microscopic objects.\textsuperscript{16}

\textit{Epistemological Realism:} Epistemological realism is a doctrine that asserts that all possible qualifications notwithstanding, the real structures of the world are cognitively accessible to scientific investigations. It claims that certain forms of evidence or empirical support are so epistemologically probative that any theory that exhibits them can legitimately be presumed to be true, or nearly so.\textsuperscript{17} The epistemological realism deals with the relation between knowing subject and the object of knowledge. While the metaphysical realism deals with the ontological assumption about the existence of ultimate reality, i.e. things-in-themselves, the epistemological realism deals with the object of knowledge relating to subject. When reality is discussed in ontological language, then it is usually understood as `thing-in-itself` and when it is discussed in epistemological language setting a relation with knowing subject, then it is understood as `object of knowledge`. However, these objects of knowledge are mind-independent according to epistemological realism. The world of our experience is real and we have the capacity to understand that reality.
Essentialism: According to this view a scientific theory is search for the truth and only the truth. Scientific enterprise in its continuous discoveries will once be able to achieve the ultimate truth. Karl Popper mentions its three basic doctrines:

(1) The scientists aim at finding a true theory or description of the world, (especially of its regularities or laws) which shall also be an explanation of the observable facts.

(2) The scientists can succeed in finally establishing the truth of such theories beyond all reasonable doubt.

(3) The true scientific theories, describe the ‘essence’ or the ‘essential nature’ of things which lie behind the appearances. Such theories are neither in need of nor susceptible to further explanation. They are ultimate explanations, and finding them is the ultimate aim of the scientists.

Position of Truth in Scientific Theory

Given that the world is real, the observation is real, and scientific theories are capable of arriving at what is real, what is the position of truth in any particular theory such as wave theory or corpuscular theory of light at any given time? Hesse says that realism is a generic term for a number of views, all holding that theories consist of true or false statements referring to real or existing entities. This position is outlined in the following lines:

Scientific Realism: Scientific realism asserts that the unobservable entities that the theoretical terms indicate do exist. By supposing their existence we can give good explanations of the behavior and characteristics of observable entities, otherwise they would remain completely inexplicable. Furthermore, such a supposition leads to predictions about observable entities that are well confirmed; the supposition is observationally successful. Abductive logic thus takes us from hypothesis about the observed world to hypothesis about the unobservable one. Scientific realism is committed to independence of unobservable. This view is called strong realism, because this view is powerfully committed in its claim that theories are correct transcriptions about their entities. This realism is also called theory-realism,
because this view claims that objects of theoretical terms are true. Observable phenomena are explained not by the mere existence of, say, electrons, but by electrons being the way, our theory says they are. However, this view should not be misconstrued to the effect that all unobservable are to be deemed to be real. Scientific realism is not committed to all the unobservable of modern science. It does take those commitments, for the most part, at face value.¹⁹

According to Hacking experimental works provide the strongest evidence for scientific realism, because entities that in principle cannot be ‘observed’ are regularly manipulated to produce new phenomena and investigate other aspects of nature. The realistic argument is: If there were an unobservable entity U that would explain P, then such an explanation is not only good, but the best. So, probably U exists. Hacking has enriched this view by pointing out that such an explanation seems particularly good where P is not merely possibly observed but actively produced. Let us suppose that U exists. Now, if we do A, which should make U do something leading to P and we then go ahead, do A, and produce P, we have powerful evidence of existence of U.²⁰

Another argument is given by G. Maxwell you are looking at an orange. Now let you make an inventory in the light of the best available scientific knowledge. Such an inventory will include all sorts of things like protons, neurons, atoms, and so on, but not the orange. The stimulation of the retina is accomplished solely by protons permitted from the atoms comprising the surface of the material object in question. The material object just is this collection of submicroscopic particles and the relations that subsist among them. In seeing an orange then you are seeing some thing that is not really there, an illusion generated by the interplay of submicroscopic particles which you do not see that are really there.²¹ All this means that the unobservable are real.

Van Fraassen says that science aims at a literally true story of what the world is like; and acceptance of scientific theories involves the belief that it is true.²² To suppose that a theory is literally true would imply, among other things, that no further anomaly could, in principle, arise from any quarter in this regard.²³ But no theory can guarantee that further anomaly will not arise. This is a big question against scientific realism.
Convergent Realism: Convergent realism consists in following claims:

(1) Scientific theories are typically approximately true and theories that are more recent are closer to the truth than older theories in the same domain.

(2) The observational and theoretical terms within the theories of a mature science are genuinely referential i.e. referring to extralinguistic entities. There are substances in the world that correspond to the ontology presumed by our best theories.

(3) Successive theories in any mature science are such that they preserve the theoretical relations and the apparent referents of earlier theories.

(4) Acceptable new theories do and should explain why their predecessors were successful insofar as they were successful.

(5) The scientific theories should be successful (in problem solving). Empirical success of theories provides striking empirical confirmation for realism.

Taking the success of present and past theories as given, proponents of convergent epistemological realism (CER) claim that if CER were true, it would follow that the progressive success of science would be a matter of course. Equally, if CER were false, the success of science would be miraculous and without explanation.24

However, history of science goes opposite to CER. For instance, literally no one criticized the particle theory of light because it did not preserve the theoretical mechanisms of the earlier wave theory; no one faulted Lyell’s uniformitarian geology on the ground that it dispensed with several causal processes prominently characterizing the earlier catastrophist geology. Darwin’s theory was not criticized by most geologists for its failure to retain many of the mechanisms of Lamarckian evolutionary theory.25

Constructive Realism: This is a lenient version of realism. Constructive realism asserts that scientists construct theoretical models that they intend to be at least partial representations of systems in the real world. The primary relationship between models and the world is not truth or correspondence or
isomorphism, but similarity. But since anything is similar to anything else in some way or other, the claim of similarity must be limited to specific set of respects and degrees. The respects in which similarity may be claimed can only be those represented in the model. The term ‘constructive’ emphasizes the fact that models are deliberately created or socially constructed by scientists.\(^{26}\) Einstein says that science is not just a collection of laws, a catalogue of unrelated facts. It is a creation of human mind, with its freely invented ideas and concepts. Physical theories try to form a picture of reality.\(^{27}\)

Nature does not reveal to us how best to represent her. So our best construction may fail to represent the world. This is why it is suggested that constructive realism does not claim a theory to be literally but only approximately true. This kind of interpretation of scientific theory sounds to be antirealistic. According to Giere, an approximate truth is not a kind of truth, indeed, it may be said to be a kind of falsehood; approximately implies ‘not exactly true’, which means false. The similarity argument in constructive realism does not face the problem of ‘independent access’. All one directly experiences are one’s own sensations. How, then, could one compare one’s sensations with the world to determine whether they correspond to it? Since all one could experience is another sensation, one could at best compare two different sensations. In short, there is no ‘independent access’ to the world.\(^{28}\)

**Naive Realism and Critical Realism:** According to naïve realism some apparent objects of knowledge exist. Human beings are naturally naïve realists. In every practice, every person is like that we entertain a doubt only after some apparent error crops up. Therefore, naïve realism holds that reality lies in the apparent objects.

On the other hand, a popular example goes against naïve realism: a pencil in a glass of water appears as broken at the water surface but unbroken to a descending finger. Repeated experience of apparent errors tends to generate some distrust about the entire phenomenal chair and furniture of the universe. This is why some persons and scientists have jumped to conclusion that no apparent reality should be trusted. This view is called ‘critical realism’. For critical realists, an oversight of these errors leads to a
inexplicable metaphysical pluralism: speculating on what is real has generated several varieties of metaphysics. According to some, reality is changing, for others reality is unchanging, yet for others reality is eternal and again some hold reality to be evolving. However, the original doubts of critical realists remain unanswered as none of the metaphysical speculators or interpreters have been able to adverse clinching or conclusive evidence in support of their ontocosmological contentions. Whatever reality we are offered turns out to be only an interpretation of reality, say critical realists.

**Tentative Realism:** This view is committed to accommodate the historical fact that various dominating theories which were entitled to be true at respective points of time, got discarded in the course of history. That is, up to certain time a theory was regarded as true and after certain time the same theory was regarded as false. 'The word 'tentative ' means 'holding on'; both 'holding on tightly as long as an idea works' and 'holding on loosely enough to be willing to let it go whenever the idea fails to work'. This view accepts the fact that knowledge, including scientific knowledge, normally involves both some certainty and some uncertainty. We will go on the basis of best available scientific knowledge; if times deem a theory to be true then it is true, and if false then it is false. Unless we are not aware of the theory to be false we will continue to hold it to be a true theory. However, this view tantamounts to 'relativism'. The term itself is against realism as reality can never be tentative.

**Realism and Truth:** According to realism, the statements of a theory are either true or false, and that many of the entities referred to in a theory do exist. There are as many true entities in the real world as there are human beings, houses, stones, stars, and so on. A realist does not maintain that every hypothetical entity exists, nor does he maintain that every statement in a theory is to come up for instant judgment as either true or false. All he needs to maintain is that some hypothetical entities are real and some theoretical statements are true. To make the realist position clear two important concepts need to be introduced. They are 'reference' and 'demonstration'. A proper name like 'Henry' can be used to make reference to some one whether or not he is present. Demonstration, and its close relative indication, is typically acts
performed with or by a gesture, in which any entity is pointed out or pointed to. An entity can be indicated not in its absence but only in its presence. Only a present thing or entity can be indicated. A thing or entity itself is the final, incontrovertible proof of its existence. It should be clear that we can refer to many things that we cannot indicate, but that if we can demonstrate anything to which we have previously made reference, on this or other occasions, then we have proved that thing exists.\textsuperscript{31}

In a theory, there are theoretical terms, and they can be used to make verbal reference to hypothetical entities, whether or not these entities can be observed or be present to our experience. Not all theoretical entities are what they seem. Some are really nothing but picturesque or shorthand forms of complex expressions whose meanings are different from that which at first hearing one might suppose them to have. Thus, there are three principles of realism:

1. Some theoretical terms refer to hypothetical entities.
2. Some hypothetical entities are candidates for existence.
3. Some candidates for existence exist.\textsuperscript{32}

However, truth can not become constitutive of realism. Devitt says that we cannot demonstrate any link between our theory and the state of the world. If truth means correspondence to reality, then truth threatens realism. Realism does not strictly entail any doctrine of truth at all. A person could, without inconsistency, be a realist without having any notion of truth in his theory. A realist may take a thoroughly skeptical view of the need for explanatory truth. On the contrary, a person who has an explanatory notion of truth in his theory may not be committed to realism; firstly, he may not apply his notion to the right statements and secondly, his notion may be an epistemic one like warranted assertibility, which involves no commitment to an objective independent reality.\textsuperscript{33}

Devitt also says that realism cannot maintain the correspondence truth. Assume that we have established correspondence truth. So our commonsense statements are correspondence true or false in virtue of reference to observable like stones, trees, cats, and so on. Next, assume that we
established scientific realism, so unobservable like electrons, neurons, and curved space-time do exist. Then the most plausible view is that scientific statements are correspondence true or false in virtue of reference to those unobservable. In view of the same, if a theory is to correspond to reality, then there emerges a very strong argument against realism:

1. If the realist’s independent reality exists, then our thought/theories must mirror, picture, or represent that reality.
2. Our thought/theories cannot mirror, picture, or represent the realist’s independent reality.
3. Therefore, the realist’s independent reality does not exist.

This position conforms the view that no theory is entirely true or false.

For Devitt, if we hold to correspondence truth, then our attempt to explain reference for scientific terms will force us back to a description theory. But we see that most of the terms of the past theories do not refer, because there is nothing that the associated descriptions pick out. Our past theories were mostly wrong. Therefore, it would be probable that the terms of our present theories did not refer as well. So scientific realism could be preserved only at the expense of correspondence truth. It may be asked as to why do we need truth where we cannot set any link between truth and realism. Why not to by-pass the notion of truth. However, L.R. Baker says that to deny the need of truth is to commit “cognitive suicide”. Therefore, we have truth in such a position where our theory merely has to aim at it.

Anti-realist Approach to Scientific Theory

Constructivism: According to this view the actual methodology of science is profoundly theory-dependent. What scientists count as an acceptable theory, what they count as an observation, which experiment they take to be well designed, which measurement procedures they consider legitimate, what problems they seek to solve, and what sort of evidence they require before accepting a theory; all are in practice determined by the theoretical tradition within which scientists work. What of the world must be
there is determined by the theoretical tradition in which scientific community in question works. 

The problem is that scientists seem sometimes to be forced by new data to abandon important features of current theories and to adopt radically new theories in their place. These phenomena must be an example of scientific theories being brought into conformity with a theory-independent world, rather than an example of the construction of the reality within a theoretical tradition. In response to this problem, constructivism often asserts that successive theories in science that represent the sort of radical ‘breaks’ in tradition at issue are incommensurable. The idea here is that the standards of evidence, interpretation and understanding dictated by the old theory, on the one hand, and by the new theory, on the other, are so different that the transition between them cannot be interpreted as having been dictated by any common standards of rationality. It follows that the adoption of a wholly new conception of the world is completely within its own distinctive standards of rationality. There may be some common terms but they should not be thought of as having the same referents in the two theories.38

Conventionalism: According to Poincare (1854-1912) ‘every conclusion presumes premises. These premises are self-evident and need no demonstration, or can be established only if based on other propositions; and we cannot go back in this way to infinity, every deductive science and geometry in particular must rest upon a certain number of indemonstrable axioms’.39 On this view he developed a positivistic and conventionalist interpretation of scientific theory. He holds that basic assumptions of the sciences are convenient definitions or conventions, which cannot be validated either by a priori methods or by inductive generalization from experience. Our choice among the possible conventions, though suggested and guided by experimental facts, is, in the last analysis, governed by considerations of simplicity and convenience. He draws a sharp distinction between two principal types of scientific hypothesis: (1) Hypotheses of the first type are essentially unverifiable. They are products of the free activity of the mind that are imposed by the scientific mind on our scientific schemes. They can neither be confirmed nor refuted by experience. (2) Hypotheses of the second
type are ordinary generalizations; they can be verified or falsified by experimental procedures. Poincare gives more attention to the first type. This is intrinsically unverifiable. He insists that a hypothesis of this kind, although not verifiable by experience, is suggested by experience, and derives its value from its fruitfulness in the scientific interpretation of experience. The facts given in experience may be assimilated to any one of indefinite number of alternative hypothetical constructions. Each of these constructions is a product of the free activity of the mind, and the choice of among them is made in accordance with considerations of convenience. Thus unverifiable hypotheses are indeed conventional, but they are not arbitrary. Experience leaves us our freedom of choice but it guides us by aiding us to discern the easiest way. Conventionalism holds that hypotheses are each internally consistent and are not inconsistent with the facts, for, the facts can neither refute nor confirm them.\(^{40}\)

Therefore, simplicity is the criterion of selection of one among the infinity of alternative hypotheses. We must choose and we can only choose the simplest. We are therefore led to act as if a simple law were, other things being equal, more probable than a complicated law. We choose the simplest law not because nature levels simplicity and thus the simplest is the objectively true, but solely in the interest of economy of thought. Assumptions are adopted not because they are true but because they are convenient. Experience tells us not which is the truest, but which is the most convenient. Conventionalism provides a simple, precise and economical language for the expression of our knowledge of nature. According to this view, ordinary language is too poor; it is besides too vague to express relations so delicate, so rich and so precise. The basic assumptions regarding force, inertia, absolute space, and absolute time are conventional in the sense that they are neither verifiable nor falsifiable. Conventionalism seeks to combine the precision and rigor of rationalism with the experimental fertility of empiricism.\(^{41}\)

Duhem also supports this view. His reasoning in support of this thesis is the following: (1) The logic of every disconfirmation, not less than of every confirmation, of a presumably empirical hypothesis \(H\) is such as to involve at
some stage or other entire network of interwoven hypotheses in which H is ingredient rather than the separate testing of the component H. (2) It is an elementary fact of deductive logic that if certain observational consequences O are entailed by the conjunction of H and a set A of auxiliary assumptions, then the failure of O to materialize entails not the falsity of H by itself but only the weaker conclusion that H and A cannot both be true. The falsity of H is, therefore, inclusive in the sense that the falsity of H is not deductively inferable from the premise \([(H.A) \rightarrow O]. \sim O\).\textsuperscript{42} According to conventionalism a true theory is not a theory which gives an explanation of physical appearances in conformity with reality. It is theories that represent in a satisfactory manner a group of experimental laws. A false theory is not an attempt at an explanation based on assumptions contrary to reality. It is a group of propositions that do not agree with the experimental laws. Thus the reduction of physical laws to theories contributes to ‘intellectual economy’ in which Ernst Mach sees the goal and directing principle of science.\textsuperscript{43}

**Fictionalism:** Theories are fictions, according to fictionalism. They read like factual accounts, and ostensively work according to the logic of description and reference, but they are products of the imagination and nothing else. They are like a novel. The episodes of a novel are coherent, and they may be quite convincing, i.e. have the ring of truth, but the key point of difference is this: the people who figure in the novel are not real people. Their names and addresses do not refer to real people and places. Art of novel-writing consists in constructing characters and endowing them with characteristics so plausibly that we are willing to treat them as real people. And we do this partly in the interest of entertainment, and partly for the insight we get into character. Because the laws of nature, as it were, according to which the characters of fiction are delineated, must be truths about real human characters. The entities gain plausibility. But the entities themselves have no more reality than the characters of fiction, and the terms which are used to describe and particularly to refer to them are like the names and addresses of characters in novels.\textsuperscript{44}

Hans Vaihinger(1852-1933) in his philosophy of the ‘as if’(Philosophe Des Als Ob),1911, advanced a positivistic and fictionalistic theory of
knowledge. His main insight is expressed by the term ‘As if’. The central contention of the philosophy of ‘as if’ is that the concepts and theories achieved by mathematics and the natural sciences, by economic and political theory and jurisprudence, by ethics, aesthetics and philosophy, are convenient fictions, devised by the human mind. He maintains that fictional constructions of the mind contradict reality and, in case of the boldest and most useful fictions, are even self-contradictory. Fictions are not, however, entirely divorced from experience—they are not invented in vacuum. Sensations are the starting point of all logical activity and at the same time the terminus to which they must return. This, of course, does not mean that fictions are verified by sensory experience—their fictional character precludes all verifications—but merely that sensory experience provides the occasions, for the stimulation and exercise of minds’ inventive activities. It also provides the spheres for the employment and application if it’s fictional constructs. The material of sensation is, according to Vaihinger, radically transformed by the thought; it is remodeled, recouined, compressed, purged of dross and mixed with alloys by the psyche itself.⁴⁵

Vaihinger’s fictionalism stresses the free, creative and inventive activity of the mind or psyche in the constitution of concepts and theories. Many of the creative activities of the mind are originally unconsciously performed and only later enter into consciousness; others are deliberately and consciously performed. But whether consciously or unconsciously devised, the resultant fictions are mental in structure. This position is by virtue, idealistic. According to this, logical thought is an activity, which fulfills the biological functions of assisting the organism to adopt and accommodate itself to its physical and social environment. This view remains at variance with pragmatism. The typical pragmatist ascribes ‘truth’ to his conceptual schemes—a truth that is attested by their practical consequences. But fictional constructs although are in contradiction to reality, have predictive function—by their help we are enabled to calculate events that occur. Although in themselves are fictitious, they lead to correct predictions regarding the future appearance of the sensations. And secondly, fictions devise hypotheses that are directed towards reality. A hypothesis is an ideational construct that
claims or hopes to coincide with some perception in the future. Fictionalism itself is the theory that distinguishes between fiction and hypothesis, and describes the role of fiction in knowledge as a hypothesis and not as a fiction.⁴⁶

**Instrumentalism:** Instrumentalism is Dewey’s version of pragmatism. This is a view of meaning and truth derived from the natural sciences and applicable to philosophy and science. Pragmatic principle of verification and meaning was propounded or characterized by William James, enunciated with remarkable theoretical precision by C.S. pierce and, developed and applied by Dewey among others. Dewey says that instrumentalism is an attempt to constitute a precise logical theory of concepts, of judgments and inferences in their various forms, by considering primarily how thought functions in the experimental determination of future consequences. The essential feature of pragmatism in its instrumental version is its references to consequences. The term ‘pragmatic’ means only the rule of referring all thinking, all reflective considerations, to consequences for final meaning and test. The meaning of judgment consists of its anticipated consequences and its truth is established by their actual verification. ‘All propositions which state discoveries or ascertainments, all categorical propositions, would be hypothetical, and their truth would coincide with their tested consequences’.⁴⁷

Traditional metaphysics and epistemology believe that reality lies behind and beyond the processes of nature; our search for reality transcends ordinary modes of perception and inference. Dewey thinks that such metaphysical and epistemological formulations are meaningless. He protests against setting up a universe as a system of fixed elements in fixed relations. Reality is, for him and the evolutionist, not a complete given, a fixed system—not a system at all, but changing, growing things. Dewey finds that experience and nature are not alien, but experience affords a means of discovering the realities of nature. So subject-object problem is here a pseudo-problem. Dewey sees in thinking an instrument for the removal of collisions between what is given and what is wanted—a means of realizing human desire. of securing an arrangement of things which means satisfaction, fulfillment, happiness. Such a harmony is the end and test of thinking.
Science in this sense is the goal of thought. When the ideas, views, conceptions, hypotheses and beliefs that we frame, succeed, secure harmony and adjustment, we call them true. We keep on transforming and changing our ideas until they work. The success of the ideas is not the cause or the evidence of its truth, but is truth itself. We must not separate the achieved existence from its process. All things are what they are experienced as and every experience is of something or the other. Things are also experienced aesthetically, morally, economically, and technologically; hence to give a just account of anything is to tell what that particular thing is experienced as. We must go to experience and see what it is experienced as. Here Dewey adopts an operational interpretation of a statement. According to instrumentalism, thinking serves human purposes, thinking is useful, and its utility is its truth. The human will is an instrument for realizing human aims. The so-called fixities such as atoms or God—have existence and import only in the problems, needs, struggles, and instrumentalities of conscious agents. So Dewey describes the inferences whereby we ascribe existence and properties to such scientific objects as atoms and electrons. This is merely to make predictions regarding perceptions registered under specified conditions.48

In his syntagma (lyon, 1658), P. Gassendi declares that all hypothetical or conditional statements should be considered to be ‘natural instruments’, i.e. devices by which knowledge can be made more orderly and penetrating.49 According to instrumentalism, a theory is a partially interpreted formal system. It is mere computational device or instrument that is regarded as logically meaningless because it does not refer to anything.50 Although theories do provide dispositional descriptions, they have nevertheless a purely instrumental function. When Galileo said of the Earth, ‘and yet it moves’, he uttered, no doubt, a descriptive statement. But the function or meaning of this statement turns out nevertheless to be purely instrumental; it exhausts it’s meaning in deducing certain non-dispositional statements. In their case, knowledge is power (the power to foresee).51

Bellarmino was willing to let Galileo say that the Earth moved around the sun provided he added that this was just a manner of speaking that is made for easier analysis and more effective inference. Bellarmino’s was an early
version of the instrumentalist view which holds theories to be neither true nor false but instead more or less effective instruments in the hands of the students of nature. Bellarmino suggested in effect that they are not contentions about fabric of the body of natural phenomena but are scalps and forceps in the hands of nature’s anatomist.\textsuperscript{52}

*Operationalism:* Operationalism in its fundamental tenets is closely akin to logical empiricism. Both schools of thought have put much emphasis on definite experiential meaning or import as a necessary condition of objectively significant discourse. Both have made strong efforts to establish explicit criteria of experiential significance. But logical empiricism has treated experiential import as a characteristic of statements, namely as their susceptibility to test by experiment or observation, whereas operationalism has tended to construct experimental meaning as a characteristic of concepts of the terms representing them, namely, as their susceptibility to operational definition. An operational definition of a term is conceived as a rule to the effect that the term is to apply to a particular case if the performance of specified operation in that case yields certain characteristic result. For example, the term ‘harder than’ might be operationally defined by the rule that a piece of mineral, X, is to be called harder than another piece of mineral, Y, if the operation of drawing a sharp point of X across the surface of Y result in scratch marks on the latter.\textsuperscript{53}

Scientific theories are unavoidably and inevitably defective and limited. The realization of this defect or limitation was a profound shock to all philosophers of science and in fact constituted a watershed in our articulation, crystallization and appropriation of operationalism. How, then, could science be devised which would be built up from concept, and which would be immune from vast revolutionary changes. Bridgman thinks that the only way in which this could be circumvented would be by confirming science to the application of those concepts that could be understood in terms of experimental operations, and then, whatever happens to theory, the content of the true scientific knowledge would remain unchanged.\textsuperscript{54} The operationalists require that each meaningful theoretical term be defined operationally; that is, specification must be given of the result of operations or experiments on
observable entities that would justify the application of the term. The terms do not refer to unobservable, but they are made up of terms referring to observable. Any theoretical term that cannot be so translated is meaningless, and should be banished from science.\textsuperscript{55}

This view is first advanced by P.W. Bridgman in the ‘Logic of Physics’ (1927). For him, what do we mean by the length of an object? We evidently know what we mean by the length if we can tell what the length of any and every object is, and for the physicists nothing more is required. To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed: that is, the concept of length involves as much as and nothing more than the set operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; the concept is synonymous with the corresponding set of operations. If the concept is physical, as of length, the operations are actual physical operations. Or if the concept is mental, as of mathematical continuity, the operations are mental operations, namely those, by which we determine whether a given aggregate of magnitude is continuous. It follows that every meaningful scientific term must be either definable exhaustively in terms of a specific and unambiguous set of possible operations, or be itself a term denoting such an operation.\textsuperscript{56}

To safeguard the objectivity in science, all operations invoked in this kind of definition, are required to be intersubjective, in the sense that different observers must be able to perform the same operation with reasonable agreement in their results. Bridgman distinguishes several kinds of operations that may be invoked in specifying the meanings of scientific terms. The principal ones are (1) what he calls instrumental operations. These consists in the use of various devices of observation and measurement, and (2) paper-and-pencil operations, verbal operations, mental operations, and the like—this group is meant to include, among other things, the techniques of mathematical and logical inference as well as the use of experiments-in-imagination. Hempel refers to them as symbolic operations. Symbolic operations always must ultimately make inference to some instrumental operations.\textsuperscript{57}
He mentions the following aspects of operationalism:

(1) 'Meanings are operational'. To understand the meaning of a term, we must know the operational criteria of its operation, and every meaningful scientific term must therefore permit of an operational definition.

(2) To avoid ambiguity, every scientific term should be defined by means of one unique operational criterion. Even when two different operational procedures have been found to yield the same results, they must be considered as defining different concepts (for example, optical and tactual length).

(3) Hypothesis incapable of operational test, or rather questions involving untestable formulations, are rejected as meaningless. If a specific question has meaning, it must be possible to find operations by which answer may be given to it. It will be found in many cases that the operations cannot exist, and the question therefore has no meaning.58

2.2 Rule versus Practice in Science

It is widely accepted that all activities of science are rule-bound as opposed to any other nonscientific enterprise. But if we take philosophical look at the position of facts, logic of discovery, and other rules of science as well, we shall get, in practice, a picture that is quite different from the widely accepted view. However, understanding the dichotomy between the methodological rules and the actual, practical and historical modes of doing science is essential for a judicious understanding of scientific discourse.

Fact-theory Relation

It is widely admitted that theory is a human creation and the fact is given. That is, facts represent some kind of reality. There exist some pure facts that are no less than reality. Fact is quite distinct from theoretical construct. So, when we justify acceptance of a theory we see whether there is agreement between the theory and the fact. If facts agree, the theory is accepted, if not, it is rejected. This is a very naïve understanding of the relation of theory to fact. But, after due philosophical consideration, such a relationship can hardly be sustained. Feyerabend holds that theories may be removed because of conflicting observations; and observations may also be
removed for theoretical reasons. This is because learning does not go from observation to theory but always involves both elements. Experience arises together with theoretical assumptions, not before them. He says that if we eliminate parts of the theoretical knowledge of a sensing subject, then we have a person who is completely disoriented and incapable of carrying out the simplest action. Even as to children's learning, the whole process starts only because the child reacts correctly towards signals, interprets them correctly, for, he possesses means of interpretation even before he has experienced his first clear sensation.\(^5^9\)

If theories and facts are intertwined, then how a theory can be tested against the data of experience, and how one theory can be said to 'account for the facts' better than another. Shapere says that test of a scientific theory can be accomplished if and only if there are at least some terms occurring in the theory which have a meaning independent of their theoretical context. And comparison of different scientific theories can be accomplished if and only if there are at least some such terms that have the same meaning in both of those different theories. If there is no such common meaning, the theories are not talking about the same things; hence cannot be compared with respect to their adequacy.\(^6^0\) A given theory rarely satisfies these conditions. This is because a theory is constructed in terms of hypothetical entities; the greatest advances in science have not been accomplished by means of laws referring explicitly to observations, but rather by means of laws that speak of various hypothetical entities. Electric, magnetic, and gravitational fields are among such hypothetical entities; they are not directly observable, and therefore, they are not theory-independent. Popper calls them metaphysical and speculative. In addition, the language representing the attributes of such hypothetical entities becomes theory-dependent.\(^6^1\) After the proved impossibility of Maxwell's demon and Heisenberg's principle of indeterminacy, men come to understand that the measuring instrument (man) is not simply a passive extension of our senses. Therefore, theory cannot be justified by the facts. Devid Hume argues that no matter how many tests confirm a hypothesis, it might still be wrong. Duhem argues that there is no such thing as a crucial experiment. No matter how unfavorable tests might
be, the hypothesis might be preserved by either adding assumptions or surrendering one of accepted assumptions. An individual hypothesis can never be tested in isolation of a whole group of other theories. Facts do not have power to make scientists accept the theory and reject it as well.62

This is widely recognized that theory emerges from the generalization of facts. In this sense, the facts are the source of theory – ‘fact precedes theory’. Fact gets the high status in empiricist account of relationship of man and universe. According to empiricism, the senses are the only channel of communication between the knowing subject and the world. The sense experience is the necessary factor in every act of acquisition and control of knowledge. For this view, man is measuring instrument that reacts by its feelers (senses) to the outside world and to its own state as well. Accordingly, scientific theories are abstracted from facts. Facts are the origin of theory. Every theory emerges from and reduces into the facts. All theoretical knowledge is epiphenomenal, and should be considered as an economical, convenient means of accounting for facts.63 However, when we ponder over Newtonian and Copernican theories or other such theories, we realize that none of them is the result of generalization of facts. These theories were formulated even when hundreds of facts did not fit in with them. Only by theory-articulation, scientists sought to work out conformity between theory and facts. From this fact of history of science, it follows that theory too plays an instrumental role in creating of facts. Consider the discovery of oxygen. The same thing is discovered by Priestly long before Lavoisier, but only after a comprehensive theory is constructed by the time of Lavoisier, was he able to recognize diphlogisticated air as oxygen. All this suggests that ‘theory precedes fact’. So, Duhem says ‘an experiment in physics is not simply the observation of a phenomenon, it is, besides, theoretical interpretation of this phenomenon.’64

Not only that, theoretical statements have their impact upon our accepting or refuting observational statements. Thus, for example, the acceptance or rejection of a statement concerning the existence or non-existence of micro-organism in the test tube depends not only on the optical theory of the microscope we use, but also on the theoretical assumption that
if there are micro-organisms in the test tube, they would be observable by means of a microscope. No observational statement accounting for results of an experiment in which a measuring instrument is used can be accepted without recourse to some theory or assumptions. Galileo claimed that by means of the telescope he had constructed, he was able to discover mountains on the moon and spots on the sun. These observations were seriously questioned because they could not be reconciled with Aristotelian cosmology and the optical theory of instruments of the times. Even when we do not use any measuring instrument in the course of an experiment, the result is interpreted, and observational statement is accepted on the grounds of different assumptions and theories concerning the structure of the world and the mechanism of our cognitive function. Ajdukiewicz says, “I do not see any difference between observation and theoretical statements. I believe that mere experimental data do not compel us to accept either or the other.” In order to construct any sentence it is necessary to use some language, and in order to construct any judgment it is necessary to use some conceptual apparatus.

Hanson says that two observers do not see the same thing, do not begin from the same data though their eyesight is normal and they are visually aware of the same object. Observing a protozoon—Amoeba, one saw a one-celled animal, the other a non-celled animal. The same thing is observed as different, seeing is not only the having of a visual experience; it is also the way in which the visual experience is had. The physicist and the layman see the same thing, but they do not make the same statement of it. The infant and the layman can see: they are not blind. But they cannot see what the physicist sees; they are blind to what he sees. All this means that there is no pure observation. If we see some fact we see that as our theory suggests. This is why O'Neil says, “theory provides, at least in part, the premises from which the facts may be inferred as conclusion.”

**Logic of Discovery**

Logic of discovery is the creative process of how to discover. The logic of discovery deals with development and articulation of an idea at every stage in its history prior to its ultimate ratification. It would include an
account of how a theory was first invented, how it was primarily evaluated and tested, how it was modified, and the like. The logic of discovery is historically prior to the context of pursuit. Context of pursuit is a region between the context of discovery and the context of ultimate justification. Something is first discovered; if found worthy of pursuit, it is entertained; if further evaluation shows it to be worthy of belief, it is accepted. These three steps show the three contexts respectively: logic of discovery, logic of pursuit and logic of justification. These three contexts mark the temporal, if not logical, history of a concept. The logic of discovery could be construed as ‘the Eureka moment’, i.e. the time when a new idea or conception is first drawn. The logic of discovery is a set of rules or principles according to which new discoveries can be generated.

In everyday discourse, ‘discovery’ is a ‘success’ word. Because to say that a person P has discovered x carries with it the strong implication that (1) what has been discovered actually exists, and (2) in reporting the discovery P has correctly described x. In everyday contexts, to say that P has discovered that y, implies that y is true. But it has the unacceptable consequences that a theory, T, has been discovered only if T is true. For P has discovered y, where y may be false. To avoid this situation, Martin V. Curd gives a neutral account of the discovery of T in terms of ‘the period of theory generation’. On this account it remains an open question whether T is true or false after it has been discovered or generated. So, discovery of a theory refers not to a specific moment but to an extended period of time. It begins at the moment when a scientist starts thinking seriously about a problem and ends when the theory or what Hanson calls the ‘finished research report’ is accomplished. So logic of discovery means the rules which guide during this period.

However, Bacon, Descartes, Boyle, Locke, Leibniz and Newton—to name only the prominent—at their core of epistemology believe that it is possible to formulate rules which would lead to the discovery of ‘useful’ facts and theories about nature. Unlike now, there was then no distinction drawn between the context of discovery and of justification. The desire to develop a logic of discovery is to increase the rate of advance at which new discoveries are made by articulating fruitful rules for invention and
innovation, and to provide a sound warrant for our claims about the world. If a fool proof logic of discovery could be devised, it would be both the instrument for generating new theories, and automatic guarantee that theories produced by use of it are epistemologically well grounded. Hume, Reid, Priestly, and multitude of other enlightenment philosophers took for granted the existence of an ‘inductive logic of discovery’. But if we trace the development of views about the logic of discovery, our optimism will be replaced by serious pessimism. There is general skepticism today about the viability of a logic of discovery because most of us cannot conceive that there might be rules that would lead us from laboratory data to theories as complex as quantum theory, general relativity, and the structure of DNA. Today’s science virtually involves theoretical entities and processes that are inferentially far removed from the data, which they explain. There might be rules to lead one from tracks on a photographic plate to claims about the line structure of subatomic particles is, to say the least, impossible. Empirical generalizations are viewed as mundane, but the theories that are replete with unobservable entities are as grandiose ontological frameworks. In this level of theorizing inductive logic of discovery have been ignored, or in some case their very existence is denied.

Unlike enumerative induction, there is some non-inductive logic of discovery that is called ‘self-corrective’ logic of discovery. Such logic involves the application of an algorithm to a complex conjunction which consists of a predecessor theory and a relevant observation. The algorithm is designed to produce a new theory that is ‘truer’ than the old. Such logic was thought to be analogous to various self-corrective methods of approximation in mathematics, where an initial hypothesis was successfully modified so as to produce a revised hypothesis that was demonstrably closer to the true value. It does not restrict itself to sentences about observable. It could deal with the genesis of deep structural theories. Unfortunately, more than a century of exploration by a succession of major thinkers has not brought the self-corrective program to fruition. No one was able to suggest plausible rules for modifying earlier theories in the face of new evidence so as to produce clearly superior replacements.
Hershel asserts that the manner of generation of a hypothesis is irrelevant. In the study of nature, we must not be scrupulous as to how we reach to acknowledge general facts, i.e., laws and theories. Whewell goes yet a step further and denies the very existence of a logic of scientific discovery. He says that scientific discovery must ever depend upon some happy thought of which we cannot trace the origin; some fortunate cast of intellect rises above all rules. No maxims can be given which inevitably lead to discovery. According to Popper, the question as to how a new idea occurs to a man—whether it is a musical theme, a dramatic conflict, or a scientific theory—may be of great interest to empirical psychology but is irrelevant to the logical analysis of scientific knowledge. There is no such thing as a logical method of having new ideas. Every discovery contains a creative intuition in Bergson’s sense.

Logic of Justification

We saw that there are no clear rules for discovery. We have now come to the view that there are no logical constraints and rules in discovery. The act of discovering is now free of hard and fast rules provided by the methodology of science. Philosophers who were interested in the logical structure of acquired knowledge, failed to establish any logic for discovery. However, they developed the idea that although discovery does not follow any logic, there is some logic of justification of that discovery. They thought that although there might not be any rules for discovery, yet there must be some rules for justifying a discovery under consideration. Since discovery occurs without any rules, it must be an irrational product; and since scientists are rational men, they cannot accept any theory without rational judgment. Therefore, for a rational acceptance, there must be some logic of justification. Although we do not apply rules when discovering, we can apply rules after the discovery. This idea makes a clear distinction between the context of discovery and the context of justification. The distinction suggests that rules may not be possible in the process of discovery but may be possible of application during the extent of justification of that discovery. So, logic of justification is based on the distinction between the context of discovery and the context of justification.
What is this distinction? How far is context of discovery different from context of justification? How come logic is not possible for discovery but possible for justification? The following considerations would be in order in bringing out this distinction:

(a) There is a temporal difference between the process of discovery and the process of justification. Chronologically speaking, process of discovery precedes the process of justification. This succession is necessary since any justification requires the existence of that which it justifies.

(b) This is a distinction between the factual and the normative. The process of discovery is factual and the process of justification is normative that provides methods of justification.

(c) Analysis of discovery is empirical; the study of a particular discovery may involve historical, psychological and sociological reasoning and research, but not logical methods. Analysis of justification is logical; the considerations, factors, methods etc. of justification are pre-established.

(d) The process of discovery and the methods of justification is the distinction between academic disciplines. Philosophy of science is seen as addressing the logical analysis of justification whereas history, psychology and sociology of science - all these empirical disciplines are seen as analyzing the process of discovery. The empirical disciplines have to learn philosophy of science. But philosophy of science cannot learn anything from the empirical disciplines since what counts as justification is solely a question of logical reasoning; the correctness of a particular justificatory procedure can be determined by logical reflection alone and by nothing else.77

According to Husserl, the laws of logic of justification do not pertain to how one thinks, but how one should think in order to think correctly. It is not concerned with psychological motives or historical circumstances which drive us to accept a statement, but with logical rules which justify it. History of science leaves nothing to contribute to the logic of justification.78
However, it is maintained that discovery may be irrational and need not follow any recognized method; justification, on the other hand, starts only after the discoveries have been made, and it proceeds in an orderly way. The distinction leads to two different questions about scholars in their fields of research: the first; what are the norms of their conduct, and the second; what should they be. The answer to the first question would then be descriptive and to the second, normative.

However, such a distinction can be sustained only by the ongoing process of argumentation and counter-argumentation. For all practical purposes such a distinction is unmaintainable. The following reasons make the unmaintainability of such a distinction categorically clear:

(a) The processes of discovery and justification are initially intertwined, with step of one type alternating with step of the other. Therefore, they are not temporally distinct processes; there is no reason to conclude that the entire process of discovery must be completed before the process of justification can begin. There are even cases in which the process of discovery and the process of justification may be nearly identical.  

(b) As is said, discoveries may be analyzed only in terms of empirical sciences. But discovery has logical aspects also. For, a discovery and a mythical creation are not identical.

(c) The common usage of ‘discovery’ includes some sort of justification. Saying ‘some one has discovered that p’ implies that he or she has acquired some knowledge about p, and this in turn means some sort of justification.

(d) Justification has external factors also. In the actual decision concerning theory-choice, there are factors that play a role, which can only be described, in sociological and psychological terms. Sociological factors come into play since the decision of a scientific community (Kuhn) with respect to theory choice can only be explained with recourse to the system of cognitive values held by this community. Psychological factors come into play since the decision of a single member of this
community can only be explained by the particular way this member interprets communal cognitive values.

(e) Psychology and other empirical disciplines are relevant to epistemology. This is seen in genetic epistemology, evolutionary epistemology and naturalized epistemology. In each of these disciplines it is maintained that empirical knowledge is relevant to the task of epistemology.

(f) Understanding of history of science goes against the distinction under consideration. There are instances where method of justification was also changed to get over certain anomalies. The process of scientific change is dialectical; error and anomaly do not need to be avoided or overcome to be conformed to the prevalent methods of justification. On the contrary, they represent the ‘other’ necessary for the development of a higher synthesis.

Feyerabend gives the historical argument that ‘history generally, and the history of revolutions in particular, is always richer in content, more varied, more many-sided, more lively and subtle than even the best historian and best methodologist can imagine’. ‘History is full of accidents and conjectures and curious juxtapositions of events and it demonstrates to us the complexity of human change and the unpredictable character of ultimate consequences of any given act or decision of men’. Moreover, science knows no ‘bare facts’ at all; the facts that enter our knowledge are already viewed in a certain way and are, therefore, essentially ideational. This being the case, the history of science will be as complex, chaotic, entertaining, and full of mistakes as the ideas it contains, and these ideas in turn will be as complex, chaotic, full of mistakes, and entertaining, as are the minds of those who invented them. In this condition, we cannot believe that the naïve and simple-minded rules which methodologists take as their guide are capable of accounting for such a ‘maze of interactions’. Feyerabend says that the fact that science exists proves that methods of justification are frequently overruled. They were overruled by precisely those procedures, which are now said to belong to the context of discovery. To put it differently: in the history of science, standards of justification often forbid moves that are caused by psychological, socio-
politico-economic and other external conditions and science survives only because these moves are allowed to prevail. He also says that we often make moves that are forbidden by methodological rules. We interpret the evidence so that it fits our fanciful ideas. We eliminate difficulties by ad hoc procedures, we push them aside, or we simply refuse to take them seriously. There is conflict between the contexts. Scientists occasionally choose the moves recommended by the context of justification, but they may also choose the moves that belong to the context of discovery.\textsuperscript{85}

Kuhn says that methodological directives are insufficient to indicate a unique substantive conclusion to many sorts of scientific questions. In order to understand the evolution of scientific knowledge it is necessary to go beyond logic and methodology, to take into account the sociology and psychology of scientific community.\textsuperscript{86} Amsterdamski says that scientists take into account the history of development of knowledge, and do not reconstruct it in isolation from the actual history of science. If their model were purely normative, they would be fully justified in ignoring all the historical data.\textsuperscript{87} Lakatos says that ‘the rational reconstruction never exhausts the history. For, history of science is always richer than its rational reconstruction, and therefore, any rational reconstruction of history needs to be supplemented by an empirical external history.’ When the real history of events does not coincide with the rational reconstruction, external history explains the origins of those inconsistencies.\textsuperscript{88}

All this means that logic of discovery and logic of justification are not distinct from each other; both are intertwined and operate under the historical process. Thus, there cannot be logic of justification as such either.

**Methodological Anarchism**

The above discussion brings out that methodological rules are untenable in both the context of discovery and the context of justification. Our present discussion goes one step further and proposes anarchism in scientific practice; rules are not just merely untenable, but rather we should not obey any unique method for science at all. What is anarchism? Scientists, according to this view, oppose any kind of restriction and they
demand that the individual be permitted to develop freely, unhampered by
laws, duties or obligations. Methodological ‘anarchism’ should be clearly
distinguished from political anarchism. Anarchism in political sense cares
little for human lives and human happiness; it contains precisely the kind of
puritanical dedication and seriousness. But anarchism that is proposed for
scientific practice is quite opposite in its attitude. It is like ‘dadaism’. A
Dadaist would not hurt even a fly—let alone a human being. Like Dadaism,
the anarchism never hurts any scientist who does not want to obey any
established method of science. It is serious neither for nor against any
method; it is in favor of individual scientist’s freedom of thought. The
methodological is far more humanitarian than political anarchism. This
model of anarchism, need not lead us to chaos.

According to this view, an individual scientist gets freedom from a
purported universal method. This view realizes that all methodologies, even
the most obvious ones, have their limits. So, an anarchist plays his role like
an under-cover agent who plays the game of Reason in order to undercut the
authority of Reason. This recommends a new methodology which replaces
induction by counter-induction and uses a multiplicity of theories,
metaphysical views, fairy-tales etc. instead of the customary pair of theory
and fact. Feyerabend argues for this multiplicity, in other words, methodological pluralism. He says that the world, which we want to explore,
is a largely unknown entity. We must, therefore, keep our options open and
we must not restrict ourselves in advance. Epistemological prescriptions may
look splendid when compared with other epistemological principles, or with
genral principles—but who can guarantee that they are the best way to
discover, not just a few isolated facts, but also some deep-lying secrets of
nature?

Historical research shows that there is not a single rule—however
plausible and however firmly grounded in epistemology— that is not violated
at some time or other. It becomes evident that such violations are not
accidental events; they are not results of insufficient knowledge or of
inattention, which might have been avoided. On the contrary, we see that
they are necessary for progress: the Copernican revolution, the rise of
modern atomism, quantum theory, the gradual emergence of the particle theory of light, occurred because some thinkers either decided not to be bound by certain obvious methodological rules, or because they unwittingly broke them. The development of Copernican point of view from Galileo to the 20th century is a perfect example in this regard. We start with a strong belief that runs counter to contemporary reason and contemporary experience. The belief spreads and finds support in other beliefs, which are equally unreasonable. Research now gets deflected in new directions, new kinds of instruments are built, and evidence is related to theories in ways until there arises an ideology that is rich enough to provide independent arguments. We can say today that Galileo was on the right track, for his persistent pursuit of what once seemed to be a silly cosmology. What it amounts to is that scientists are not bound to obey a certain absolute, unchanging set of rules for scientific practice. Such a methodological orientation is summed up by Feyerabend in the dictum 'Anything goes'.

2.3 Conception of Progress in Theory -change

At this point, it would be in order to discuss some important aspects of scientific progress. How is a scientific change considered as progress? In which way can we conceive of it? However, we will try to present an outline regarding this matter here:

**Progress Thesis**

A theory of scientific progress is concerned with a variety of questions. Among them, the following are very much important:

(1) In the course of epistemic change, has there been real progress or merely change? If so, what is the nature of that progress?

(2) Is there a final goal towards which science is progressing? If so, what is this goal? Could there be something like the ultimate answer encapsulating the ultimate scientific theory? Are we approaching this final picture?

(3) Since the notion of progress entails not merely the change but improvement in our knowledge, what normative standard of
comparison is involved there? How can we define the notion of one
theory being epistemically better than another one?94

First of all, it is beyond question that there has not been merely
epistemic change over time. Historically speaking, in our knowledge of the
world there has been progress. There appears to have been progress in the
past, but whether there has been real progress in some absolute sense or not
is a different question. This progress has occurred both on the historical level
(that is, collective growth) and on the individual level. Piaget argues that over
the course of time our knowledge has increased and improved. Firstly, if we
look at the history of science we will see that there is progress just as there is
individual development. Every one would seem to admit that adults know
more than children do, and know it in a more adequate way. Secondly, if
there were no progress, then reason would not evolve rationally. ‘Reason
cannot change without reason’ (la raison n’en peut changer qu’avec raison).
Thus, if epistemic change is to be rational, then there must be progress in the
change. If ‘reason does not change without reason’, then, the only
explanation ‘later is better’ can be the reason of the change.95 In every
discipline, there is a succession of theories in time, and the new are different
from the old. But this does not by itself imply progress. In order for change
to be considered as progress, the change must claim that the new situation is
‘better’ than the previous one. ‘Scientific progress means scientific success:
the success of scientists in getting better and better theories’.96 Again, the
question is: better for what? This question presupposes a goal of the change.
Therefore, ‘progress is a goal-theoretic concept. To say that ‘ X represents
progress’ is an elliptical way of saying ‘X represents progress toward goal
Y’. If our concern is with cognitive (as opposed to material, spiritual, or other
forms of) progress, then Y must be some cognitive aim or aims, such as truth,
greater predictive power, and greater problem solving capacity, greater
coherence, or greater content.97 Given the aim of science is truth we must
see, in order to make a rational sense of science, science as already including
some knowledge of unobservable and being committed to improving this
knowledge of the unobservable. This unobservable is the proper aim of
science. And to acquire and improve the knowledge of unobservable is the
task of science.98
However, to get an adequate understanding of scientific progress we should distinguish systematically between two types of progress, theoretical and empirical. Theoretical progress means progress in theoretical understanding. Aim-oriented empiricism tells us that the world is physically comprehensible. Suppose $T$ is the comprehensive, unified, true theory of everything. Suppose also that $T_n$ can be derived from $T$, $T_{n-1}$ from $T_n$ and ... $T_0$ from $T_1$. Suppose further that none of these derivations can be reversed. In this case, $T_0$, $T_1$...$T_n$ constitutes theoretical progress. To...$T_n$ constitutes empirical progress if we can define approximate versions of these theories such that $T \rightarrow T_n \rightarrow T_{n-1} \rightarrow ... \rightarrow T_0$ and thereby the problem is solved. Empirical progress is no less of intellectual achievement than the theoretical progress. However, empirically successful theories may even be false in strict sense. For, science is not concerned just with the discovery of descriptive laws in order to develop a utilitarian ability of predicting and controlling nature. Instead, science produces laws under the pressure of the mind’s natural tendency to explain nature causally. Problem is solved with the satisfaction of mind. However, this satisfaction is not for ever. In course of time, people may again be dissatisfied with previous solution. Regardless of true solution of problem, solution has an important role for theory choice. For there is good reason to claim that a good error, or a new irrational is cognitively more relevant than many solutions. Without a criterion of demarcation for anomaly and error, progress cannot be thought of. Conversely without a notion of progress, anomaly and error become either a matter of subjective judgment or a philosophically unproblematic issue.

Cohen says that Progress is a value term. It denotes some forms of improvement between at least two parameters. Thus, it is a two-place relation. The progress of knowledge that we believe the sciences achieve over time involves the relation between two intersubjectively shared belief-descriptions. The basic progress relation can be conceived of as being of the following kind:

(1) Progress $= \{< T_1, T_2 >, ... \}$

Depicting progress in this manner amount to saying that progress is a two-place relation linking two theories. It is an asymmetric and transitive relation.
With regard to improvement, there is the yardstick for comparing theories. This yardstick is the value that should be constant to the formulation of the relation. For a realist, this value is truth or truthlikeness. If one holds other views about science; other aims, like problem-solving or empirical adequacy, may serve equally well under a purely formal point of view. There may not be the obvious indications for ascertaining whether or not such progress is actually obtained between two theories. Any such estimation will be subject to human fallibility. We may believe that a certain theory-change was progressive, but may turn out that it was not. Therefore, the question of ideal semantic version of progress may arise as to what true progress is.

Cohen says if we try to capture those ascriptions of progress that deal with our estimations of progress, the progress relation changes from a two-place to a three-place relation. It now looks like this:

\[(2) \text{Progress} = \{< T_1, T_2, E>, \ldots \}\]

Here \(E\) denotes the evidence that is cited as a ground for believing that \(T_1\) is better than \(T_2\). If the available evidence changes, then our estimations of progressiveness may change with it. The equation (1) represents an ideal semantic notion of progress, and (2) does an epistemological version of progress. The progress is one thing, and our estimation of progress is quite another. The truth is one thing, indication of truth is quite another. What kind of estimations of progress and what kind of indications of truth, should be regarded sufficient for matters of dispute over theory preference is still unsolved.

However, Cohen also analyses the equation for empirical progress. Here we replace the basic entities that are to be compared in terms of theoretical improvement. Instead of dealing with theories (T), we now deal with two phenomena or sets of phenomena (P).

\[(3) \text{Progress} = \{< P_1, P_2, E >, \ldots \}\]

The rationale for this move lies in the fact that the conditions under which we would like to replace one set of phenomena with another are quite different from those that apply in the case of high-level theories. Phenomena are judged on the basis of data and the inference tickets used in their formulation.
A scientific phenomenon is a conceptual abstraction sprung out of the available data; it is far removed from sensible experiences. Theories are judged on the basis of phenomena and at the same time phenomena constitute an impressive intellectual achievement of science. Therefore, it is natural to conclude that progress in phenomenal change should complete our conception of scientific progress.\textsuperscript{104}

Empirical progress occurs in three ways: (1) $P_1$ contains the discovery of new phenomena in relation to $P_2$. There are those cases where we somehow manage to establish new phenomena, i.e. phenomena that did not earlier figure within the empirical basis of our theories. The discovery of new type of galaxies or stars may serve as an intuitive example from astronomy. (2) $P_1$ corrects known phenomena in relation to $P_2$. There are those cases where we manage to 'fine-tune' some known phenomena, i.e. specify them in new and better ways. This includes the case of merging or dividing known phenomena. (3) $P_1$ represents new methods or instruments in relation to $P_2$. There are cases where new methods or instruments provide us access to hitherto inaccessible areas of reality. The introduction of radio telescopes or electron microscopes may serve as prominent example here. By means of these instruments, new types of phenomena become empirically detectable which hitherto were beyond the reach of scientific investigation.

Cohen also says that empirical progress is perhaps relativised to discipline. For, some methodological standards are designed for the particular purposes of a given discipline, which may be at variance with standards from other disciplines. The degree to which they are in conformity with those very general standards is a matter of empirical investigation. Then the disciplinary methodological standards are considered as epistemologically prior. Therefore, the scientific rationality and scientific progress become intertwined, as two sides of the same coin. This view gives us the room to deviate from an insider judgment. We may find that the methodological standards accepted in the discipline do not validate the change. Or we may find that the standards do indeed validate the particular change, but the scientists were unaware of it and accepted the change for irrelevant reasons.\textsuperscript{105}
Regarding empirical progress, what precisely could it mean to say that we have discovered a new phenomenon? There may be three definitions of discovering new phenomenon. First, the new phenomenon, P, may emerge as the abstraction of a new set of data, utilizing legitimate inference tickets. Second, a new phenomenon, P, may be established by higher abstraction from given phenomena. Third, a new phenomenon, P, may be the result of merging several lines of abstractions from data to phenomenon, where each individual line employs some inference tickets that yield uncertainty with regard to P. Now come to the second kind of empirical progress and ask what could it mean to say that $P_1$ corrects some phenomena in relation to $P_2$. There are also three definitions of correcting the phenomenon. First, there are cases where a discipline realizes that certain inference tickets are much more problematic than assumed earlier. Second, there are cases where known phenomenon have been described differently by utilizing new inference tickets or new set of data or both. Third, this case of correcting may occur through the combination of these processes. One may replace a known phenomenon by utilizing a new classification that is better supported by the known data and inference tickets. However there are different views about whether scientific change results in progress or not. Following are among them:

**Cumulativism**

There is a belief (positivistic) that theory change is cumulative or content increasing. The main theme of accumulation is that success of old theories is always captured by their successors, which are at the same time expected to be able to account for things unexpected by their predecessors. Therefore, science progresses quite simply, because later theories can always do everything their predecessors could and more. There is another view (post-positivistic), which denies that theory change is cumulative. This view arises from the fact that in the history of science there are examples after example of theories that failed to solve all the problems solved by their predecessors. Both views accept wholly uncritically that wholesale retention of explanatory content is a precondition of cognitive progress. Therefore,
According to the first view there is progress in scientific change, for scientific change is cumulative; on the other hand, the second view holds that science does not progress, for scientific change is not cumulative. According to simple accumulation, the development of science is a simple accretion of knowledge—accumulation of truth. Scientists admit that each law or theory remains without change. In the growth of science, there is only the enrichment without loss. Even when a new, more general theory is formulated, the previous one preserves its truth-value. There is no revolution in the history of science; or there is only one revolution—after that science is progressing with continuity. This is an extreme view of cumulativism. According to this view, the new truths remain together with the old ones and complement them. The result is that the global amount of human knowledge is increased, in the sense that more and more aspects of reality become known as a result of this proliferation of viewpoints, leading to a proliferation of domains of investigation.

Revolutionism

According to this view scientific change occurs without continuity. Every new theory is radically different than the previous one. The new theory sees the problems and their importance together with their solutions from a different vantage point. What is problem for the old may not be the same for the new; what is solved according to the old may not be solved at all. That is, their standards, methods of inquiry, rules for justification, ways of observations, techniques and evaluations of experiments—all are so different that two theories constitute two different worlds. Their concepts and terms are incommensurable to each other. In this condition, if there occurs a change, the new theory produces such a different result that cannot make any addition to the old. This kind of change is mere change without having any growth. This view of scientific change is called revolutionism. As in social revolution, in science a new theory falsifies the old one. In normal science continuity reigns but in periods of revolution the continuity is broken. Revolutionary change just means that two theories see the same thing from two different perspectives; so there cannot be any progress in this change.
Dialecticism

Neither cumulativism nor revolutionism can conform the actual history of science. Both are extremities, which do not hold water. So with a view to arrive at a better understanding of time-bound activities of science they must be replaced by a synthesis that unites the oppositions and at the same time goes beyond them. This may be called dialectical view of scientific change. This view realizes the role of cumulativity postulate (CP) and at the same time notices contradictions between the corresponded and the corresponding theories. Every new theory is more general or higher theory than the old ones. Contradiction of two lower level theories is explained away in this higher one; therefore, leading to an enrichment of knowledge. However, this higher theory becomes incommensurable to another higher theory, entailing another synthesis in the process. The contradictory theories together do not make any progress, but the higher one in relation to two lower theories does make progress. For example, the competition of Cuvier’s catastrophism and Lyell’s uniformitarianism in paleontology did not last long and Darwin’s work led to a synthesis, which was commonly acknowledged. In this case, a competition of two one-sided theories led to a new theory, which is synthesis of the competing theories and at the same time, their negation. The whole process may be called a dialectical ascent. This view recognizes both the period of stability and the period of revolution.

Infinity of Progress

Progress is infinite. We cannot say, even in principle, that after such and such steps we will reach the goal. Scientific progress will never stop. There are three reasons for this infinity: (1) science uses better and better measuring instruments, and discovers more and more precise laws. The improvement of measuring instruments and methods has no limit. (2) The world is infinitely complicated, even at the surface. Everything has (probably) infinitely many properties; we discover them in turn. Every real process is infinitely complex because infinitely many side factors intervene; we take them into account one by one. (3) The world is (probably) infinitely complex in depth. Structure of matter consists of different levels; we reach them gradually. It is probable that the number of these levels is infinite.
Therefore, we should go forever discovering new fundamental elements of matters, and its new basic laws. The number of scientific revolutions would be infinite. Lenin says the infinity of scientific progress is associated with infinity of complexity of essence. Human thought goes deeper from appearance to essence, from essence of the first order to the essence of the second order etc. without end.¹¹⁰

According to Krajiwski scientific revolutions usually consist of revealing new idealizing assumptions of the old theories, and creating new factual theories. We approach absolute truth along a correspondence sequence $T_1, T_2, T_3$...In such a sequence truth content gradually increases:

$$\text{Trc} (T_1) < \text{Trc} (T_2) < \text{Trc} (T_3) < ...$$

At the same time we have new formulations of the initial theory, due to gradual revelation of its appropriate idealizing assumptions: $T_1, T'_1, T''_1, ...$. This sequence also approaches absolute truth although in a different sense. We are dealing here with a model truth. When we introduce a concept of model truth-content, $m\text{Trc}$, analogous to the concept of classical truth-content, we may write:

$$m\text{Trc} (T_1) < m\text{Trc} (T_2) < m\text{Trc} (T_3) < ...$$

In other words, we approach finally absolute truth on two planes: a factual and an idealizational. On the first plane, we have increasingly more exact factual laws. Here we approach absolute classical truth in an infinite process. In the second place, we have more adequate formulations of idealizational laws; we approach absolute model truth in an infinite process. Both processes are closely connected and neither of them is possible without the other. The concept of classical truth and concept of model truth are necessary in order to obtain a real picture of scientific progress.¹¹¹

**Logic and History**

Scientific progress can be considered in two different ways. One is logic (internal history) that provides a *model of progress*, and the other is *actual progress* achieved (external history, with sociological, ideological, psychological factors etc.) that is accounted for in the history of science. For Krajiwski, model of the progress of science ought to be compatible in basic
outline with the real history of science. When it is not, it must be criticized. However, real history is never completely compatible with a model---numerous deviations are inevitable. In this case, we should criticize rather the history. This is not paradox as practice is never fully reasonable. It always deviates from rational scheme. Both have a bearing on each other. This is why, we can see an incompatibility of scientific rational with the actual historical. Here scientific rationale and history both could be criticized by each other. For, nothing is sacrosanct in science and history of science.\textsuperscript{112}

\textit{Scientists' Concept of Progress}

Scientific progress in the eyes of scientists is different from that in the eyes of philosophers. Unlike philosophers, scientists maintain a ‘\textit{logical relation}’ between theories in the historical development of physics. Scientists like Misner, Thorne, and Wheeler hold that physics develops and expands with its unity by a network of ‘\textit{correspondence principle}’. Correspondence principle follows that the newer more sophisticated theory is better than its predecessor, because it gives a good description of a more extended domain of physics, in the way that the old theory gives the newer the power to recover the old. Rohrlich, one of the famous scientists, says that the old theories, having been proved correct over a long time, did not really become wrong. For him, they only become restricted to a limited domain of validity. Newtonian mechanics becomes restricted to phenomena in which the velocities are small compared with the velocity of light. He also says about another aspect of change that while the predictions of a theory will always remain correct when used in the domain of validity, the foundations of the theory may be radically modified by a more general theory. The notion of absolute space and absolute time are abandoned in the special theory of relativity. Here the lower level theory is derived from the higher, through which there build up a hierarchy of theories.\textsuperscript{113}

Another scientist is Boltzmann. He says about scientific change that Physics is an essentially changing - perhaps ever-changing - enterprise. At no moment, it is quite right but seldom it is quite wrong. Physical theories that are well confirmed and accepted for a long time would in restricted sense be of eternal value. For all the discontinuity, there is some continuity even in the
sense of bridging over those discontinuities. He argues that formerly one used to say that the old view has been recognized as false. This sounds as if the new ideas were true. Thereby it is clearly expressed (1) that the earlier theory too had been useful because it gave an, if only partially, true picture of the world, and (2) that the possibility is not excluded that the new theory in turn will be superseded by a more suitable one.114

For Walther Nerst, in contrast to inductive generalization there is another way where thoroughgoing ideas on the nature of certain phenomena are developed by purely speculative activity. Correctness of this kind of knowledge has to be tested by experiment only subsequently. Their success though does not provide correctness (does prove their usefulness), their failure does mean not only uselessness, but also their falsity. If any theory becomes successful, it has retained its validity over a wide range, but the limits of its applicability have been more sharply defined. There is scarcely one law established by an investigator of the highest rank that has not preserved for all time within its range of applicability. It cannot be said that the electromagnetic theory of light has completely overthrown the old optical theory put forward by Fresnel and others. On the contrary, now as formerly, an enormous range of phenomena can be adequately dealt with by the older theory. The new theory implies a great advance, but by no means nullifies the success of the older theory. He, therefore, concludes that scientific theories cannot be withered leaves in the course of time. They appear to be endowed under certain restrictions with eternal life. Every famous theoretical discovery of the day will doubtless undergo certain restrictions on future development and remain for all time the essence of a certain sum of truths.115

Disproof View of Progress: Einstein is one of the scientists who maintains the disproof view of scientific progress. According to disproof view, in the course of scientific change the older theory gets disproved and thereby rejected by the proponent of the new theory. Regarding quantum theory and classical mechanics Einstein says that quantum mechanics of Planck overthrew classical mechanics, in the case where sufficiently small masses move with sufficiently small velocities and sufficiently high acceleration. Kienle the astronomer shows himself to be a typical advocate of
the disproof view. For him, progress occurs by disproving an empirically well-supported theory. Progress is launched by the clash of theory and experience. Hermann Bondi, a physicist cosmologist, describes the fate of Newton's theory of gravitation by comparing the enormous number of tests each of which the theory had passed brilliantly. But it becomes disproved by a slight discrepancy in the motion of planet mercury. Newton's theory of gravitation was good and solid, tested well over a hundred thousand times, but when such a theory falls victim to the increasing precision and calculation, one certainly feels that he can never rest assured. So also, in Bondi's view theory-change occurs in disproving the earlier theory. Therefore, we cannot speak of progress as progress in which knowledge becomes more and more certain and more and more all-embracing. The true importance of this disproof view is that it leads to a jump in understanding.116

**Conceptual Change View:** This view is developed by the physicists, above all by Bohr and Heisenberg. Heisenberg says that by the unexpected phenomena in electromagnetics and atomic physics, there occurs a strange development. This development resulted in a change of meaning in many of the most fundamental concepts of physics. Then it is realized that the new phenomena can be understood, but they cannot be understood in the same sense as the phenomena of earlier physics. The word 'understanding' itself has changed its meaning. He holds that the aim of physics is to establish close theories. A close theory is a theory whose basic concepts have already uniquely determined the basic laws of the theory. This type of theory cannot be improved with small change. Great progress consists in the transition from one closed theory to another that becomes its successors. If a theory is closed then its improvement occurs only when its basic concepts become inapplicable. Its improvement then involves a conceptual change. According to Heisenberg there are four closed theories of physics: Newtonian mechanics, statistical thermodynamics, classical electro-dynamics and quantum mechanics. It was a conjecture of Heisenberg's that a fifth closed theory will come up in connection with a final theory of elementary particles. Of course, classical electro-dynamics and quantum mechanics are successors of Newtonian mechanics. The still missing fifth closed theory will become a
successor of each of these three theories. Upto now we know of only one closed theory which has been superseded.\textsuperscript{117}

\textit{Correspondence Principle:} Conceptual change view of change follows that in the relation between two theories, there is the relation in time, and there is all the historical stuff that such a case involves. Bohr and Heisenberg were convinced that above this there must be some logical relation between the theories expressing definite correspondence of their respective contents. Heisenberg says that new phenomena in science could not always be explained by using the known laws of nature. In some cases, new phenomena could only be understood by new concepts. These new concepts again could be connected in a close system and represented by mathematical symbols. Much richer and more complete conceptual connections have been assumed to hold between classical and quantum mechanics since the days of Bohr’s theory of atom. The assumption is: for high quantum numbers the orbital frequencies of an electron in the atom would approximate the radiation frequencies. This assumption postulated by Bohr is called ‘correspondence principle’.\textsuperscript{118}
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Chapter – 3

The Aim of Progress and the Falsifiable Hypothesis

3.1 Nature of Falsifiable Hypothesis

*Origin of Hypothesis*

For Karl Popper, knowledge produced from any source can be scientific, if it is refutable and, in principle, amenable to appropriate or established procedures of testing. In other words, any theory can be considered a scientific theory if its claim can be subjected to critical evaluation. This theory may emerge from any source; myth, metaphysics, fiction etc. This view is anchored on the two-pronged realization, viz; (a) it is not clear and distinct that ‘truth is manifest’ and (b) induction cannot be the characteristic method of science.

The theory of ‘Manifest Truth’ is born out of the epistemological optimism inspired by Western Renaissance: a faith in man’s power of discernment and understanding leading to certainty of knowledge. At the heart of new optimistic view of the possibility of knowledge lies on the doctrine that ‘truth is manifest’. The view of ‘Manifest Truth’ is espoused by Descartes.

He says that truth may perhaps be veiled, but it may reveal itself. And if it does not reveal itself, it may be revealed by us. Removing the veil may not be easy, but once the naked truth stands revealed before our eyes, we have the power to see it, to distinguish it from falsehood, and to know that it is true. Truth is always recognizable as truth. Thus, truth has only to be unveiled or discovered. Once this is done, there is no need for further argument. We have been given eyes to see the truth, and the ‘natural light’ of reason to see it by. He mentions Bacon’s and Descartes’ view that ‘there was no need for any man to appeal to authority in matters of truth, for, each man carried the sources of knowledge in himself—either in his power of sense perception which he may use for the careful observation of nature, or in his power of intellectual intuition which he may use to distinguish truth from falsehood by refusing to accept any idea which is not clearly and distinctly perceived by intellect.’

According to Karl Popper, if truth is manifest, it need not be explained, rather it creates the need to explain falsehood. How can we ever fall
into error if truth is manifest? The Cartesian answer would be: through our own sinful refusal to see the manifest truth. Or, our minds harbour prejudices inculcated by education and tradition. Or, other evil influences which have perverted our originally pure and innocent minds. Ignorance may be the work of powers conspiring to keep us in ignorance, to poison our minds by filling them with falsehood, and to blind our eyes so that they can not see the manifest truth. This is called conspiracy theory of ignorance. The conspiracy theory of ignorance is well known in its Marxian form as the conspiracy of a capitalist press that perverts and suppresses truth and fills the worker's minds with false ideologies. However, such a conspiracy does not permit us to know the truth, otherwise truth is manifest.\footnote{2}

However, Popper criticizes the view of manifest truth and says that 'manifest truth' is not manifest at all. In support of his contention, he forwards three reasons. Firstly, he argues that 'the simple truth is that truth is often hard to come by, and that once found it may easily be lost again. Erroneous beliefs may have an astonishing power to survive, for thousand years, in defiance of experience, and without the aid of any conspiracy. Thus the optimistic epistemology of Bacon and of Descartes cannot be true'. Secondly, epistemology of manifest truth has also led to disastrous consequences. The theory that 'truth is manifest' leads to every kind of fanaticism. For, it holds that only the most depraved wickedness can refuse to see the manifest truth. Only those who have every reason to fear truth can deny it, and conspire to suppress it. Thirdly, theory of manifest truth not only breeds fanatics but it may also lead to authoritarianism. This is so simply because truth is not manifest, as a rule. The allegedly manifest truth is therefore in constant need, not only of interpretation and affirmation, but also of reinterpretation and reaffirmation. An authority is then required to pronounce upon, and lay down, almost from day to day, what is to be the manifest truth. And it may learn to do so arbitrarily and cynically.\footnote{3} 'Know thyself how little thou knowest' was swept away by 'truth is manifest' thesis. Therefore, manifest truth could only replace one authority—that of Aristotle and the Bible—by another. The new authority is the authority of sense and the authority of the intellect.\footnote{4} However, he denies manifest truth and calls it false epistemology.
According to Popper, true epistemology must realize that all of us may and often do err, singly and collectively. However, this very idea of error and human fallibility involves another one—the idea of objective truth, the standard that we may fall short of. Thus, the doctrine of fallibility should not be regarded as a part of a pessimistic epistemology. This doctrine implies that we may seek for truth, for objective truth, though more often than not, we may miss it by a wide margin. And it implies that if we respect truth, we must search for it by persistently searching for our errors: by indefatigable rational criticism and self criticism. Therefore, we acquire knowledge from criticism, in other words, attempted refutation of the existing one. For Popper, knowledge is produced from critical examination of what is asserted, not by the tracing of its origins. Thus, the question, ‘what is the source of knowledge, is wrongly put. Similarly, the question, ‘what are the best sources of our knowledge’? is also wrongly put. For, no such pure, untainted, certain, ideal source exists. There are all kinds of sources of our knowledge, but none has the overarching authority. We have to give up the idea of ultimate source of our knowledge and admit that all knowledge is human; and that it is mixed with our errors, our prejudices, our dreams, and our hopes. All we can do is to grope for truth even though it be beyond our reach. Therefore, Popper proposes to replace the question of the sources of our knowledge by the entirely different question: how can we hope to detect and eliminate error. The proper answer to this question is by criticizing the theories and guesses of others, and also by criticizing the theories and guesses of our own. Popper says, “I do not know what is source of our knowledge, what I want you to criticize my tentative assertion as severely as you can; and to design some experimental test which might refute my assertion. If you do, I shall gladly, and to the best of my powers, help you to refute it”. So, knowledge is an adventure of ideas. These ideas are produced by us, and not by the world around us.

Therefore, any production of human mind, such as myth, metaphysics, story, guess, whatsoever, may be the source of our knowledge; this is the first step of our knowledge adventure. The second step is to try to eliminate error from that product by criticism, and that criticism may be as severe as possible at any given time; for, we will try our best to eliminate the falsehood from our
product. Regarding the source of our knowledge there is a further question as to what is the method to produce a theoretical assertion? For Popper, it is *conjecture* rather than *induction* by which we obtain the initial assertions about the world.

What is the induction? Any inference, which passes from singular statements, such as accounts of the results of observations or experiments, to universal statements, such as hypotheses or theories, is called induction. According to a widely accepted view, the logic of scientific discovery would be identical with inductive logic. But it is easy to see that the method of science is ‘conjecture and refutation’ rather than induction. Popper says, it is far from obvious, from a logical point of view, that we are justified in inferring universal statements from singular ones, no matter how numerous. For, any conclusion drawn in this way may always turn out to be false: no matter how many instances of, say, white swans we may have observed, this does not justify the conclusion that ‘all swans are white’.

Inductive method is strongly supported as a characteristic method of science by Reichenbach. He calls it the ‘principle of induction’. For him, to eliminate it from scientific research would mean nothing less than to deprive science of its basic method of investigation. This is the principle by which scientists decide the truth or falsity of their theories. Without it science would no longer have the right to distinguish its theories from the fanciful and arbitrary creations of a poet’s mind. For Popper, inductive logic cannot be deemed to be a principle. In case we accept induction as a principle, it would amount to accepting that such a principle would be the statement with the help of which we can put inductive inferences into a logically acceptable form. But the principle of induction cannot be a purely logical truth like a tautology or an analytic statement. If there were such a thing as a purely logical principle of induction, all inductive inferences would have to be regarded as purely logical or tautological transformations. But principle of induction is a synthetic statement, it has no such analytic property that is, a statement whose negation is self-contradictory, or logically not possible.

The so-called principle of induction is thus superfluous. Furthermore, such a principle can lead to inconsistencies as well. For, the principle of
induction must be a universal statement in its turn. Now, if we try to regard its truth as known from experience, then the very problems which occasioned its introduction will arise all over again. To justify it, we should have to employ inductive inferences; and to justify these we should have to assume an inductive principle of a higher order; and so on. Thus, to attempt to base the principle of induction on experience breaks down, since it must lead to an infinite regress. Hume says that truth of induction is a sheer belief based on human psychology. In fact, there are no necessary connections between inductive sequences of observations. As Born points out that no observation or experiment, however extended, can give more than a finite number of repetitions. Therefore, statement of law—B depends on A—always transcends experience. Yet this kind of statement is made everywhere and all the time, and sometimes from scanty material.

Popper further claims that observations too are not a source of scientific knowledge. He forwards three reasons to show that scientific theories generally claiming to be derived from observations, are actually not derivable from them:

Firstly, it is intuitively not credible,
Secondly, it is historically false,
Thirdly, it is logically impossible.

Firstly, he says that if we examine the character of a theories, and of relevant observational statements, we shall not be convinced of any kind of necessary relationship between them. Take, for example, Newtonian theory of gravitation. There are no observational statements which directly or indirectly validate or support his theory. In the first place, observations are always inexact, while theory makes the exact assertions. Not only this, it is a great triumph for Newtonian theory that it stood up to subsequent observations regarding precision far beyond what could be attained in Newton’s own time. Now it is incredible that more precise statements, let alone the absolutely precise statements of the theory itself, could be logically derived from less exact or inexact ones i.e. observations. Moreover, on the one hand, an observation is always made under very special conditions, and each observed situation is always a highly specific situation. While the theory claims to apply
in all possible circumstances, on the other. For example, a theory about planetary system applies not only to Mars or Jupiter, but to all planetary motions and all solar systems. Furthermore, observations are always concrete, while theory is abstract. For example, we can never observe anything like Newtonian force. Without the dynamical theory it is simply impossible to measure force. For all these reasons, it is intuitively not credible that the theory should be derivable from observations.

Secondly, it is historically false to believe that Newton’s dynamics was derived from observations. Popper says that this belief is widespread but that is merely a myth having no historical truth. To show this he refers to the part played by the most important precursor, Nicolas Copernicus. He says that Copernicus’ idea of placing the Sun rather than the Earth in the center of the universe was not the result of new observations but of a new interpretation of old and well-known facts in the light of semi-religious Platonic and Neo-Platonic ideas. The crucial idea can be traced back to the Plato’s Republic. There we read that the Sun plays the same role in the realm of visible things, as does the Idea of the Good in the realm of Ideas. The Idea of the Good is the highest in hierarchy of Platonic Ideas. Accordingly, the Sun, which endows things with their visibility, vitality, growth, and progress, is the highest in the hierarchy of the visible things in nature. This Platonic Idea, then, forms the historical background of the Copernican revolution. Popper claims that it does not start with observations, but with religious or mythical ideas. So looked at from a historical or genetical point of view observations were not the source of heliocentric thesis. Historically speaking, idea of heliocentricity preceded the observations thereof. The idea become indispensable for interpretation of the observations; they had to be interpreted in its light.

Thirdly, it is logically impossible to derive, for example, Newton’s theory from observations. This conviction follows intimately from Hume’s critique of the validity of inductive inference. Hume argues that if a theory follows from observations, no logically possible future observations can contradict the class of past observations. In other words, if B is a self-consistent observation- statement about a possible future event, and K is any class of pure observational statements about past events, then B can always be conjoined
with K without contradiction. Now if we add to this a theorem of pure logic, namely: ‘whenever a statement B can be conjoined without contradiction to a class of statements K, then it can also be conjoined without contradiction to any class of statements that consists of statements of K together with any statement that can be derived from K.’ Thus we can prove our point: if Newton’s theory could be derived from a class of K of true observation-statements, then no future observation B could possibly contradict Newton’s theory and the observations of K. But we can logically derive from Newton’s theory and past observations a statement that tells us whether there will be an eclipse of the sun tomorrow. Now if this derived statement tells us that tomorrow there will be no eclipse of the sun, then our B is clearly incompatible with Newton’s theory and the class K. It follows logically that it is impossible to assume that Newton’s theory can be derived from observations.

J.M. Keynes in his *A Treatise on Probability* says that inductive inference, although not strictly valid, can attain some degree of reliability or of probability—inductive inferences are probable inferences. It is not given to science to reach either truth or falsity but scientific statements can only attain continuous degree of probability whose unattainable upper and lower limits are truth and falsity respectively. However, the idea of probability is again unsuitable for principle of induction. For, Popper says that, if a certain degree of probability is to be assigned to statements based on inductive inference, then this will have to be justified by involving a new principle of induction. And this new principle in its turn will have to be justified, and so on. Nothing is gained if the principle of induction is taken not as true, but only as probable. In short, like every other form of inductive logic, the logic of probable inference or probability logic leads either to an infinite regress, or to the doctrine of apriorism. According to Popper, there is another problem with probability. *The probability of statement is always the greater the less the statement says.* It is inverse to the content or the deductive power of the statement, and thus to its explanatory power. Accordingly, every interesting and powerful statement must have a low probability, and vice versa. A statement with high probability will be scientifically uninteresting, for, it says little and has no explanatory power. Although we seek theories with a high degree of corroboration, as
scientists we do not seek highly probable theories but explanations; that is to say, powerful and improbable theories.\footnote{15}

Thereafter, Popper examines the positivistic dogma of meaning in inductive logic. He mentions that meaning in inductive logic is equivalent to the requirement that all the statements in empirical science must be capable of being finally decided, with respect to their truth and falsity. This means that their form must be such that to verify them and to falsify them must both be logically possible. In this connection he indicates Schlick’s view: a genuine statement must be capable of conclusive verification; and Waismann’s view: ‘if there is no possible way to determine whether a statement is true then that statement has no meaning whatsoever. For, the meaning of the statement is the method of its verification’. Popper says, ‘but there is no’, ‘such thing as induction. Thus, inference to theories, from singular statements which are ‘verified by experience’ is logically inadmissible. Theories are therefore never empirically verifiable’. In view of the same, he denies induction as sole method of science. Then what is the characteristic method of science, is a question which needs elaborate consideration.

In this regard, Popper holds that a system can be empirical or scientific only if it is capable of being tested by experience. This suggests that falsifiability rather than verifiability is the criterion of meaning of scientific discourse. In other words, it is not required that scientific system will be capable of being singled out once and for all in a positive sense; but it is required that its logical form will be such that it can be singled out, by means of empirical test, in a negative sense. It must be possible for an empirical scientific system to be refuted by experience. Thus, the statement ‘it will rain or not rain here tomorrow’ will not be regarded as empirical, simply because it cannot be refuted, whereas the statement ‘it will rain here tomorrow’ will be regarded as empirical.\footnote{16}

It might be said that falsification is impossible, for any theoretical system should never be conclusively falsified. For, it is always possible to find some way of evading falsification. It is even possible without logical inconsistency to adopt the position of simply refusing to acknowledge any falsifying experience whatsoever. Popper says, on the contrary, that what
characterizes the empirical method is its manner of exposing theories to falsification. Its aim is not to save the lives of untenable systems but, on the contrary, to select that which by comparison is the fittest, by exposing them all to the fiercest struggle for survival.\textsuperscript{17} He says that there is a distinction between falsifiability and falsification. Falsifiability is the sole criterion of the empirical character of a system of statements. As to falsification, special rules must be introduced which will determine under what conditions a system is to be regarded as falsified. We say that a theory is falsified only if we have accepted a basic statement which contradicts it. This condition is necessary, but not sufficient. For, we have seen that non-reproducible single occurrences are of no significance to science. Thus a few strong basic statements contradicting a theory will hardly induce us to reject it as falsified. We shall take it as falsified only if we discover a reproducible effect, which refutes the theory. In other words, we only accept the falsification if a low level empirical hypothesis which describes such an effect is proposed and corroborated. This kind of hypothesis may be called a falsifying hypothesis. That the falsifying hypothesis must be empirical and so falsifiable, only means that it must stand in a certain logical relationship to possible basic statements. The hypothesis should be corroborated’ refers to tests which it ought to have passed—tests which confront it with accepted basic statements. Thus, the basic statements play two different roles. On the one hand, we have used the system of all logically possible basic statements in order to obtain the logical characterization of the form of empirical statements. On the other hand, the accepted basic statements are basis for the corroboration of hypothesis. If accepted basic statements contradict a theory, then we take them as providing sufficient grounds for its falsification only if they corroborate a falsifying hypothesis at the same time.\textsuperscript{18} Therefore, according to Popper scientific theories are in essence falsifiable hypotheses.

Popper says that a hypothesis may be metaphysical, mythical or the assertion of any sort, but it is scientific if it is refutable by the empirical. For him, in certain sense, science is myth making just as religion is. Nevertheless, it has been different from religion because the former is empirically open to criticism whereas the second is not. There are two possible attitudes towards
any tradition or the assertion. One is to accept the assertion uncritically, often even being aware of it. This is the first order attitude. The other possibility is a critical attitude, which may result either in acceptance or in rejection, or perhaps in a compromise. This is the second order attitude. This second order attitude is the critical or argumentative attitude. Any assertion, which adopts this second order critical attitude, does become different.

Therefore, the man to whom a myth was handed on would take the myth and would apply it to various things which it was supposed to explain, such as the movement of planets. Then he would say: I do not think that this myth is very good, for, it does no explain the actual observable movement of the planets; or whatever it might be. Thus, it is the myth or the theory which leads to, and guides, our systematic observations - observations undertaken with the intention of probing into the truth of the theory or the myth.

From this point of view, Popper claims, the growth of the theories of science should not be considered as the result of the collection or accumulation of observations. On the contrary, the observations and their accumulation should be considered as the result of the growth of the scientific theories. This is what Popper calls the 'searchlight theory of science — the view that science itself throws new light on things; that it not only solves problems but it creates many more; and that it not only profits from observations but leads to the new ones. In this way, we look out for new observations with the intention of probing into the truth of our myths; and the myths then become changed from their rough manner to the scientific theory; and in time they become more realistic and agree better with observable facts. In other words, under the pressure of criticism the myths are faced to adopt themselves to the task of giving us an adequate and a more detailed picture of the world in which we live. Scientific theories are not just the result of observations, they are, broadly speaking, the product of myth-making.

Krajewski, calls this view hypotheticism because according to it the investigation starts from a hypothesis. When scientists want to explain a set of phenomena, they create a hypothesis, which is a candidate for becoming a law. Next, they deduce different empirically testable consequences from it and test them in experiments. If the test gives positive result, the candidate is admitted
as a law, though this decision is never final. If the test gives negative result, the candidate is rejected. In this competition, the defeated and the defeater both are hypotheses.\(^{21}\)

They are myth-making, for, they are produced from the imaginative mind of the scientists—they are not the abstract from the observations. First myth is produced then we observe according to that myth. Without theory, in other words, myth, we could not orientate ourselves in the world; we could not live, because our observations are always interpreted; we observe the thing as our theory suggests. For instance, the Marxists literally observe class struggle everywhere, for, their theory suggests that history of mankind is the history of struggle between classes; The Freudian observe everywhere repression and sublimation; the Adlerian sees how feelings of inferiority expresses themselves in every action and every utterance; all observations are designed by their theory.\(^{22}\)

Therefore, theory is not produced by abstraction from observations: Observation, itself, cannot operate without theory. For Popper, nothing can be built on pure data, because there is nothing as pure data; there is nothing simply ‘given’ to us uninterpreted. All our knowledge is interpretation in the light of our expectations, our theories.\(^{23}\) Observation is very important; but its function is not that of producing theories. It plays its role in rejecting, eliminating, and criticizing theories; and it challenges to produce new myths, new theories which may stand up to these observational tests. Thus, scientific myths under the pressure of criticism become so different from religious myths. But, in their origin, remain myths or inventions just like the others.\(^{24}\)

**Hypothesis as Conjecture**

For Popper, such myths are conjectures, and criticism of such myths refutation. For him, interplay of such ‘conjectures and refutations’ is the source of scientific knowledge. This is at once both the method and the source of knowledge. This method is nothing but a critical discussion of a problem under consideration. For Popper, critical discussion often relies upon a considerable amount of common background knowledge. This does not mean that background knowledge is a priori, and cannot be critically discussed in its turn. It only means that criticism never starts from nothing.\(^{25}\)
For him, if a teacher tells a young scientist, “Go round and observe”, he is badly advised. But he is well advised if his teacher tells him, “Try to learn what people are discussing nowadays in science. Find out where difficulties arise, and take an interest in disagreements. These are the questions which you should take up.” In other words, you should study the problem-situation of the day. This means that you pick up, and try to continue, a line of inquiry, which has the whole background of the earlier development of science behind it.  

Popper says that the world is infinitely complex and we do not know where and how to start our analysis of this world. There is no wisdom to guide us; even a scientific tradition does not tell us as to where the point of departure is. It only tells us where and how other people started and where they got to. It tells us that people have already constructed in this world a kind of theoretical framework—not perhaps a very good one, but one which more or less works. It serves us as a kind of network, as a system of coordinates to which we can refer the various complexities of this world. We use it by checking it over, and by criticizing it. In this way, we make progress.

The crucial question regarding conjecture is as to how we make it. According to Popper, our conjecture starts always from problems—either from practical problems or from a theory, which has run into difficulties. Once we are faced with a problem, we may begin to work on it. We may respond to such a situation in two ways: Firstly, we may proceed by attempting a guess or conjecturing a solution to our problem, and secondly, we may attempt to criticize our usually somewhat feeble guess. Such a guess or a conjecture may withstand our criticism and experimental tests, for some time. But as a rule, we soon find that our conjectures can be refuted, or that they do not solve our problem, or that they solve it only in part. We also find that even the best solutions—those able to resist the most severe criticism of the most brilliant and ingenious minds—soon give rise to new difficulties leading to new problems. Thus, we may say that the growth of knowledge proceeds from old problems to new problems by means of conjectures and refutations.  

However, we may agree that our conjecture starts from problems, but we may still think that our problems must have been the result of observations and experiments. For, there can be nothing in our intellect which has not
entered it through our senses. That is, observation precedes problems. But the actual course happens to be fully different. Popper says that every animal is born with some expectations or anticipations, which could be framed as hypothesis - a kind of hypothetical knowledge. We have a measure of innate knowledge from which we may begin, even though it may be quite unreliable. This innate knowledge, this inborn expectation, will, if disappointed, create our first problems. And the ensuing growth of our knowledge may be described as consisting throughout of corrections and modifications of previous knowledge. That observation cannot precede all problems may be illustrated by a simple experiment. 'This experiment consists of asking you to observe, here and now. You are all cooperating, and observing! However, at least some of you, instead of observing, will feel a strong urge to ask: 'what do you want me to observe?' If this is your response then the experiment is successful. What we are trying to illustrate is that, in order to observe we must have in mind a definite question, which we might be able to decide by observation.\(^{28}\)

Popper says that observation is always selective. Popper asked some physics students in Vienna to take a pencil and a piece of paper and after careful observation write down what they have observed. They asked, of course, what he had wanted them to observe. The instruction 'observe' is absurd. So, it needs a chosen object, a definite task, an interest, a point of view, a problem. And its description presupposes a descriptive language; it presupposes similarity and classification, which in turn presupposes interests, point of view, and problems.

For Katz, even animals cannot do without selection. He says that an animal according to his needs, divides the environment into edible and inedible things. Popper says that this rule applies not only to animals but also to scientists. For an animal, a point of view is provided by its needs, the task of the moment, and its expectations. For the scientists the point of view is provided by his theoretical interests, the special problems under investigation, his conjectures and anticipations, and the theories which he accepts as a kind of background: his frame of reference, his horizon of expectation. For him, scientific knowledge is produced from observations and observations are produced from some hypothesis, in other words, from some conjecture in
response to a felt problem and an urge of resolving the problem in expected manner. The paradox ‘which comes first, hypothesis or observation?’ is responded to by Popper by maintaining the priority of hypothesis. It is quite true that any particular hypothesis we choose will have been preceded by observations—the observations, for example, which it is designed to explain. But these observations in their turn presuppose the adoption of a frame of reference, a frame of expectation, a frame of theory. Popper says, if they were significant, if they created a need for explanation and thus gave rise to the invention of hypothesis, it was because they could not be explained within the old theoretical framework, the old horizon of expectation. In this way, if we go back to more and more primitive theories and myths we shall in the end find unconscious, or ‘inborn expectations’ operating behind them.

According to Popper, it does not mean that the expectations are ‘inborn ideas’ but rather every organism has inborn ‘reactions’ or ‘responses’, and among them, responses adopted to impending events. These responses we may describe as ‘expectations’. The newborn baby ‘expects’ in this sense to be fed. He says, ‘inborn expectations’ may quite reasonably be called ‘inborn knowledge’ as well. This knowledge is not, however, valid a priori, for an inborn expectation, no matter how strong and specific, can be mistaken. Such expectations or knowledge may be valid a priori, only on psychological or genetic grounds, i.e. prior to all observational experiences. One of the most important of these expectations is the expectation of finding regularity. It is connected with an inborn propensity to look out for regularities, or with our need to find regularities.29

However, the expectation of finding regularities is not only psychologically a priori but also logically a priori. It is logically a priori to all observational experiences, for, it is prior to any recognition of similarities; and all observations involve the recognition of similarities. But in spite of being logically a priori in this sense the expectation is not valid a priori. We can easily construct an environment, which is so chaotic that, we completely fail to find regularities.30

However, we start with a problem, a difficulty. This problem may be practical or theoretical. Whatever it may be, when we first encounter the
problem we do not know much about it. At best, we have only a vague idea as to what our problem really consists of. How, then, can we produce an adequate solution? Obviously, we cannot. We must first get better acquainted with the problem. But how? Popper answers that we can do so by producing an inadequate solution, and by criticizing it. He says that only in this way can we come to understand the problem. For, to understand a problem means to understand its difficulties; and to understand its difficulties means to understand why it is not easily soluble— why the more obvious solutions do not work. We might therefore produce additional more obvious solutions. These solutions must be criticized in order to find out why they do not work. In this way we become acquainted with the problem and may proceed from bad solutions to better ones—provided that we have the creative ability to produce new guesses, and more new guesses.\textsuperscript{31}

Another prerequisite for starting our research, according to Popper, is ‘background knowledge’. For, to recognize some event as problematic we must have some preconceptions; be it mythical, metaphysical, fictional, or anything else, with which the event disagrees and is thereby considered as problematic. Moreover, to produce some solution or its criticism we must need some preconceptions that will regulate us. Without any connection, we cannot say anything. Popper says that while discussing a problem we always accept (if only temporary) all kinds of things unproblematic. These accepted things do constitute what Popper calls background knowledge. This background knowledge may operate as the starting point for the discussion of a problem under consideration. Few parts of this background knowledge will appear to us in all context as absolutely unproblematic, and any particular part of it may be challenged at any time, especially if we suspect that its uncritical acceptance may be responsible for some of our difficulties. But almost all of the vast amount of background knowledge, which we constantly use in any informal discussion will, necessarily remain unquestioned.\textsuperscript{32} We need it because we cannot start from nothing, we always start from some previous knowledge. We conceive of truth and approach to truth within the content of available background knowledge. According to our method ‘growth of knowledge’ consists in modification of previous knowledge—either it is alternation or is large scale rejection.\textsuperscript{33}
According to Popper, initially when our tentative solution is discussed
and criticized, everybody tries to find a flaw in it and refute it. Whatever the
result of these attempts may be, we certainly learn from them. If the criticism
of our friends or opponents is successful, we shall have learnt much about our
problem. We shall know more about its inherent difficulties than we do before.
And if even our most acute critics do not succeed, if our hypothesis is able to
resist their criticism, then again, we shall have learned much; both about the
problems and about our hypothesis, its adequacy and its ramifications. And as
long as it does better, in the face of criticism, than its competitors, it may
temporarily and tentatively be accepted as part of current scientific teaching.34

He says, “without waiting passively for repetition to impress or impose
regularities upon us, we actively try to impose regularities upon the world. We
try to discover similarities in it, and to interpret it in terms of laws invented by
us. Without waiting for premise, we jump to conclusion. These may have to be
discarded later, should observation show that they are wrong”.35 We jump first
to any theory and then while testing it try to find whether it is good or not; i.e.
by repeatedly applying the critical method, we eliminate many bad theories and
invent many new ones.36

This kind of discovery is quite metaphysical. Brody says, “While we
regard a physical theory as a hypothetical explanation of material reality, we
make it dependent on metaphysics”.37 Here metaphysics, as opposed to analytic
philosophy, is considered to be perfectly meaningful. Falsifiability is the
criterion which makes metaphysics quite meaningful for science. If
metaphysics is meaningful for science then we feel reassured that there is no
context of discovery. The theory may come from any source. The only criterion
is that such theories must be falsifiable or refutable.38

This view is called by Popper ‘critical rationalism’. This position is in
between Descartes’ ‘truth is manifest’ and Bacon’s ‘nature is open book’. Its
essential difference from them is that it strongly restricts our claim to
knowledge. There is no ultimate source of knowledge. Every source, every
suggestion is welcome; and every source, every suggestion, is open to critical
examination and can be subjected to interrogation, examination and criticism.
Clarity and distinctness are not criteria of truth, but obscurity and confusion do
Therefore, science must begin with myths and with the criticism of myths; in other words, science does begin with ‘conjecture and refutation’.

3.2 Scientific Progress through Falsification

Trial and Error

If science advances by the way of changing its traditional myths, then we need something with which to start. If we have nothing to alter and to change, we can never get anywhere. Thus we need two beginnings for science: new myths, and a new tradition of changing them critically.

‘Conjecture and refutation’ as the procedure of scientific knowledge makes it possible to understand that scientific theories are not the digest of observations but are inventions—conjectures boldly put forward for trial to be eliminated if they clash with observations. There is a question here if observation cannot be free of theory then how could the observation be the test of the theory. It is true that observations are rarely accidental. But as a rule observations are undertaken with the definite intention of testing a theory, by obtaining, if possible, a decisive refutation.

For him, every test of theory, whether resulting in its corroboration or falsification, must stop at some basic statement or other, which we decide to accept. If we do not come to any decision, and do not accept some basic statement or other then the test will have led nowhere. However, considered from a logical point of view, the situation is never such that it compels us to stop at this particular basic statement rather than at that, or else give up the test altogether. For any basic statement can, again in turn, be subjected to tests. This procedure has no natural end. Thus, if the test is to lead us to anywhere, we have to stop at some point or other and say that we are satisfied, for the time being.

Therefore, basic statement is a primitive statement, because this is the statement located at the starting point of our investigation. We do not make any investigation for its truth. In this sense, it is conventional also, for it is the statement which is taken for granted on the basis of agreement. ‘They are accepted as the result of decision. And accepted basic statements are the basis
for the corroboration of a hypothesis, and thus indirectly for falsification. Popper argues against the view that basic statements can be justified by reference to perceptions, which he regards as a kind of psychologism. He admits that the decision to accept a basic statement is causally connected with our experiences, but we do not attempt to justify a basic statement by these experiences. Experiences can motivate a decision and acceptance or rejection of a statement, but a basic statement cannot be justified by them—no more than by thumping the table.\footnote{However, we need basic statements in order to decide whether a theory is to be called falsifiable, i.e. empirical. And we need them for the corroboration of a falsifying hypothesis, and thus for the falsification of theories. Therefore, corroboration of a theory by any test premised on basic statements would be tentative, for, we do know that this test is not the only test, there may be many other tests that would lead to the falsification of the theory. But falsification of the theory would be final, for, we know that at least this very test falsifies the theory. Beside, there may be many other tests of this kind.}

The role of the basic statement in the method of ‘trial and error’ is established on the position of observation: although theories cannot logically be derived from observations, they can clash with observations, they can contradict observations. This makes it possible to infer from observations that a theory is false. The possibility of refuting a theory by observations is the basis of all empirical tests. Therefore, a theory is empirical not when it is produced through conjecture, but when it is tested by the observations. The test of a theory is always an attempt to show that the competitor theory is mistaken, that is, that theory entails a false assertion. From a logical point of view, all empirical tests are therefore attempted refutations.\footnote{Such an attempted refutation can be made only on the basis of observation. Therefore, conjecture gives us theory and observation gives us empiricallity of theory.}

Now the question is as to why do we put forward our theory to the trial? We need to do so because our theory which is mainly conjecture created by us may be constructed wrongly. Our conjecture is produced from the expectation of a solution—expectation for regularities. We have the propensity to look out for regularities, and to impose laws upon nature. Popper says that this propensity leads us to the psychological phenomenon of ‘dogmatic
thinking’. He says that we expect regularities everywhere and attempt to find them even where there are none. And we stick to our expectations even when they are inadequate. In this situation, without a trial, our theory will remain a myth. Only a critical discussion can make the conjecture a proper scientific theory. This dogmatism is to some extent necessary. It is demanded by the situation, which can only be dealt with by forcing our conjecture upon the world. Moreover, the dogmatism allows us to approach a good theory in stages, by way of approximation. Dogmatic attitude makes us stick to our first impressions, which is indicative of strong belief.\textsuperscript{45}

According to Popper, a theory needs to be under trial to be a scientific theory, as dogmatism is never a scientific attitude. What we call ‘science’ is differentiated from the myths not by being some thing distinct from a myth, but by being accompanied by a tradition of critically discussing the myth. The application of this tradition results in the acceptance or the rejection of the theory or perhaps in a compromise. This tradition is the critical or argumentative attitude. In a certain sense, sciences are myth-making just as religion is; but they are different from myth only because they adopt the critical attitude. As a result, the myth changes and changes in the direction of giving a better and better account of the world. And they change us to observe things, which we would never have observed without these theories or myths.\textsuperscript{46}

Critical attitude is ready to modify its tenets; it admits doubt and demands tests. This is indicative of our uncertainty with regard to the theories under consideration. For, the attitude is clearly related to the tendency to test them, to refute them. The attempt to sort out error through critical discussion is the characteristic of science. For Popper, this is the method of ‘trial and error’. This is also called the method of falsification; for, in this method we try to discover a mistake in a theory to falsify or eliminate it.

Conjectures may be considered as dogmatic rather than rational, but the ‘trial and error’ method makes them scientific. There is nothing irrational if we tentatively accept some theory with an eagerness to revise it with experiment. Popper says that if this is our task, then there is no rational procedure other than the method of ‘trial and error’. The method is not simply identical with the scientific method; this is applied not only by Einstein but
also, in a more dogmatic fashion, by the amoeba. The trial and error method fundamentally is the method used by any living organism in the process of adaptation. So, this is very natural to the human intellectual process. Men seem inclined to react to a problem either by putting forward some theory or by fighting against such a theory, once they detect its weakness. This struggle of ideologies, which is obviously explicable in terms of the method of ‘trial and error’, seems to be characteristic of any thing that may be called a development of human thought.  

Popper says that in this method we first boldly propose a theory then try our best to point out its possible errors. If our critical efforts do not succeed then we accept them tentatively. From this point of view, all laws, all theories, are essentially tentative, or conjectural or hypothetical, even when we feel unable to doubt them any longer. For, before a theory has been refuted we can never know in what way it may have to be modified. This method can easily by-pass the logical problems that arise against induction. The first is Hume’s discovery that it is impossible to justify a law by observation and experiment, since it ‘transcends experience’. In trial and error method, we may reject a law or theory on the basis of new evidence, without necessarily discarding the old evidence which originally led us to accept it. Secondly, the principle of empiricism which asserts that in science only observation and experiment may decide upon the acceptance or rejection of scientific statements, including laws and theories does not cut much ice. There are many historical examples when a theory has not been rejected even in the face of contradiction with the experimental facts. Newton’s theory had no complete experimental support. It appeared to be in disagreement with natural phenomena, yet it survived for centuries until Einstein’s theory appeared. According to the method of trial and error, a theory can be rejected only by that experiment which at the same time corroborates the falsifying hypothesis— the hypothesis that is presently competing.

By this method, we also can by-pass the question, which arose against induction: ‘why is it reasonable to believe that the future will be like the past’? In this method it is perfectly reasonable to act on the assumption that future will, in many respects, be like the past, and that well-tested laws will continue
to hold; but it is also reasonable to believe that such course of action will lead us at times into severe trouble, since some of the laws upon which we heavily rely may easily prove unreliable.\textsuperscript{50}

In the ‘trial and error’ method, we consciously attempt to make our theories or conjectures suffer in the struggle for the survival of the fittest. Our corroboration may be uncertain, but our falsification would be certain. However, the fittest theory is not fittest in pragmatic sense. “There are many false theories which 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”.\textsuperscript{51} On the contrary, they are fittest in the spirit of truth. In this method, we search for truth and the falsified theories are known or believed to be false while the non-falsified theories may still be true. The success of this method depends very largely on the number and variety of the trials: the more we try the more likely it is that one of our attempts will be successful.\textsuperscript{52} In this method, scientists ought to take into account less probable hypothesis at first and expose them to the most severe test. If a hypothesis is falsified it must be rejected. If it is not falsified it is corroborated, and temporarily persists. But this does not mean that it is true or even probable true; further tests sooner or later usually falsify it. The falsification is complete and conclusive, the corroboration is never so.\textsuperscript{53}

The physicist J.E. Wheeler uses the word ‘mistake’ in Popperian spirit when he comments about the learning process in science viz; “our whole problem is to make the mistake as far as possible”.\textsuperscript{54} Since “truth is hard to come by out of mistakes, it needs both ingenuity in criticizing old theories, and ingenuity in the imaginative invention of new theories”.\textsuperscript{55}

Illustrating ‘trial and error’ method Popper says that faced with a certain problem, the scientist offers, tentatively, some sort of solution—a conjecture or theory. Science accepts this theory only provisionally. Then criticism and testing go hand in hand; the theory is criticized from very many different points, which may be vulnerable. Theories are put forward tentatively and trial out. If the out come 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. Furthermore, ‘it is the critical procedure, which contains those choices, those rejections, and those decisions, which show that we have learnt from our mistakes, and thereby added to our scientific knowledge’. Its success depends mainly on three conditions: one, sufficiently numerous ingenious theories should be offered; two, the theories offered should be sufficiently varied and, three, sufficiently severe tests should be made. In this way we may, if we are lucky, secure the survival of the fittest theory by elimination of those which cannot compete in the struggle for survival.

According to Popper, this is like dialectic method of the development of human thought. Dialectic is a theory, which maintains that development is characterized by what is called the dialectic triad: thesis, antithesis and synthesis. First there is some idea, theory, or movement, which may be called a thesis. Such a thesis will often generate opposition, because, like most things in this world, it will probably be of limited value and will have its weak spots. This opposing idea or movement is called the antithesis, because, it is directed against the thesis. The struggle between the thesis and antithesis goes on until some solution is reached which, in a certain sense, goes beyond both thesis and antithesis by recognizing their respective values and by trying to preserve the merits and avoid the limitations of both. This solution, which is a third step, is called the synthesis. Once attained the synthesis in its turn may become the first step of a new dialectic triad. In this case opposition will be aroused again which means that synthesis can then be described as a new thesis, which has produced a new antithesis. The dialectic triad will thus graduate to a higher level, and it may again negotiate a higher level when a second synthesis has been attained.

For Popper, such a dialectic development may be explained by showing that it operates in conformity with the method of ‘trial and error’. But it is not exactly the same as the development of a theory by trial and error. The trial and error method deals only with an idea and its criticism, i.e. thesis and antithesis. And the struggle between an idea and its criticism or a thesis and antithesis leads not to a synthesis but to the elimination, and the competition of theories would lead to the adoption of a new theory only if enough theories are at hand and are offered for trial. In dialect, only one thesis is offered to start
with and the one is opposed to the other. But trial and error method is slightly wider and not confined to one idea and criticism.\(^9\)

Popper says that dialectic is slightly different from the trial and error theory in some other respect also. For, the trial and error theory will be content to say that an unsatisfactory view will be refuted or eliminated. On the other hand, a dialectician emphasizes that although the view may have been refuted, there will most probably be an element in it, which is worthy of preservation. For a dialectician—a thesis produces an antithesis leading to synthesis. For trial and error theory, it is only our critical attitude, which produces the antithesis. Criticism is, in a very important sense, the main motivating force of any intellectual development. Without contradiction, without criticism, there would be no rational motive for changing our theories. There would be no intellectual progress. Contradiction is fertile only in so far as we are determined not to put up with contradictions, and to change any theory which involves contradictions. If we put up with contradictions, it will no longer be productive of intellectual progress. Criticism is a self-corrective and open-ended process. It ought to or it does invite counter-criticism with gusto. While dogmatism may refuse to be criticized by asking as to 'why', criticism will have to respond to counter-criticism by 'why not'. Otherwise scientific progress will come to a standstill or dead end.\(^{60}\)

**Growth of Science**

However, there is no real danger that the growth of science will come to an end. Popper holds that our infinity of ignorance and our trial and error process of learning together ensures that science can never complete its task. Such a danger may be in authoritarianism of a criterion, but in our process of learning, there is no place for such an attitude. Here every theory is welcome but none has the final authority. Every theory must come under the severe pressure of criticism, for, it is the criticism which makes a theory scientific. There may be another danger from faith in formalization and precision, but since our producing theory starts from the expectation of some solution of the problem-situation and making the conjecture of the solution, there is no place for formalization. Conjecture being mainly metaphysical, there can never be any structural limitation as such. Conjecture opens up all kinds of possibilities
for imagination; so, danger lies only in the lack of imagination, which is attributable not to the science but to the scientists. This is not likely to happen that human being as a whole will become bereft of imagination; someone or the other does take a critical stand against an existing theory or prevailing orthodoxy.

Therefore, infinity of imagination or growth in the process of conjecture and refutation inseparably characterize the project of scientific research. Growth is of seminal significance in science. Apart from having vital social and political implications, scientific growth basically signals onward march of intellectual progress. The continued growth is essential to the rational and empirical character of scientific knowledge. If science ceases to grow it looses that character. It is the way of its growth which makes science rational and empirical, the way in which scientists differentiate amongst available theories and choose the better one or the way they give reasons for rejecting all the available theories thereby suggesting some of the conditions with which a satisfactory theory should comply.

However, Popper maintains that growth in the process of ‘conjecture and refutation’, is not the accumulation of observations but rather the repeated overthrow of scientific theories and their replacement by better or more satisfactory ones. So “it is not like a library, as more and more books accumulate so more and more knowledge accumulates. But scientific growth is by criticism: it grows by a method more revolutionary than accumulation—by a method which destroys, changes, and alters, the whole things including its most important instruments, the language in which our myths and theories are formulated”. The critical examination of theories of this sort leads us to subject them to further tests and, if we deem it necessary to overthrow them. This leads us to further experiments and observations of a kind which nobody would ever have dreamt of without the stimulus and guidance provided both by our theories and our criticism of them.

If there is competition of the sort stated just above between the existing and falsifying hypotheses, the competing theories become incomparable. How, then, will we understand the growth among the competing theories? According to Popper, “the case is that not only are all the theories
conjectural but also all appraisals of theories, including comparison of the theories, and including the observational tests. All appraisals of theories are appraisals of the state of their critical discussion". This condition in the long run leads either to dogmatism or to an infinite regress or to the relativistic doctrine of rationality; consequently, we are caught amongst a phethora of incommensurable frameworks. A rational and fruitful discussion is impossible unless the participants share a common framework of basic assumptions, or at least, unless they have agreed on such a framework for the purpose of the discussion. A discussion among participants who do not share a common framework may be difficult. A discussion will also be difficult if the frameworks have little in common, and it will be the easier the greater the overlap between the frameworks. In this context, popper says that we should logically distinguish between a mistaken method of criticizing, and the correct method of criticizing. The mistaken method of criticizing is what starts from the question: how can we establish or justify our theories? By contrast, the correct method of critical discussion starts from the question: what are the consequences of our theories? Are they all acceptable to us? Thus it consists in comparing the consequences of different theories and tries to find out which of the competing theories or frameworks has consequences that seem preferable to us. It is thus conscious of the fallibility of all our methods and it tries to replace all our theories by better ones.

We may say that more the fruitful discussion, more the participants learn from it. The more interesting questions and difficult questions they are asked, the more new answers they are induced to think of. Consequently, more they are shaken in their opinions, the more their intellectual horizon is extended. Fruitfulness in this sense will almost always depend on the original gap between the opinions of the participants in the discussion. The greater the gap, the more fruitful can the discussion be provided that such a discussion is not altogether impossible owing to incommensurability of frameworks.

However, even without a discussion there is also a possibility of a fruitful confrontation amongst people deeply committed to different frameworks. It is understandable that two ways of life and two ways of looking at the world are incommensurable. Yet, two theories, which try to
solve the same family of problems, including their offspring, need not be incommensurable. It is possible for us to transcend the two different frameworks—psychologically, socially and genetically or physiologically—by critical method. We can understand even a bit of the language of the bees. Admittedly, this understanding is conjectural and rudimentary. But almost all understanding is conjectural, and the deciphering of a new language is always rudimentary to start with. It is the method of science, the method of critical discussion, which makes it possible for us to transcend not only our culturally acquired but even our inborn frameworks. This method has made us transcend not only our senses but also our partly innate tendency to regard the world as a universe of identifiable things and their properties. Critical thought can challenge and transcend a framework even if it is rooted not only in our conventional language but also in our genetics. Revolution does not produce a theory incommensurable with its predecessors; the very task of the revolution is to explain the old category of thing-hood by a theory of greater depth.69

The myth of the framework is clearly the same as the doctrine that one cannot rationally discuss anything that is fundamental; or that a rational discussion of principles is impossible. This doctrine is logically an outcome of the mistaken view that all rational discussion must start from some principles. This is mistaken, for, behind it there is the tacit assumption that a rational discussion must have the character of a justification or of a proof or of a demonstration, or of a logical derivation from admitted premises. But the kind of discussion which is going on in the natural science might have taught us that there is also another kind of rational discussion: a critical discussion, which does not seek to prove or justify or establish a theory.70

Though in the method of ‘trial and error’ we do not and cannot justify a theory, we can, through this method, have a criterion, the criterion of criticism, to choose the better among the competing theories. This possibility eventuates scientific change into progress. For Popper, the history of science, like the history of all human ideas, is a history of irresponsible dreams, of obstinacy, and of error. But science is the one of the very few human activities in which errors are systematically criticized and fairly often corrected. We do make mistakes, but by recourse to continuous criticism, we often learn from
our mistakes. And this is why we speak clearly and sensibly that we can make progress in scientific endeavours. We have the criterion of criticism to choose among the competing theories and we can, as well, gradually eliminate errors. While scientific change signals progress, the process of change does not entail any loss. In almost all human endeavors, there is change, but rarely the progress, for, the gain is balanced by some loss. Given that elimination of the error is the result of progress, there is no loss in science. Such progress without incurring any loss is possible in science thanks to the criterion of criticism.

Although the world at any moment's necessarily conjectural or theory impregnated, this does not prevent us from progressing to better theories. How do we do it? The essential step is the linguistic formulation of our beliefs. This objectifies them; and this makes it possible for them to become targets of criticism. Thus, our beliefs are replaced by competing theories, by competing conjectures. And through the critical discussion of these theories we can progress and continuity graduate to better theoretical positions.

According to Popper, we may be able to say, by the criterion, whether it would be an improvement on other theories with which we are accounted. We have criterion of relative potential satisfactoriness, which can be applied to a theory even before we know whether or not it will turn out. This criterion of relative satisfactoriness is extremely simple and intuitive. It characterizes as preferable the theory which tells us more, 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. All these properties, which we desire in a theory, can be shown to amount to a higher degree of empirical content or of testability.

For him, all these properties can be stated as three requirements for the theory to get nearer to truth. The first requirement is that the new theory should proceed for some simple, new, powerful and unifying idea about some connection between hitherto unconnected things or facts or new theoretical entities. This requirement is called 'simplicity'. It seems to be intimately connected with the idea that our theories should describe the structural properties of the world—an idea which it is hard to think out fully without
getting involved in an infinite regress. Secondly, we require that the new theory should be independently testable, that is to say, apart from explaining all the explicanda, which it was designed to explain, it must have new and testable consequences. It must lead to the prediction of phenomena which have not so far been observed. If the second requirement is satisfied then the new theory will represent a potential step forward, whatever the outcome of the new tests may be. For, it will be better testable than the previous theory. Moreover, we will be confronted by the new problems to be solved by the new explanatory theories. And to some extent, it will be fruitful as an instrument of exploration. Thirdly, we require that the theory should pass some new, and severe tests. Some of the most interesting and most admirable theories ever conceived can be refuted at the very first test. For, even the greatest physicist can not anticipate the secretes of nature: his inspiration can only be guesses, and it is no fault of his or of his theory, if it is refuted. Even Newton's theory was in the end refuted; and we hope that we shall in this way succeed in refuting, and improving upon every new theory.

However, Popper offers a simple and obvious idea about the content of a theory and shows how science makes progress. He argues that 'the informative content of the conjunction, ab, of any two statements, a and b, will always be greater than, or at least equal to, that of any of its components. Let a be the statement 'it will rain on Friday'; b the statement 'it will be fine on Saturday'; and ab the statement 'it will rain on Friday and it will be fine on Saturday'. It is then obvious that the informative content of this last statement, the conjunction ab, will exceed that of its component a and also that of its component b. And it will also be obvious that the probability of ab will be smaller than that of either of its components.

Let us suppose that Ct(a) is 'the content of the statement a', and Ct(b) is 'the content of the statement b', and Ct(ab) is 'the content of the conjunction a and b'. we have

\[(1) \text{Ct}(a) \leq \text{Ct}(ab) \geq \text{Ct}(b)\]

This contrasts with the corresponding law of the calculus of probability.

\[(2) \text{P}(a) \geq \text{P}(ab) \leq \text{P}(b)\]
These two laws, (1) and (2), together state that with increasing content, probability decreases, and vice-versa. In other words, content increases with increasing improbability. Thus if our aim is the advancement or growth of knowledge, then a high probability (in the sense of probability calculus) cannot possibly be our aim as well; these two aims are incompatible.

Popper says that this head-on collision would be avoidable if people were not so generally inclined to assume uncritically that a high probability must be an aim of science. What is more important, one merely has to recognize that the property which we cherish in theories and which we may perhaps call ‘verisimilitude’ or ‘truth likeness’ is not a probability in the sense of the calculus of probability of which ‘P(a) ≥ P(ab) ≤ P(b)’ is an inescapable theorem. If we aim, in science, at a high informative content—if the growth of knowledge means that we know more, that we know $a$ and $b$, rather than $a$ alone, and that the content of our theories thus increases — then we have to admit that we also aim at a low probability, in the sense of the calculus of probability. And since a low probability means a high probability of being falsified, it follows that a high degree of falsifiability, or refutability, or testability is one of the aims of science—in fact, precisely the same aim as a high informative content. So, the criterion of potential satisfactoriness is thus testability or improbability. Only a highly testable or improbable theory is worth testing, and is actually satisfactory if it withstands sever tests. However, it is possible to compare the severity of a test objectively and to admit the explanatory power and the degree of corroboration of a theory.

This criterion can easily be illustrated with the help of historical examples. The theories of Kepler and Galelio were unified and superseded by Newton’s logically stronger and better testable theory, and similarly Fresnel’s and Faraday’s by Maxwell’s. Newton’s theory and Maxwell’s’ in turn, were unified and superseded by Einstein’s. In such a case, the progress was towards a more informative and therefore logically less probable a theory. A theory which is not in fact refuted by testing those new and bold and improbable predictions, to which it gives rise can be said to be corroborated by these severe tests. Galelio’s discovery of Neptune, Hertz’s discovery of electromagnetic wave, Edington’s eclipse observations can well remind us the fact. There are
other important discoveries, which lead not to corroboration, but to its refutation. For instance, Lavoisier's classical experiments do not establish the oxygen theory of combustion; yet they tend to refute the phlogiston theory.\textsuperscript{77}

Therefore, \textit{every refutation should be regarded not as a failure but as a great success; not merely a success of the scientist who refuted the theory, but also the scientist who created the refuted theory and who thus in the first instance suggested the refuting experiment.} Even if a new theory should meet an early death, it should not be forgotten; rather its beauty should be remembered and history should record our gratitude to it—for bequeathing to us new and perhaps still unexplained experimental facts and new problems, and for services it has thus rendered to the progress of science during its successful but short life. The refuted theory is always important. For, our aim as scientists is to discover the truth about the problems and we must look at our theories as serious attempts to find the truth. If they are not true, they may be important stepping-stones towards the truth, instruments for further discoveries.\textsuperscript{78}

History of science may say that the individual scientist wishes to establish his theory rather than refute it. But from the point of view of progress in science, this wish can easily mislead him. Moreover, if he does not himself examine his favorite theory critically, others will do so for him. The only result, which will be regarded by them as supporting the theory, will be the failure of interesting attempts to refute it; failure to find counter examples where such counter examples would be most expected, in the light of the best of the competing theories. Thus, it need not create a great obstacle to science if the individual scientist is biased in favor of a pet theory. However, Cloude Bernard's comment viz; ‘those who have an excessive faith in their ideas are not well fitted to make discoveries’\textsuperscript{79} deserves our serious attention and consideration.

Popper holds that we need not only successful refutations, but also positive success. We must manage reasonably often to produce the theories that contain new predictions, especially predictions of new effects, new testable consequences suggested by the new theory and never thought of before. ‘If we should only succeed in refuting our theories but not in obtaining some verifications of predictions of a new kind, it means that our problem has
become so difficult that the structure of the world is beyond our powers of comprehension. So, we need two kinds of successes: success in refuting our theories, and success on the part of some of our theories in resisting at least some of our most determined attempts to refute them. In this way we make the progress in science.

The above lines suggest that science progresses from theory to theory by eliminating the false theory. And this consists of a sequence of better and better theory, in other words, better deductive systems. However, a scientific theory is an attempt to solve a scientific problem, which is connected or concerned with the discovery of an explanation. Therefore, science should be visualized as progressing from problems to problems—to problems of ever increasing depth. The conscious task before the scientists is always the solution of a problem through the construction of a theory, which solves the problems, for example by explaining unexpected and unexplained observations. Thus science always starts from and ends with problems—problems of an increasing depth, and an ever increasing fertility in suggesting new problems. So science grows from problem to problem; science progresses towards the increasing depth of the problems by gradual elimination of errors. Every change renders progress, for in every refutation we eliminate our mistakes, thus enriching our knowledge.

### 3.3 The Problem of Realism with Falsification

**Truth as Regulative Principle**

If science progresses from problem to problem, what about truth that is most interesting about progress? Before discussing the question, we should see what is the truth we expect in progress. It is very simple and obvious that we expect the objective truth. However, Tarski re-established a theory of absolute or objective truth, which shows that we are free to use the intuitive idea of truth as correspondence with the facts. The highly intuitive character of Tarski’s ideas seems to become more evident if we first decide explicitly to talk ‘truth’ as a synonym for ‘correspondence with the facts’, then we proceed to define the idea of ‘correspondence with the facts’. There are two formulations, each of
which states very simply under what conditions a certain assertion corresponds to the facts.

(1) The statement, or the assertion, ‘snow is white’ corresponds to the facts if, and only if, snow is, indeed, white.

(2) The statement, or the assertion, ‘Grass is red’ corresponds to the facts if and only if, grass is, indeed, red.

This is an objective notion of truth. There are many other notions of truth that are subjective in their character. The coherence theory holds ‘consistency’ for truth, the evidence theory holds ‘known to be true’ for truth, and the pragmatic or instrumentalist theory holds ‘usefulness’ for truth. These are all subjective or epistemic theories of truth. They are subjective in the sense that they all stem from the fundamental subjectivist position which can conceive of knowledge only as a special kind of mental state, or as a disposition; or as a special kind of belief, characterized, for example, by its history or by its relation to other beliefs. Whereas, Tarski’s is objective or metaphysical theory of truth. The objective theory of truth leads to a very different attitude. This may be seen from the fact that it allows us to make assertions such as the following: a theory may be true even though nobody believes it, and even though we have no reason for accepting it, or for believing that it is true. A similar assertion, which the correspondence theory would make quite naturally, is this: even if we hit upon the true theory, we shall, as a rule, be merely guessing, and it may well be impossible for us to know that it is true. On the other hand, a theory may be false, although we have comparatively good reasons for accepting it.82

This is the theory of objective or absolute truth. The question is do we achieve this kind of truth in scientific progress through the method of ‘trial and error’? We in science seek for highly informative theory, while the highly informative theory has the low probability to be true. In other words, if our new theory is more informative than the old one, it means that the new would, in contrast to the old, have low probability to be true. From this point of view, we do obviously not obtain the truth or approach the truth. Then, how will we conceive of the theory as progressing to achieve the truth stated above? So far.
we have seen our learning process ‘conjecture and refutation’, it is obvious that by our method, we seek truth, and to do that we eliminate falsity. Thus, it is also obvious that science allows us to say that we search for truth. Scientific change, occurred by our constant attempt to falsify the existing theory, is a change for real.\textsuperscript{83}

In this context we need to analyze our learning process to find out what do we obtain through it? However, one great advantage of the process of ‘conjecture and refutation’ is that it allows us to say that we carry on search for truth, though we may not know when to have found it. According to Popper, this method does not give us any criterion of truth but we are nevertheless guided by the idea of truth as a regulative principle. He further says that though there is no general criterion by which we can recognize truth, there is something like criterion of progress towards the truth. According to him, the status of truth in the objective sense, as correspondence to the facts, and its role as a regulative principle, may be compared to that of a mountain peak, which is permanently, or almost permanently, wrapped in clouds. The climber may not merely have difficulties in getting there—he may not know when he gets there, for, he may be unable to distinguish, in the clouds, between the main summit and some subsidiary peaks. Yet this does not affect the objective existence of the summit, and if the climber tells us ‘I have some doubts whether I reached the actual summit’, then he does, by implication, recognize the objective existence of the summit. The very idea of error, or doubt implies the idea of an objective truth which we may fail to reach. He says that though it may be impossible for the climber ever to make sure that he has reached the summit, it will often be easy for him to recognize that he has not reached it, or not yet reached it; for example, when he is turned back by an overhanging wall. Similarly, there will be cases when we are quite sure that we have not reached the truth. Coherence may not be a criterion of truth — for even a demonstrably consistent system may be false in fact— incoherence does establish falsity. So, for him, if we are lucky, we may discover inconsistencies and use them to establish the falsity of our theories.\textsuperscript{84}

Verificationists demand that we accept a belief only if it can be justified by positive evidence; that is to say, shown to be true. However, as
falsificationists we believe that we never give positive reasons justifying the belief that a theory is true. Therefore, science has nothing to do with the quest for certainty or probability or reliability. In our theory of falsification, 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 where we are mistaken; in the hope of learning from our mistakes; and if we are lucky, we hope also of proceeding to better theories. Popper, therefore, calls falsificationist the negativist.\(^5\)

Though negativists, we follow a critical discussion in search of mistakes with the serious purpose of eliminating as many of these mistakes as we can, in order to get nearer to truth. So truth here is a regulative principle, for, all our scientific activities are conducted in the hope of getting nearer to truth but actually at every step of theory change, we just liberate ourselves from the falsity rather than obtaining the truth. This kind of truth is a ‘formal’ or ‘procedural’ truth. There is the concept of ‘substantive truth’ but that is independent of the method. In other words, the substantive truth being beyond method regulates it.\(^6\)

In view of the same, Popper says, we will have to accept that the task of science is the search for truth—for true theories. And since our search for truth starts from the expectation for some solution of the some relevant and interesting problems, our research is thus for the relevant and interesting truth. Therefore, we stress that truth is not the only aim of science. We want more than mere truth. What we look for is interesting truth—truth which is hard to come by. And in the natural sciences what we look for is truth which has a high degree of explanatory power. Mere truth is not enough; what we look for is answer to our problems — a difficult, a fertile problem, a problem of some depth. Popper says that we are as much interested in truth as the member of a court of justice. When the judge tells a witness that he should speak ‘the truth, the whole truth, and nothing but the truth’, then what he looks for is as much of the relevant truth as the witness may be able or willing to offer. A witness who likes to wonder off into irrelevancies is unsatisfactory as a witness, even though these irrelevancies may be truisms, and thus part of ‘the whole truth’. It is quite obvious that what the judge—or anybody else—wants when he asks for
'the whole truth' is as much interesting and relevant true information as can be got. And many perfectly candid witness have failed to disclose some important information simply because they were unaware of its relevance to the case. And we are interested in bold conjectures due to the methodological conviction that only with the help of such bold conjectures can we hope to discover interesting and relevant truth.

**From Truth to Verisimilitude**

Scientific research aims at the objective truth. It also conjectures the interesting truth that is relevant in the context of human epistemological and methodological struggle. Then, how does the conjectural and human truth make sense in the context of scientific search for objective truth? How does conjectural and human truth approach to objective truth? We see a solution of the question in Popper's method of trial and error. He says that the idea of 'approach to truth' is based on the assertion that 'one theory corresponds better to the facts than the other.' But can we really speak of better correspondence? Is there such a thing as degree of truth? If an earlier theory is T₁ and a later theory is T₂, then has T₂ superseded T₁, or progressed beyond T₁, by approaching more closely to the truth than T₁? He says that 'there is no doubt whatever that we can say of a theory T₂ that it corresponds better to the facts; or that as far as we know it seems to correspond better to the facts, than another theory T₁. To clarify the issue Popper mentions the following list of six types of cases in which we should be inclined to speak of better correspondence.

1. T₂ makes more precise assertions than T₁, and these more precise assertions stand up to more precise tests.
2. T₂ takes account of, and explains, more facts than T₁.
3. T₂ describes, or, explains, the facts in more detail than T₁.
4. T₂ has passed test which T₁ has fail to pass.
5. T₂ has suggested new experimental tests, not considered before T₂ was designed and T₂ has passed these tests.
6. T₂ has unified or connected various hitherto unrelated problems.
If we reflect upon this list, we can see that the content of the theories $T_1$ and $T_2$ plays an important role in it. For, in our list of six cases, the empirical content of theory $T_2$ exceeds that of theory $T_1$. This suggests that we combined here the idea of truth and the idea of content into one. Therefore, every statement or theory is not only either true or false but has independent of its truth value—some degree of 'likeness', which is defined in terms of truth and falsity content. Popper calls this 'likeness' 'verisimilitude'. The idea of degree of better correspondence to truth is therefore the degree of greater 'likeness' or verisimilitude to truth.88

But how is it possible for 'truth and falsity content' to define the 'degree of likeness', in other words, degree of truth? Popper has offered a solution of this question in his book *Logic of Scientific Discovery*. He says that the content of a statement $a$ is the class of all the logical consequences of $a$. If $a$ is true, then this class can consist only of true statements, for, truth is always transmitted from a premise to all its conclusions. But if $a$ is false, then its content will always consist of both true and false conclusions. For example, 'it always rains on Sunday' is false, but its conclusion that 'it rained last Sunday' can happen to be true. Thus, there may be more or less truth in a statement in so far as its content consists of a greater or lesser number of true statements.

Popper calls this class of the true logical consequences of $a$ the 'truth-content' of $a$; and the class of the false consequences of $a$ the 'falsity-content' of $a$. Assuming that the truth-content and the falsity-content of two theories $T_1$ and $T_2$ are comparable, we can say that $T_2$ is more closely similar to the truth, or corresponds better to the facts, than $T_1$, if and only if either:

(a) The truth-content but not the falsity content of $T_2$ exceeds that of $T_1$,  
(b) The falsity-content of $T_1$, but not its truth-content, exceeds that of $T_2$.

If we now work with the assumption that the falsity-content and the truth-content of a theory $a$ are in principle measurable, then we can go slightly beyond this definition and define a measure of the verisimilitude or truthlikeness of $a$, in symbol $V_s(a)$. The simple definition will be:

$$V_s(a) = C_t(a) - C_f(a).$$

where $C_t(a)$ is a measure of the truth content of $a$, and $C_f(a)$ is a measure of falsity content of $a$. It is obvious that
Vs(a) satisfied our two demands, according to which Vs(a) should increase, if and only if either:

(a) If CtT(a) increase while CtF(a) does not, and
(b) If CtF(a) decreases while CtT(a) does not.

This is the approximation to truth, or verisimilitude. However it also has the same regulative character as the absolute truth. According to our learning process, 'conjecture and refutation' everything is conjecture. Producing theory, falsifying hypothesis, observational test, and even our knowledge about the approximation to the truth and everything else undergoes attempted refutation. So our knowledge of approximation is only a guess. But we can examine our guess critically, and if it withstands severe criticism, then this fact may be taken as a good critical reason in favour of it (approximation to truth).^89

Popper says that verisimilitude so defined, the maximum verisimilitude would be achieved only by a theory, which is not only true, but also completely comprehensively true. It means it would correspond to all facts, as it were, and only to real facts. He concedes this to be a remote and unattainable ideal than a mere correspondence with some facts. But all this holds only for the maximum degree of verisimilitude, and not for the comparison of theories with respect to their degree of verisimilitude. The idea of higher or lower degree of verisimilitude seems less remote and more applicable and therefore perhaps more important for the analysis of scientific methods than the idea of absolute truth itself.^90

Popper mentions another role of verisimilitude. Even after T2 has been refuted in its turn, we can still say that it is better than T1. For although both have been shown to be false, the fact that T2 has withstood tests which T1 did not pass, may be a good indication that the falsity content of T1 exceeds that of T2, while its truth content does not. Thus, we may still give preference to T2, even after its falsification, because we have reason to think that it agrees better with the facts than did T1. Similarly, a theory T2 that is more precise than T1 has a higher degree of verisimilitude than T1. Newton’s dynamics, for example, even though we may regard it as refuted, has of course, maintained its superiority over Kepler’s and Galileo’s theories. The reason is its greater
content or explanatory power. Newton’s theory continues to explain more facts than did others, continues to explain them with greater precision and unify the previously unconnected problems of celestial and terrestrial mechanics.\(^9\)

In the final analysis, we should not be confused about the difference between ‘verisimilitude’ and ‘probability’, because ‘likely’ is the other word for ‘probability’ which comes originally from ‘like the truth’ or ‘verisimilar’. However, progress in science means progress towards more interesting and less probable theories, and this means, as a rule, progress towards less familiar and less comfortable or plausible theories. So, theory of greater verisimilitude is totally different idea of ‘probability’. Both they are different in an important respect. The logical probability represents the idea of approaching logical certainty, tautological truth, through a gradual diminution of informative content. Verisimilitude, on the other hand, represents the idea of approaching comprehensive truth. It thus combines truth and content while probability combines truth with lack of content.\(^9\) Popper says that if we take the word ‘probable’ in any of the many senses which satisfies the calculus of probability, then it can never be shown to be ‘probable’.\(^9\) Moreover ‘Probability statements are not falsifiable’.\(^9\) Thus we should not attribute truth, or probability to our theories. The use of such standards as truth, and approximation to truth, plays a role only within our criticism. We may reject a theory as untrue; and we may reject a theory as being less close or approximate to truth than one of its predecessors or competitors.\(^9\)

Therefore, verisimilitude is neither ‘truth’ nor ‘probability’, it is kind of truth at the level of our interest. It is comprehensive truth. On the other hand, absolute truth is regulative of this comprehensive truth or verisimilitude. Our gradual achievement of verisimilitude at its highest reach will still remain the verisimilitude, for, it is essentially verisimilitude, not truth. Our theory being conjectural, producing theory and its criticism is an endless process. In the learning process, we always find a refuting hypothesis and no hypothesis has the authority not to undergo criticism. The issue of empirical science in this way appears fully agnostic.\(^9\)

Then what do we do when we search for truth? According to Popper, we search, then, for greater verisimilitude only. He explains this position as
following: Let us take a square as representing the class of all statements, and divide it into two equal sub areas, the true statements (T), and the false ones (F):

Now the task of science is to cover by hits as much as possible of the target (T) of the true statements, by the method of proposing theories or conjectures that seem promising, and as little as possible of the false area (F). It is very important that we try to conjecture true theories but truth is not the only property of our conjectural theories. For, we are not particularly interested in proposing trivialities or tautologies. ‘All tables are tables’ is certainly true—it is more certainly true than Newton’s and Einstein’s theories of gravitation, but it is intellectually unexciting. That is why scientific research is least interested in such tautological, sure and certain trivialities.

According to Popper, we are after interesting and enlightened truth, we are after theories that offer solutions of interesting problems. If at all possible, we are after deep theories. When we speak about approach to truth, it means ‘the whole truth’ that is, the whole class of true statements, the class T. A false theory may appear to be nearer to truth than other false theory. For example, ‘it is now 9.45 p.m.’ seems nearer to truth than ‘it is now 9.40 p.m.’ if in fact it is 9.48. Therefore, the idea of higher or lower verisimilitude is applicable both to false and true statements. Their truth content is essential in our context. In this sense we can say that the aim of science is the better approximation to truth, greater verisimilitude.97

Newton never believed, Popper says, that his theory was really the last word, and Einstein never believed that his was more than a good approximation to the true theory. This is because the truth, in other words, the class of all true statements is unattainable. What we can do is to increase the truth content and decrease the falsity content. Now, will our long struggle of increasing truth content and decreasing the falsity content at some time end up in the attainment of all true statements or will verisimilitude ever culminate into the absolute truth? Such a situation is impossible of attainment as our theory in its’ essence is conjectural. A conjectural theory is characterized by two things: (a) there is
no restriction to produce another conjecture better than the first; every previous knowledge makes the scientists more competent to produce the competent theory to refute previous ad infinitum and (b) Our theory being mere conjecture we cannot be certain for any assertion to be true. As a result, there always is the scope for any other conjecture to be shown better by any trick of the imagination or creativity. In that case, we may regard, with our capacity at the time, the truth as falsehood and vice-versa, because the conjecture does not possess any absolute criterion of truth. Our method says that no corroboration is conclusive. And if every previous theory is false according to the method, then meta-induction allows us to say that every future theory, when it is past by other, will also be false. At this point, Popper says, scientific community ought to be, and to a considerable degree actually is, an open society in which no theory, however dominant and successful is ever sacred.98

There may arise another question that if truth is illusive as considered in our learning process and if our theory is the creation of our imaginative mind, then is it not that scientific knowledge is essentially subjective? For Popper, it would be so if our method had claimed to know the reality. For, theory of knowledge has a certain essential subjectivity. The question such as ‘How do I know?’ and ‘what do I know?’ start inevitably from personal experience. Its data are egocentric. But in our method of ‘trial and error’, we admit that our knowledge is guesswork. Therefore, the question ‘How do I guess?’ and ‘what do I guess?’ are not analogous at all. Popper says that this question is psychological; it has no epistemological impact. The difference between ‘How do I know? What do I know?’ and ‘How do I guess? What do I guess?’ is as to the former ‘I do know’ but to the latter ‘I do not know’. In our method, we do not prove that our guesses are correct, but we are most anxious to have them criticized in order to replace them if possible by better guesses. The moment we replace the idea of knowledge by that of guesswork, the apparently ‘essential subjectivity’ of the theory of knowledge disappears. Guesses, as opposed to this, are proposals, and as such may be met by anybody’s counter-proposals.99

The shift from ‘knowledge’ to ‘guess work’ necessitates that all laws and theories be deemed to be hypothetical or conjectural. We must regard them
to be guesses. Therefore, from a rational point of view, we should not rely on any theory, for, no theory has been shown to be true, or can be shown to be true. Nevertheless, we have reason to prefer as basis for action the best-tested theory. The shift from 'truth' to 'verisimilitude' and from 'know' to 'guess' gives us a middle position. For, in relation to truth we cannot speak of the subjective part of theorizing, when producing a conjecture. The idea of verisimilitude we can incorporate both parts:-- subjective and objective. In our method of 'conjecture and refutation' the subjective part is in conjecture, because we are allowed here to use our creative imagination, to use myth; and the objective part is in the process of refutation, because our criticism is always directed by the regulative idea of truth, and any body can propose his counter-proposal for severe criticism. Although the work of rejection, in other words, the work of criticism by severe test gives us objective result, the result may not correspond to reality, because our criticism too is the conjecture of another sort. In this method, we know what is false but we do not know what is true.

To summarise the above discussion, we can say that for Popper fundamental scientific theories are by nature hypothetical. We have analysed the process of scientific change with a view to understand whether such a change as conceived by Popper entails the progress or not, and also whether such progress entails the gradual attainment of truth or not. For Popper, although hypotheses are falsifiable, every falsification culminates into progress, and every step of progress aims at truth.

However, all these issues chiefly depend on the nature of fundamental scientific theories which Popper calls falsifiable hypotheses. For Popper hypotheses may originate from myth, metaphysics, fiction whatsoever, but they can be scientific if they are falsifiable. They are myth-making, for they are produced from the imaginative mind of the scientists – they are not abstracted from the observations. This position denies induction as the method and verification as the criterion of a scientific statement. Rather, falsifiability is the criterion which, to begin with, accords scientific status to such hypothesis. Falsifiability is the criterion which makes metaphysics quite meaningful or relevant for science.
For Popper, only a critical discussion can make any conjecture a proper scientific theory. What we call 'science' is differentiated from the myths not by being something distinct from a myth, but by being accompanied by a tradition of critically discussing the myth. Conjectures may be considered as dogmatic rather than rational, but the 'trial and error' method makes them scientific. There is nothing irrational if we tentatively accept some theory with an eagerness to revise it with experiment. Popper maintains that growth in the process of 'conjectures and refutations' is not the accumulation of observations but rather the repeated overthrow of scientific theories and their replacement by better or more satisfactory ones. So it is not like a library, as more and more books accumulate so more and more knowledge accumulates. But scientific growth occurs in a way that is more revolutionary than accumulation. In its growth science destroys, changes and alters everything including its most important instruments and language. Yet science makes progress, for if a hypothesis falsifies the other, the falsifying hypothesis is better than falsified one. Therefore, every falsification is considered as growth.

Popper holds that in such a growth we gradually get closer and closer to truth. For him, when we falsify a theory by another one, it means that we eliminate some mistakes from our previous knowledge. The elimination of falsity - content of knowledge logically entails increase in truth-content. However truth attained in such a progress is 'regulative truth', for we do not achieve truth directly, but it is conceived only through eliminating falsity. For Popper, we may mistakenly falsify a true theory and corroborate a false one. So we do not have such a criterion by which truth could be sorted out categorically. Yet the ongoing process of falsification is directed towards truth. So, truth in science is regulative truth, which is assessed through a mutual calculation of truth-content and falsity-content, a degree of truth which Popper calls verisimilitude. Verisimilitude is not just truth, but truth-likeness. So through scientific progress as conceived by Popper we do not attain the absolute truth, rather something like truth.
References

2. Ibid., p. 7.
3. Ibid., pp. 8-9.
4. Ibid., pp. 15-19.
5. Ibid., p. 16.
6. Ibid., p. 25.
7. Ibid., p. 27.
8. Ibid., p. 95.
10. Ibid., p. 28.
11. Ibid., p. 29.
13. Ibid., p. 23.
17. Ibid., p. 42.
18. Ibid., pp. 86-87.
25. Ibid., p. 240.
26. Ibid., p. 129.
28. Ibid., p. 259.
30. Ibid., p. 48.
34. Ibid., p. 261.
36. Ibid., p. 55.
41. Ibid., p. 46.
45. Ibid., p. 49.
46. Ibid., p. 127.
47. Ibid., p. 312.
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49. Ibid., p. 54.
50. Ibid., p. 56.
51. Ibid., p. 56.
52. Ibid., p. 312.
57. Ibid., p. 222.
58. Ibid., pp. 313-314.
59. Ibid., p. 314.
60. Ibid., pp. 316-317.
61. Ibid., p. 216.
62. Ibid., p. 215.
63. Ibid., p. 129.
66. Ibid., pp. 59-60.
67. Ibid., p. 36.
68. Ibid., p. 36.
69. Ibid., pp. 55-59.
70. Ibid., p. 59.
74. Ibid., pp. 241-242.
75. Ibid., p. 242.
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81. Ibid., p. 222.
82. Ibid., pp. 223-225.
85. Ibid., pp. 228-229.
88. Ibid., pp. 232-233.
89. Ibid., pp. 233-234.
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Chapter – 4

The Status of Progress in Paradigm-shift

4.1 Nature of Paradigm

What is Paradigm?

In science all achievements are not of the same level. There are some such achievements that some particular scientific community acknowledges as supplying the foundation for its further practice. These achievements expound the body of accepted theories and illustrate many or all of its successful applications with exemplary observations and experiments. Aristotle’s Phisica, Ptolemy’s Almagest, Newton’s principia and optics, Franklin’s Electricity, Lavoisier’s Chemistry, Lyell’s Geology and many other works are achievements of this kind. They serve, for a time, implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners. This level of achievements is sufficiently unprecedented to attract an enduring group of adherents away from competing modes of scientific activity. And they are sufficiently open-ended to leave all sorts of problems, for the redefined group of practitioners, to resolve. This kind of achievements are stipulated as paradigms by Kuhn.

For him, paradigms are some accepted examples of actual scientific practice. They include law, theory, application and instrumentation. They provide models from which there spring up particular traditions of scientific research. Ptolemaic or Copernican astronomy, Aristotelian or Newtonian dynamics, corpuscular optics or wave optics are examples of paradigms. Men whose research is based on a shared paradigm are committed to the same rules and standards for scientific practice. This commitment and the apparent consensus, are prerequisites for normal science. Normal science means research firmly based upon one or more past scientific achievements.\(^1\) Seen sociologically, a paradigm is a set of scientific habits. By following them successful problem-solving be it intellectual, verbal, behavioral, technological, any or all of them can be carried on. It depends on the type of problems being solved.\(^2\)

According to Kuhn, without a paradigm scientific development cannot be counted and conceived of. In the absence of a paradigm or some candidate
of paradigm, all of the facts—that could possibly pertain to the development of a given science, are likely to seem equally relevant. As a result, mere fact gathering becomes nearly random activity. Furthermore, in the absence of a reason for seeking some particular form of recondite information, mere fact gathering is usually restricted to the wealth of data. And then it becomes a pool of casual facts, observations and experiments. Kuhn says that mere fact gathering; for example, Pliny’s encyclopedic writings or Baconean natural histories, produces a morass. Then one hesitates to call this literature science though this is filled with information, some of which is very recondite. This fact-gathering is necessary in the pre-paradigm stage, but paradigm is necessary for this fact-gathering to be a significant science. Kuhn says that what the fluid theory did for electricians is that it suggested which experiments would be worth performing and which would not. This theory is highly directed to the fact-collection and theory-articulation. As a result, it removes the confusion. He mentions Bacon’s statement that truth emerges more readily from error than from confusion.\(^3\)

According to J. L. Aronson, scientific paradigm is a network of rules and commitments that make sense of a variety of interrelated scientific activities. We compare different scientific theories not in terms of their outward formal structures but rather in terms of their respective networks of rules and commitments. Science is just like any other human activity, subject to its own set of rules and forms of life.\(^4\)

However, once a Physics textbook told the students that light is transverse wave motion. But during the eighteen century the paradigm for this field, provided by Newton, taught that light was a material corpuscle. This is a transformation of the paradigms, which is called scientific revolution, and this successive transformation from one paradigm to another via revolution is the usual developmental pattern of mature science.\(^5\)

In this development there are different stages of affairs, respectively: non-paradigm science, multiple-paradigm science and dual-paradigm science. Non-paradigm science is the state of affairs right at the beginning of the process of thinking about any aspect of the world, i.e. the stage when there is no paradigm. Kuhn says that at this stage only the easily accessible facts are
collected in a casual manner, where all facts seem equally relevant. And these different and overlapping sets of facts are interpreted in differing metaphysical or quasi-fanciful ways. At this stage, there can be a sort of scientific research that is non esoteric. Though the practitioners are scientists, the net result of their activity is something less than science. This is a pre-scientific philosophical stage.

Secondly, in multiple-paradigm science, far from there being no paradigm, there are on the contrary too many. Within the sub-field defined by each paradigmatic technique, technology can sometimes be quite advanced and puzzle-solving research can progress. But each sub-field as defined by its technique is so obviously narrow and trivial than the field as defined by intuition. Thirdly, during the period of crisis, however, just before a scientific revolution, Kuhn says, many of the characteristics of pre-paradigm science again set in. During this period there are always two competing paradigms struggling for the mastery. This is considered as dual-paradigm science.

However, paradigm is mere a model or pattern, or is rarely an object of replication. Kuhn says that unlike judicial decision in the common law, paradigm is an object for further articulation and specification under new or more stringent conditions. The success of a paradigm is largely a promise of success discoverable in selected and still incomplete examples. Normal science is the actualization of that promise. And actualization is achieved by extending the knowledge of facts that the paradigm displays as particularly revealing and by increasing the extent of the match between those facts and the paradigm's preconditions. Laudan says that paradigm is the primary element of scientific thought which offers a model of scientific tradition and activity. Paradigm is way of looking at the world, broad quasi-metaphysical insights or hunches about how the phenomena in some domain should be explained. However, paradigm is distinguished from hypothetic-deductive theory. In hypothetic-deductive theory, premise is fixed, and we know what it is all. On the contrary, paradigm is a 'way of seeing', where the more we articulate the paradigm the more we know what it is. Paradigm is a metaphysical model that more or less comprises the scientific community's beliefs about Nature.
Paradigm Determines all Scientific Activities

Paradigm determines the normal science which we call problem-solving research. Normal science is a practice that performs the work of actualization of the paradigm even though the techniques and standards of actualization are provided by paradigm too. Therefore, normal science is not an independent enterprise. Losee says, ‘normal science is a conservative enterprise.’

However, all activities of normal science are determined by paradigm. In scientific research, most scientific observations consume much time, equipments and money. Here the paradigm determines on what aspects of nature do scientists report?, what determines their choice?, what motivates the scientists to pursue that choice to a conclusion?, etc. Kuhn says that paradigm does all these things for normal science. Research cannot go with confusion. Thus paradigm provides a vision for normal science. But this vision of normal science is drastically restricted vision. This restriction is born from confidence in a paradigm and is essential to the enterprise to be normal science. Syncrotons and radiotelescopes are the examples of how much paradigm is necessary for the scientists. Scientists undertake research only when a paradigm assures them that the facts they seek are important. Furthermore, from Tycho Brahe to E. O. Lawrence, some scientists have acquired great reputations, not for any novelty of their discoveries, but for the precision, reliability and scope of the methods they developed for redetermination of a previously known sort of facts. It indicates that paradigm determines the importance of the research topic.

From this point, another aspects comes out that normal science seems an attempt to force nature into the relatively inflexible box that the paradigm supplies. As a result, no part of the aim of normal science is to call forth new sort of phenomena, and nor do scientists normally aim to invent new theories. Instead, normal scientific research is directed to the articulation of those phenomena and theories that the paradigm already supplies. Even determination of fact, which is one of the most important works of normal science, is not independent of paradigm. Rather it is directed to those facts that can be compared directly with the predictions of the paradigm. History of
science tells us that Atwood’s Machine—first invented almost a century after the principia had appeared—gave the first unequivocal demonstration of Newton’s second law; special telescope is designed to demonstrate the Copernican prediction of annual parallax; Foucault’s apparatus was directed to show that the speed of light is greater in air than in water. These and many others like them illustrate the immense effort and ingenuity, that have been required to bring the nature and the theory into closer and closer agreement. But the attempt to demonstrate this agreement also is obviously more dependent upon a paradigm than Nature. For instance, without the principia, measurement made with the Atwood Machine would have meant nothing at all. Experiments and observations articulate the paradigm by resolving some of its residual ambiguities and by permitting the solution of problems to which the paradigm had previously only drawn attention.

The following example will tell us how the factual determination does also depend on paradigm. If we ask an eminent physicist and a chemist whether a single atom of helium is or is not molecule, both would answer without hesitation, but their answers would not be the same. For the chemist the atom of helium was a molecule because it behaves like one with respect to the kinetic theory of gas. For the physicist, on the other hand, the helium atom was not a molecule because it displays no molecular spectrum. Presumably, both men are talking of the same particle, but they are viewing it through their own research training and practice. Their experiences in problem-solving told them what a molecule must be. Undoubtedly their experiences had had much in common, but they do not, in this case, tell the two specialists the same thing. Their answers were different because their paradigms were different.

Experiments, which are another most important work in normal science, are no more than the alternative ways of applying paradigm to the new area of interest. For example, the experiments of heating and cooling, by mixtures and by change of state of matter, were no more than the paradigm application of the caloric theory of heat. Heat could be released and absorbed in many other ways, so many experiments could be undertaken to elaborate these various possibilities, but all these experiments would arise from the caloric theory as paradigm. Thus, once the phenomenon of heating by
compression was established, all further experiments in the area became paradigm-dependent in this way. Often a paradigm, developed for one set of phenomena, is ambiguous in its applications. Then experiments are necessary to articulate the paradigm, but those experiments are, again, oriented by paradigm. There are no paradigm-independent experiments either.

Kuhn says that a part of normal theoretical work consists simply in the use of existing paradigm to predict factual information of intrinsic value. The manufacture of astronomical ephemeredes and the computation of lens characteristics, are examples of this sort. These are manipulations of theory whose purpose is to display a new application by reformulation of paradigm. The principia retained, as a first venture, some of the clumsiness because so much of its meaning was only implicit in its application. So, from Euler and Lagrange in the eighteenth century to Hamilton, Jacobi and Hertz in the nineteenth and many European brilliants repeatedly endeavored to reformulate mechanical theory in an equivalent but logically and aesthetically more satisfying form.

Thus, whatever works are done by normal science are but the articulation of the paradigm. Kuhn says that normal science does not discover the new facts, but rather it discovers those facts that fit the paradigm. Any fact, which is not to articulate paradigm, does not get significance and it remains a mere fact. But the fact that is related to the paradigm is one that scientists expect, and therefore becomes an important one. For example, in the nineteenth century little attention was paid to the experiment that measured electrical attraction with the device like the pan balance. They yielded neither consistency nor simple result because they could not be used to articulate the paradigm from which they are derived. Therefore they remained mere facts, unrelated and unreliable to the continuing progress of electrical research.

Normal science is puzzle-solving activities and it is entirely determined by paradigm. For, what is puzzle and when it is solved – both are determined by paradigm. Kuhn says that puzzle means the special category of problems that can serve to test ingenuity or skill in a solution. Dictionary illustrations are ‘jigsaw puzzle’ and ‘Cross word puzzle’. These words characteristically resemble the problems of normal science. There is no question of criterion of
goodness in a puzzle, that its outcome should be intrinsically interesting or important. On the contrary, the really pressing problems, such as, a cure for cancer, or the design of a lasting peace, are often not puzzle at all, because they may not have any solution. Consider the Jigsaw puzzle whose pieces are selected at random from each of two different puzzle boxes. Since that problem is likely to defy even the most ingenious of men; it cannot serve as a test of skill in solution. In any usual sense it is not a puzzle at all. He says that not the intrinsic value but the assured existence of a solution is criterion for a puzzle. So, when a paradigm, as a criterion for choosing problems, is taken for granted, we assume that the problems have some solution. If it is to classify as a puzzle, a problem must be characterized by assumed solution. There must also be rules that limit both the nature of acceptable solutions and the steps by which they are to be obtained. To solve a jigsaw puzzle is not, for example, merely to make a picture. Either a child or a contemporary artist could do that by scattering selected pieces upon the neutral ground. The picture thus produced might be far better, and certainly be more original, than the one from which the puzzle had been made. Nevertheless such a picture would not be a solution. To achieve that all the pieces must be used, their planeside must be turned down, they must be interlocked until no hole remains. These are among the rules that govern the jigsaw puzzle solution. Similar restrictions upon the crossword puzzle, riddles, chess problems, are readily discovered. Thus ‘paradigm is taken as guarantying the existence of a solution to every puzzle generated by apparent discrepancies between it and observations. Similarly normal science, like puzzle-solving activity, is a highly determined activity.

**Paradigm is Responsible for Novelty**

We have seen so far the characteristics of science are determined by paradigm. Now we will see that it is paradigm which is responsible for novelty too. Normal science does two things. Firstly, normal science, as a puzzle solving activity, is highly cumulative enterprise, and, therefore, aims at the steady extension of the scope and precision of scientific knowledge. But normal science does not aim at novelty of facts or theories. Even when it is highly successful, it finds none. Secondly, there comes some situation when
normal science, due to being oriented by paradigm encounters some phenomena which paradigm can not assimilate. History of science suggests that this sort of new and suspected phenomena are repeatedly uncovered by scientific research, and radical new theories are again and again been invented by scientists to assimilate them. The scientific enterprise has developed a uniquely powerful technique for producing such phenomena. Kuhn says that if this characteristic of science is to be reconciled, then research under a paradigm must be a particularly effective way of inducing paradigm change. The crisis originates because normal science ultimately leads only to the recognition of such anomalies. Anomalies produced inadvertently by a game played under one set of rules, requires the elaboration of other set for their assimilation. After they have become parts of science, the enterprise in which novelties are assimilated is never quite the same game.

Anomalous facts, after being assimilated, become expected by the paradigm and become new facts. Assimilation of anomalous facts demands a complete adjustment with theory. Until the scientists have learnt to see nature in a different way, the new fact is not quite a scientific fact at all. Factual novelty is not independent of theoretical novelty; new facts emerge with new theory. To see how factual novelty follows from theoretical novelty in scientific discovery, we examine, following Kuhn, a particular famous example, the discovery of oxygen.

In his famous book *The Structure of Scientific Revolution*, Thomas Kuhn writes that at least three different men have a legitimate claim to the discovery of oxygen. Several other chemists also must, in the early 1770's, have had enriched air in a laboratory vessel without knowing it. However, the earlier of the claimants to prepare a relatively pure sample of the gas was the Swedish apothecary, C. W. Scheele. We may, however, ignore his work since it was not published until oxygen’s discovery had repeatedly been announced elsewhere and thus had no effect upon the historical pattern. The second in time to establish a claim was the British scientist and divine, Joseph Priestly. He collected the gas by releasing it from heated red oxide of mercury as one item in a prolonged narrow investigation of the ‘airs’ evolved by a large number of solid substances. In 1774 he identified the gas thus produced as
nitrous oxide, and in 1775, led by further tests, he declares the same as common air with less than its usual quantity of phlogiston. The third claimant was Lavoisier. He started the work that led him to oxygen after Priestley’s experiments of 1774 and possibly as the result of a hint from Priestly. Early in 1775 Lavoisier reported that he obtained ‘air itself entire’ by heating the red oxide of mercury — it emanates purer and more respirable. By 1777, probably with the assistance of a second hint from Priestly, Lavoisier had concluded that the gas was a distinct species, one of the two main constituents of the atmosphere. This is a conclusion that Priestly was never able to accept.

Now the question is who first discovered oxygen. In any case, when was oxygen discovered? In that form, question could be asked even if only one claimant existed. Regarding priority and date, an answer does not at all concern us. Nevertheless, an attempt to produce one will illuminate the nature of discovery, because there is no answer of the kind that is sought. Kuhn says that discovery is not the sort of process about which the question can be asked. Priestley’s claim to discovery of oxygen is based upon his priority in isolating a gas that was later recognized as a distinct species. But Priestley’s sample was not pure, and, if holding impure oxygen in one’s hands is to discover it, then that had been done by every one who ever bottled atmospheric air. Besides, if Priestly is the discoverer, when was the discovery made? In 1774 he thought he had obtained nitrous oxide, a species he already knew; in 1775 he saw the gas as dephlogisticated air. Lavoisier’s claim may be stronger, but it presents the same problem.

We cannot award the discovery to Lavoisier for the work of 1775, which led him to identify the gas, as the ‘air itself entire’. Presumably we wait for the work of 1776 and 1777, which led Lavoisier to see not merely the gas but what the gas was. Yet even this award could be questioned, for, in 1777 and to the end of his life Lavoisier insisted that oxygen was an atomic ‘principle of acidity’, and that oxygen gas was formed only when that ‘principle’ did interact with caloric. Shall we, therefore; say that oxygen had not yet been discovered in 1777? Some may be tempted to do so. But the principle of acidity was not banished from chemistry until after 1801.
caloric linger until the 1860's. Oxygen had become a standard chemical substance before either of those dates.

Thomas Kuhn says that clearly we need a new vocabulary and new concepts for analyzing events like the discovery of oxygen. The sentence 'oxygen is discovered' is undoubtedly correct but it misleads by suggesting that discovering something is a single simple act assimilable to our usual concept of seeing. Ignoring Scheele, we can safely say that oxygen had not been discovered before 1774, and we would probably also say that it had been discovered by 1777 or shortly thereafter. Any attempt to date the discovery must inevitably be arbitrary because discovery as a sort of phenomenon is necessarily a complex event, one which involves recognizing both that something is and what it is. If oxygen were dephlogisticated air then we should insist without hesitation that Priestly had discovered it.

However, what Lavoisier announced in his paper from 1777 on, was not so much the discovery of oxygen as the oxygen theory of combustion. That theory was the keystone for a reformulation of chemistry so vast that it is usually called the chemical revolution. What the work on oxygen did was to give new form and structure to Lavoisier's earlier understanding. It told him a thing he was already prepared to discover the nature of the substance that combustion removes from the atmosphere. That advance awareness of difficulties must be a significant part of what enabled Lavoisier to see in experiments like Priestley's a gas that Priestly had been unable to see there himself. Conversely the fact that a major paradigm revision was needed to see what Lavoisier saw must be the principal reason why Priestly was, to the end of his long life, unable to see it. Therefore, it is paradigm, which enables the scientists to see what something is. Rorty says, normal science is closed, rational, but revolutionary science is the introduction of new problems and explanations. For Kuhn, observation and conceptualization, fact and assimilation of it by theory are insuperably linked in discovery. Only when all relevant conceptual categories are prepared in advance, can discovering that and discovering what occur. This conceptual assimilation, we can say, involves a change in paradigm.

The discovery of the Leyden Jar displays all these features as well as the others we have observed before. When it began, there was no single
paradigm for electrical research. Instead a number of theories were in
competition. None of them succeeded in ordering the whole variety of
electrical phenomena very well. That feature is the source of the anomalies.
And this provides background for the discovery of Leyden Jar. One of the
competing schools of electricians took electricity to be a fluid. And that
competition led a number of scientists to attempt bottling the fluid by holding
the water-filled glass vial in their hands, touching the water to a conductor
suspended from an active electrostate generator. On removing the Jar from the
machine and touching the water with his free hand, each of these
investigations they experienced a severe shock. Those experiments did not
provide electricians with the Leyden Jar. That device emerged more slowly
and it is again impossible to say just when its discovery is completed. The
Leyden Jar emerged only after the experiments that necessitated the drastic
revision of the fluid theory and thus provided the first full paradigm for
electricity.

In science as in playing card experiment, novelty emerges only with
difficulty—manifested by resistance—against a background provided by
expectation e.g. by paradigm. Initially, only the anticipated and usual are
experienced even under circumstances where anomaly is later to be observed.
But further acquaintance does result in awareness of something wrong and
does relate the effect to something that has gone wrong before. That
awareness of anomaly opens a period in which conceptual categories, e.g.
paradigm, are adjusted until the initially anomalous has become the
anticipated. At this point the discovery has been completed.\textsuperscript{20}

4.2 Paradigm-Shift versus Scientific Progress

\textit{Proliferation of Paradigms}

Normal science as rule governed enterprise makes progress by
articulating the paradigm. But there comes some such situation that any
articulation of paradigm does fail to assimilate certain anomalous phenomena.
They are neither anticipated nor the paradigm can provide such rules to
absorb them. Kuhn says that awareness of such anomalies is a necessary
precondition for the emergence of a new paradigm. Galileo's contributions to
the study of motion depended closely upon difficulties discovered in Aristotle's theory by scholastic critics. Thermodynamics was born from the collision of two existing nineteenth century physical theories. Quantum mechanics was born from a variety of difficulties surrounding blackbody radiation, specific heat, and the photoelectrical effect. In all these cases, the awareness of anomaly had lasted so long and penetrated so deep that one can appropriately describe the field affected by it as a growing crisis.21

But mere awareness of anomaly is not sufficient for the emergence of the new one, because there may be residual expectation that the theory will solve all the problems in future. If situation gets more difficult which demands a large-scale paradigm-destruction and major shift in the problems and techniques of normal science, then the theory is gradually preceded by a period of profound professional insecurity. That insecurity generates the persistent failure. And this failure of existing paradigm is the next step to the awareness of anomaly, which is the prelude to a search for new one.

In this regard, Kuhn mentions a famous case of the 'awareness of anomaly' and the 'failure of the paradigm', in the emergence of Copernican astronomy. When its predecessor, the Ptolemaic system, was first developed, it was admirably successful in predicting the changing position of both stars and planets. No other ancient system had performed so well. For the stars, Ptolemaic astronomy is still widely used as an engineering approximation. For the planets, Ptolemaic predictions were as good as Copernican. But for a scientific theory, to be admirably successful, is never to be completely successful. With respect both to planetary position and to precision of the equinox, predictions made with Ptolemaic system never quite conformed to the best available observations. These discrepancies constituted many principal problems for many of Ptolemy’s successors. Then they sought to solve the anomaly by adjusting their paradigm and they, given a particular discrepancy, were invariably able to eliminate it. But as time went on, astronomy's complexity was increasing far more rapidly than its accuracy and correcting of the discrepancies. By the early sixteenth century an increasing number of Europe’s best astronomers recognized that the astronomical paradigm was failing in applications to its own traditional problems.
However, that recognition of failure was prerequisite to Copernicus’ rejection of Ptolemaic paradigm and his search for a new one. Though there are some historical elements, such as, the social pressure for calendar reform, the medieval criticism of Aristotle, the rise of renaissance, neoplatonism and other external elements significant in determining the timing of breakdown, the failure of Ptolemy’s system remained as the core of the crisis. Confronted with anomaly or with crisis scientists take a different attitude toward the existing paradigm. The willingness to try any thing, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals—all these occur in this situation and nature of there research changes accordingly.

However, such a failure in the normal problem-solving activity culminates into proliferation of the theories to assimilate the anomalous phenomena. Kuhn says that the rules of normal science of the dominant paradigm break down, and through this proliferation of different articulations of different competing theories, the rules of normal science become increasingly blurred. In this situation, no paradigm can dominate over the research field, and every paradigm seeks to dominate. Copernicus complains that in his day astronomers were so inconsistent in their investigations that they may be compared to an artist who gathers the hands, feet, head, and other parts for his image from diverse models, where each part is exactly drawn, but not related to a single body, since they in no way match each other—resulting in the monster of a man. The situation usually becomes so intolerable that scientists sometimes desert their field of operation. Kuhn calls this stage the ‘period of crisis’. Proliferation is the sole character of this stage. On the way to the emergence of a new paradigm, this proliferation is an inevitable stage. By the time Lavoisier began his experiments on airs in the early 1770’s, there were almost as many versions of the phlogiston theory as there were pneumatic chemists.

Kuhn says that the emergence of the relativity theory was also preceded by such a crisis and proliferation. In the late seventeenth century a number of natural philosophers criticized Newton’s version of absolute space. They were very nearly able to show that absolute positions and absolute
motions were without function at all in Newton's system. And they did succeed in hitting the classical concept by their fully relativistic conception of space and motion. But, for him, their critique was purely logical and aesthetical—at no point did they relate their views to any problems that arose when applying Newton's theory to nature. As a result, their views died with them. Relativistic philosophy of space began to be related with the acceptance of the wave theory of light after about 1815. If light is wave motion propagated in mechanical ether governed by Newton's laws, both celestial observation and terrestrial experiment become potentially capable of detecting drift through the ether. But numerous articulations of the ether theory to absorb drift, failed. The situation changed again only with the gradual acceptance of Maxwell's electromagnetic theory. Despite its Newtonian origin, it ultimately produced a crisis for the paradigm from which it did spring up. Einstein's special theory of relativity emerged in 1905 only after a whole services of earlier observations designed to detect drift through the ether turned out to be anomalous.  

Thus proliferation is the usual symptom of crisis, which results from the persistent failure of the existing paradigm. This crisis is the necessary precondition of the emergence of the new one. Further developments depend on the response of scientists to the crisis. History of science has traced out that scientists even when confronted by severe and prolonged anomalies leading to loss of faith and consideration of alternatives, do not renounce the paradigm—one that has led them into crisis. Scientists do not reject a paradigm just by being confronted with anomalies or counter-instances, because anomalies are counter-instances only to a prevalent epistemological theory. Scientists do not falsify the paradigm only because that there are counter instances against the theory. Kuhn cites two reasons for such a situation. Firstly, there are two reasons for that; one, there is no such thing as research without counter instance, which differentiates normal science from science in a state of crisis. Counter-instances arise because no paradigm for scientific research ever completely resolves all its problems. Secondly, every problem—that normal science sees as a puzzle—can be seen from another viewpoint, as counter-instances and as a source of crisis. Copernicus saw as a
counter instances what most of Ptolemy’s other successors had seen as puzzles in the match between observation and theory. Lavoisier saw as counter-instances what Priestly had seen as successfully solved puzzles in the articulation of the phlogiston theory. And Einstein saw as counter-instances what Lorentz, Fitzgerald and others had seen as puzzles in the articulation of Newton’s and Maxwell’s theory. They can at best help to create a crisis or reinforce one that is already very much in existence. By themselves they cannot and will not be the reason to reject the paradigm, because anomalies could be defended. Scientists will devise numerous articulations and ad hoc modifications of their theory in order to eliminate any apparent conflict. A scientific theory is declared invalid only if an alternative candidate is available to its place— the competitor of the old. Newton’s second law faced difficult factual and theoretical research; a large amount of observations went against it; yet it functioned for committed Newtonians, like a purely logical statement, unless the appearance of an alternative paradigm i.e. the special theory of relativity. For Kuhn, it is because there is no such a thing as research in the absence of a paradigm. To reject one paradigm without simultaneously substituting another is to reject science itself.26

According to Kuhn, at embryo level, often a new paradigm emerges before a crisis has developed or been explicitly recognized. A minor breakdown of the paradigm is sufficient to induce in someone a new way of looking at the field. Take, for example, Thomas Young’s first account of the wave theory of light; it appeared at a very early stage of a developing crisis in optics. That time, the crisis was almost unnoticeable. It emerged as an international scientific scandal only after a decade of Young’s account. So in any way there must be a candidate for paradigm for a competition. This competition is a necessary condition for scientific change.

During the period of crisis, just before a scientific revolution. Kuhn says that many of the characteristics of the pre paradigm science again set in. except that the locus of difference is both smaller and more defined. During this period there are always two competing paradigms struggling for the mastery.27
The competition begins with the failure of the paradigm and when prevailing theory itself is under attack. This attack becomes a genuine test for the theory and a negative outcome of a test may be regarded, not as the personal failure of the experimenters but as a failure of the theory. When a paradigm remains dominant, failure is attributed to the experimenters, because dominance of the paradigm ensures that there must have been some solution. But when a paradigm is under attack, failure goes to the paradigm, because that time theory lose that status. In Kuhn’s words, ‘a failure that had previously been personal may then come to seem the failure of the theory under test’. However, for Kuhn, this failure reflects not on the paradigm but on the man—the man committed to the paradigm. Then his colleagues see him as ‘ the carpenter who blames his tools’. In this situation scientists become willing to leave the prevailing paradigm.

Non-existence of Rationality

In the process of paradigm-shift, there occurs a proliferation of them. Now the question is as to how scientists choose one of them. What is the process by which a new candidate for paradigm replaces its predecessors? A new interpretation of nature, whether by a discovery or a theory, first comes in the mind of one or a few individuals. It is they who first learn to see science and the world differently. Kuhn says that their ability to make the transition is facilitated by two circumstances that are not common to most other members of the profession. One, they are the people whose attention has been invariably and intensely concentrated upon a crisis provoking problems. Two, they are men so young or new to the crisis-ridden field where practice has committed them less deeply than most of their contemporaries to the world view and rules determined by the old paradigm. However, other reasons are at work why scientists shift their allegiance.

The question is how are they able to convert the entire profession or the relevant professional subgroup to their way of seeing science and the world? What causes the group to abandon the tradition of normal research in favor of another. Here philosophers will readily try to explain such a shift by recourse to a enquiry about the testing verification, or falsification of established theories. But history of science has a different story to tell.
Feyerabend says that history generally, and the history of revolution in particular, is always richer in content, more varied, more man-sided, more lively, and subtler than even the best philosopher and best methodologist can ever imagine. History is full of accidents, conjectures, and curious juxtapositions of events. It demonstrates to us the complexity of human change and the unpredictable character of ultimate consequences of any given act or decision of men. In this condition it can not be believed that the naïve and simple-minded rules which methodologists take as their guide are capable of accounting for such a maze of interactions.  

How, then, will we justify that which of the competing paradigms would be accepted or rejected. In principle, without applying a standard, justification is impossible. There must be a criterion on the basis of which one can regard a theory better than other, and can reach the decision to accept or reject one of the competing theories.

According to Kuhn, there are no external standards to permit such a judgment. He says, what occurs in the history of science is neither a decline nor a rise by standards, but simply a change demanded by the adoption of a new paradigm. Furthermore, that change has since been reserved and could be worked out again. Einstein succeeded in explaining gravitation as attractions and that explanation has turned science to a set of canons and problems that are more like those of Newton’s predecessors than of his own predecessors.

He says that Newton’s mechanics improves on Aristotle’s and that of Einstein’s improves on Newton’s as instruments for puzzle solving. However, he shows that in their succession there is no coherent direction of ontological development, on the contrary, in some important respects, though by no means in all, Einstein’s general theory of relativity is closer to Aristotle’s than either of them to Newton’s.

The choice between competing paradigms cannot be resolved by any criterion of normal science. Since two scientific schools disagree about what is a problem and what is a solution, they will inevitably talk through each other’s paradigms: each paradigm will be shown to satisfy criteria it dictates for itself and to fall short of a few of those dictated by its opponent. Secondly,
since no paradigm ever solves all the problems it defines, and since no two
paradigms leave all the same problems unsolved, paradigm debates always
involve the question: what is the ‘significant problem’ among others that they
solved?

For Kuhn, the proponents of competing paradigms will often disagree
about the list of problems which any candidate for paradigm must resolve.
Their standards or their definition of science is not the same. Since the old
paradigm is the background of the new, it ordinarily incorporates much of the
vocabulary and apparatus that the traditional paradigm had previously
employed. However, it seldom employs these borrowed elements in quite the
traditional way. Within the new paradigm, old terms, concepts, and
experiments fall into a new relationship one with the other. For example, what
Ptolemy meant by ‘earth’ was fixed- position. His earth, at least, could not be
moved. Correspondingly, innovation of Copernicus was simply to move the
earth. Rather, it was a completely new way of regarding the problems of
physics and astronomy --- one that necessarily changed the meaning of ‘earth’
and ‘motion’. Therefore, Kuhn says that the proponents of the competing
paradigms practice their tradition in different worlds. One contains
constrained bodies that falls slowly, the other pendulum that repeats their
motion repeatedly. In one, solutions are compounds, in the other, mixtures.
One is embedded in a flat, the other in a curved, matrix of space. Practicing in
different worlds, the two groups of scientists see different things when they
look from the same point in the same direction. They see the things in
different relations. That is why a law that cannot even be demonstrated to one
group of scientists, may occasionally seem intuitively obvious to another.34
Thus the puzzles are relativised to a paradigm. What may be a puzzle in one,
may not be so in other. Therefore, one cannot be in a position to say that
theory_1 is better than theory_2 because theory_1 is a better puzzle solver than
theory_2.35

For Kuhn, a research worker, when he is engaged in normal science
where testing are performed, is a solver of puzzles not a tester of paradigm.
Though he may try out a number of alternative approaches—rejecting those
that failed to yield the desired result ---he is not testing the paradigm when he
does so. Instead, he is like a chess player who tries out various alternative moves in search for a solution. But it happens only with the problem stated and the board physically or mentally before him. These trials, whether by the chess players or by the scientists, are trials only of themselves, not of the rules of the game. They are possible only so long as the paradigm itself is taken for granted. Therefore a scientist who performs testing, verification or falsification, is not in a position to test the paradigm. 36

Scientists are not in a position to make verification of paradigms either. Verification is like natural selection; it picks out the most viable among the actual alternatives in a particular historical situation. It makes little sense to suggest that verification is establishing the argument of fact with theory, because all historically significant theories have agreed with fact, but only more or less. There is no more precise answer to the question whether or how well an individual theory fits the facts.

Probability test may be another one, which is supposed to make the test of paradigm. But, for Kuhn it again begs the question. In their most usual form, probabilistic theories all have recourse to one or another of the pure or neutral observational languages. According to probabilistic theory, a theory should pass either of the tests: one, it should pass the test where we compare the given scientific theory with others which might be imagined to fit the same collection of observed data, two, it should pass the test which demands the constructions in imagination of the entire test that the given scientific theory might conceivably be asked to pass. In both cases, scientists have constructed some alternatives to compare with. But since there can be no scientific or empirically neutral system of language or concepts, the proposed construction of alternate tests and theories must proceed from within one or another paradigm based tradition. Thus restricted, it would have no access to all possible theories. As a result probabilistic theories disguise the verification situation as much as they illuminate it.

For paradigm testing, it makes a great deal of sense to ask which of two actual and competing theories fits the facts better. But history of science shows this as vain; neither Priestly nor Lavoisier's theory, for example, agreed precisely with existing observations, yet few contemporaries hesitated
in concluding that Lavoisier’s theory provided the better fit of the two. Again, some process like counting the number of problems solved by each might settle competition more or less routinely. But in fact these conditions are never met completely. The proponents of competing paradigms are always at least slightly at cross-purposes. Neither will grant all non-empirical assumptions that the other needs in order to make its case. Like Proust and Portholes arguing about the composition of chemical compounds, they are bound partly to talk through each other. Though each may hope to convert the other to his way of seeing science and its problems, neither may hope to prove his case. The competition between paradigms is not the sort of battle that can be resolved by proofs.\textsuperscript{37}

If we examine situations where scientists are required to make choice between the handful of paradigms that confront them at any time, we discover that the relevant evidence and appropriate methodological standards fail to pick out one contender as unequivocally superior to its extant rivals. Laudan calls such situations cases of logical indetermination by way of contrasting them with more global or more familiar forms of indetermination. For, although methodological rules and standards do constrain and delimit to some degree a scientist’s choices of options; those rules, and standards are never sufficient to compel or unequivocally warrant the choice of one paradigm over other.\textsuperscript{38}

Of course, it is true to history of science that philosophers offer the methods and principles for scientific research. It is also true to the history that there is not a single rule that remains valid under all circumstances, and not a single agency to which appeal can always be made. For Feyerabend, the result of historical research is that there is not a single rule, however plausible and however firmly grounded in epistemology, which is not violated at some time or other. It becomes evident that such violations are not accidental events; they are not result of insufficient knowledge or of inattention, which might have been avoided. On the contrary, we see that they are necessary for progress. Indeed, one of the most striking features of recent discussion in the history and philosophy of science is the realization that events and developments, such as discovery of atomism in antiquity, the Copernican
evolution, the rise of modern atomism (kinetic theory, dispersion theory, stereochemistry, quantum theory), the gradual emergence of the wave theory of light, occurred because some thinkers, either decided no to be bound by certain obvious methodological rules, or because they unwittingly broke them.39

For Laudan, it would drive us to the conclusion that every scientist has different reasons for his theory preference from those of his co-workers. The view entails, among other things that it is a category-mistake to ask, say, why physicists think Einstein’s theory is better than Newton’s; for, on Kuhn’s view, there must be as many different answers to that question as there are physicists.40 There are no rules, which allow the members of a scientific community objectively to evaluate fundamental scientific theories, therefore we can not objectively say that soccer is a better game than chess or that a strike in soccer is better than a move in chess.41 Normal science is the normal condition of science. Within normal science, the genuine testing of the prevailing theories is rendered, in some mysterious psychological-cum-sociological way, impossible.42 Laudan says that the consequences of hypotheses may be judged when sustained by experiment; but such a judgment is intra-theoretic, inaccessible to proponents of alternative theories.43

Test of a scientific theory can be accomplished if and only if there are at least some terms, that have theory-independent meaning; and a comparison of different scientific theories can be accomplished if and only if there are at least some such terms that have the same meaning in both of those different theories. If there is no such common meaning, the theories are not talking about the same things, and hence cannot be compared with respect to their adequacy. From this point of view, even if the distinction between ‘theoretical’ and ‘observational’ terms is not a sharp one, nevertheless there must be some overlap of meaning between different theories if they are to be comparable.44 Shapere says that no experiment or piece of observational evidence is ‘crucial’ to the falsification of any single scientific statement, because there are always many alternative courses that may be taken in the face of counter evidence.45

Richard Rorty says that holistic theory seems to licence everyone to construct his own little whole—his own little paradigm, his own little
practice, his own little language-game—and then to crawl into it.\(^6\) Dudly Shapere maintains the same view; he says that it is impossible to segregate a component of the meanings of terms occurring in different theories such that those theories will have the same, or overlapping, observational vocabularies; even though the same terms may occur in those different theories, those terms do not have the same meanings, for meaning depends intimately on and varies with theoretical context.\(^7\) Moreover, as a problem changes, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word-game, or metaphysical play. The normal scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.\(^8\)

However, Kuhn denies the thesis that a theory is rejected when it is falsified by a fact. He shows that every theory meets with different anomalies, i.e. facts contradicting it, but is never rejected by scientists unless they find a theory better than the previous one.\(^9\) In science, the testing situation never consists 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.\(^10\) The decision to reject one paradigm is simultaneously the decision to accept another; and the judgment leading to that decision involves the comparison of both paradigms with nature and with each other.\(^11\)

Thus, what can be said is that no standards of rationality operate in any process of justification between the successive theories. History of science is a testimony to the same. Copernicanism made few converts for almost a century after Copernicus’ death; Newton’s work was gradually accepted, particularly on the continent, more than half a century after the principia appeared; Priestly never accepted the oxygen theory; nor Lord Kelvin the electromagnetic theory; and so on. Therefore, Maxplank remarked that a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up which is familiar with the new.\(^12\)

For Feyerabend, standards that have worked for scientific research, work not for their logical force but for their material effect. Just as a well
trained pet will obey his master no matter how great the confusion in which he finds himself, and no matter how urgent the need to adopt a new pattern of behaviour, so, in the very same way a well trained rationalist will obey the mental image of his master, he will conform to the standards of argumentation he has learnt, he will adhere to these standards no matter how great the confusion in which he finds himself, and he will be quite incapable of realizing that what he regards as the 'voice of reason' is but a causal after-effect of the training he has received. He will be quite unable to discover that the appeal to reason to which he succumbs so readily is nothing but a political manoeuvre. Therefore, we conclude that there does not exist the rationality by which scientists would be able to justify which of the paradigms is better, or, how much it fits the nature.

**Paradigm-shift: Replacement without Retention**

Kuhn mentions that in principle, a new phenomenon might emerge without reflecting destructively upon any part of past scientific practice. For example, discovering life in some less well known part of the galaxy would not destruct the existing paradigm that is incompatible with the existence of life on the moon (though today discovering the existence of life on the moon would destruct the existing paradigm). By the same token, a new theory does not have to conflict with any of its predecessors. It might deal exclusively with phenomena not previously known. For example, the quantum theory deals with subatomic phenomena unknown before twentieth century. Or, the new theory might be simply a higher-level theory than those known before. This higher-level theory links together a whole group of lower level theories without substantially changing any. Today, the theory of energy conservation provides just such links between dynamics, chemistry, electricity, optics, thermal theory and so on. Still other compatible relationships between old and new theories can be conceived. If they were scientific, the development would be genuinely cumulative.

But there is increasing reason he says, to wonder whether accumulation can possibly be an image of science. For, after pre-paradigm period the assimilation of all the new theories and of almost all new sorts of phenomena demands the destruction of a prior paradigm. Consequently, there grows a
conflict between competing schools of scientific thought. Cumulative acquisition of novelty is not only rare in fact, but also improbable in principle. For him, only normal research is cumulative. For, the man who is striving to solve a problem defined by existing knowledge and technique, is not, however, just looking around. He knows what he wants to achieve, and he designs his instrument and directs his thought accordingly. But, unanticipated novelty, the new discovery, can emerge only to the extent that his anticipations about nature and his instruments prove wrong. So, there must be a conflict between the paradigms that disclose anomalies.

There are, in principle, only three types of phenomena, in response to which a new theory might be developed. The first consists of phenomena already well explained by existing paradigm, and these explanations seldom provide either motive or point of departure for theory construction. When they do, the theory that results is seldom accepted. A second class of phenomena consists of those whose nature is indicated by existing paradigms, but whose details can be understood only through further theory articulations. These are the phenomena to which scientists direct their research much of the time, but that research aims at the articulation of existing paradigms rather than at the invention of new ones. Kuhn says, that only when these attempts at articulation fail, do scientists encounter the third type of phenomena. These phenomena are recognized anomalies whose characteristic feature is their stubborn refusal to be assimilated to existing paradigm. This type alone gives rise to new theories. This kind of new theory demands the replacement of the old. For, hypothesis which is new gives us the evidence that cannot be obtained in any other way. Therefore, the consistency condition, condition which demands that new hypothesis agrees with accepted theories, cannot be conformed. History of science tells us that Newton’s theory is inconsistent with Galileo’s law of free fall and with Kepler’s law; that geocentric astronomy is inconsistent with the heliocentric one; that wave optics is inconsistent with geometrical optics; and so on.

Now if new theories are called forth to resolve anomalies in the relation of an existing paradigm, then, according to Kuhn, the successful new theory must somewhere permit predictions that are different from those
arrived from its predecessors. And that difference could not occur if the two were logically compatible. So in the process of being assimilated, the second must displace the first. For example, even a theory like energy conservation, which today seems a logical superstructure, did not develop historically without paradigm destruction. Instead, it emerges from a crisis in which an essential ingredient was the incompatibility between Newtonian dynamics and caloric theory of heat. Only after the caloric theory of heat had been rejected, could energy conservation become part of science. So, it is hard to see how new theories could arise without these destructive changes in beliefs about nature.\textsuperscript{57}

Kuhn argues that by nature, the successive paradigms are different from each other, because they tell us different things about the population of the universe and their behavior. For example, they differ about such questions as the existence of subatomic particles, the materiality of light and the conservation of energy. These are the substantive differences between successive paradigms. But paradigm differs in more than substance, for, they are directed not only to nature, but also back upon the science. For, they are the source of the methods, problem-field, and standards of solution. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. Therefore, some old problems may be relegated to another science or declared unscientific. Others that were previously nonexistent or trivial, may, with a new paradigm, become the very archetypes of significant scientific achievements. Moreover, the problems change, so often along with the standards that distinguish a real scientific solution from a mere metaphysical speculation, word-game, or mathematical play. So, the normal scientific research of the successive paradigms is not only incompatible but also often actually incommensurable.\textsuperscript{58}

Paradigms constitute the ways in which scientists of different traditions view the world. They guide scientists differently when they frame their experiments and theories. A paradigm determines what the scientist of a given tradition takes to be the facts, what his problems are, and the standard he requires a theory to meet: and all these will, in general, vary from paradigm to paradigm. For example, since Newtonian physics is based on a paradigm
different from Einstein’s; the later is not, a more general and accurate version of the former; terms such as ‘space’, ‘time’, ‘mass’ and the like have entirely different meaning in the two theories. S. Toulmin argues that men who accept different ideas and paradigms will not even have the same problems; events which are ‘phenomena’ in one man’s eyes will be passed over by the other as ‘perfectly natural’.

For Kuhn, new theory means a transition from the old. He says that the transition is not achieved by an articulation or extension of the old paradigm. Rather it is a reconstruction of the field from new fundamentals. This is a reconstruction that changes some of the field’s most elementary theoretical generalizations as well as many of its methods and applications. During the transition period, there will be large but never complete overlap between the problems that can be solved by the old and by the new paradigm. But there will be a decisive difference in the models of solution when the transition is complete. The profession will have changed its view of the field, its methods and its goal.

According to him, this transition is just like ‘picking up the other end of the stick’. This is a process that involves handling the same bundle of data as before, but placing them in a new system of relations with one another by giving them a different framework’. Or, this is like a visual gestalt; the marks on paper that were first seen as a bird are now seen as an antelope, or, vice versa. However, he says that parallel can be misleading. Scientists do not see some thing as ‘something else’; instead, they simply see it. We have already examined some of the problems created by saying that Priestly saw oxygen as dephlogisticated air. In addition, the scientist does not preserve gestalt’s subject’s freedom to switch back and forth between ways of seeing. Nevertheless the switch of gestalt is useful elementary prototype for what occurs in full-scale paradigm shift.

Kuhn holds that when paradigms change, the world itself changes with them. Led by a new paradigm, scientists adopt new instruments and look in new places. Even more important, during revolution scientists see new and different things when looking with familiar instruments in place they have looked before. He says that, it is rather, as if, the professional community had
been suddenly transported to another planet where familiar objects are seem in different light and are joined by unfamiliar ones as well. Thus after revolution, scientists are responding to different worlds. At this point, the familiar demonstrations of a switch in visual gestalt prove so suggestive. What were ducks; in the scientist’s world before the revolution are rabbits afterwards. The man who first saw the exterior of the box from above, later sees its interior from below. The world is not fixed once and for all by the nature of environment and of science. Rather it is determined jointly by the environment and by the normal scientific tradition. Therefore, when the normal scientific tradition changes, the scientist’s perception of his environment must be reeducated, and he must learn to see a new gestalt. After he has done so, the world of his research will seem here there, incommensurable with the one he had inhabited before. Scientists from different worlds, then, operate at cross-purposes.\textsuperscript{62}

The change of paradigm with the character of this sort is called by Kuhn scientific revolution. For him, essential part of the change can be described by the metaphor of this political term; there is parallelism between political change and scientific change. Political revolutions are inaugurated by the political community. There grows a sense that existing institutions have ceased adequately to meet the problems posed by an environment and, in part, created by the same institutions. In much the same way, scientific revolutions are inaugurated by growing sense that an existing paradigm has ceased to function adequately in the exploration of nature to which that paradigm itself had previously led the way. In both political and scientific development, the sense of malfunction that leads to crisis is prerequisite to revolution.\textsuperscript{63}

However, scientific revolutions, seem revolutionary only to those whose paradigms are affected by them. To outsider they may seem normal parts of the development process. Astronomers, for example, could accept x-rays as mere addition to knowledge, because their paradigms were unaffected by the existence of the new radiation. But for men like Kelvin, Crookes, and Roentgen, whose research dealt with radiation theory with cathode ray tubes, the emergence of x-rays necessarily violated their paradigm.\textsuperscript{64}
Kuhn analyses political revolution and shows that scientific revolution occurs in the same way. He says that political revolutions aim to change political institutions in ways that those institutions themselves prohibit their own applications. Their success therefore necessitates the partial relinquishment of one set of institutions in favor of another, and in the interim, society is not governed by institutions at all. Initially it is crisis that attenuates the roles of political institutions, as we have already seen it attenuates the role of paradigms. An increasing number of individuals become increasingly alienated from political life and behave more and more eccentrically within it. Then many of those individuals commit themselves to some concrete proposals for the reconstruction of society in a new institutional framework. At that point the society is divided into competing camps or parties - one seeks to defend the old institutional constellation, the other seeks to institute some new one.

Once that polarization has occurred, political recourse fails. Because they differ about the institutional matrix within which political change is to be achieved and evaluated, and because they acknowledge no supra-institutional framework for the adjudication of revolutionary difference. Then the parties to revolutionary conflict must finally resort to the techniques of mass persuasion, often including force. Though revolutions have had a vital role in the evolution of political institutions, that role depends upon their being partially extra-political and extra-institutional events.

Kuhn says that the historical study of paradigm change reveals very similar characteristics in the evolution of science. Like the choice between the competing political institutions, the choice between competing paradigms proves to be a choice between incompatible modes of community’s life. Here also the choice is not and cannot be determined merely by the evaluative procedures, and, for, normal science depend in part upon a particular paradigm whereas that paradigm itself is at issue. When paradigms enter into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm’s defense. The resulting circularity does not make the arguments wrong or even ineffectual. The man who premises a paradigm, when arguing in its defense, can nevertheless
provide a clear exhibit of what scientific practice will be like for those who adopt the new view of nature. For Kuhn, that exhibit can be immensely persuasive, and often competitively so. Yet whatever its force is, the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistically compelling for those who refuse to step into the circle. As in political revolution, so in paradigm choice, there is no standard higher than the assent of the relevant community. The community of scientists within which persuasive arguments are effective, becomes quite a social group.  

Aronson says that scientific change is not an objective, continuous, process, but is driven by social and political forces, which make a replacement of the old by an entirely new set of scientific institutions. Thus, scientific progress cannot be characterized as a linear accumulation of knowledge. Kuhn says that periods of revolution overthrow the old paradigm and break the continuity. The new theories are incommensurable with the old ones. 

So, since the new paradigm is entirely conflicting with the old, they cannot both be accepted simultaneously; but rather the new can be accepted only if the old is rejected.

4.3 Paradigm-shift and the Question of Realism

Psychology of Discovery

Construction of a paradigm and replacement of the earlier by the later paradigm are the activities that are directed mostly on the basis of psychological elements effective on scientists’ thought. As a result, both activities remain far from being objective. Moreover, paradigm-shift occurs essentially to meet up the crisis. This is an instrumentalist aim of theory where theory is constructed with the purpose to solve the puzzles. In other words, paradigm does not aim to describe the reality of the world. Given the characteristics of paradigm, scientists do not shift their allegiance from one paradigm to another on the basis of any rationale, such as, testing, verification, falsification etc. In this matter neither proof nor error is at issue. How, then, do they reject the old and accept the new paradigm? Shapere says that ‘no process yet discovered by the historical study of scientific
development at all resembles the methodological stereotype of falsification by direct comparison with nature. That remark does not mean that scientists do not reject scientific theories, or that experience and experiments are not essential to the process in which they do so. But it does mean that the act of judgment that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world.\textsuperscript{68}

Feyerabend says that arguments that are offered by Galileo do not suffice and his utterances are indeed arguments in appearance only. For, Galileo uses propaganda. He uses \textit{psychological} tricks in addition to whatever intellectual reason he has to offer. These tricks are very successful: they led him to victory. His propaganda obscures this fact by insinuating that the new results which emerge are known and conceded by all, and need only be called to our attention to appear as the most obvious expression of the truth. He says that Galileo reminded us that there are situations in which the non-operative character of shared motion is just as evident and as firmly believed as the idea of the operative character of all motions is in other circumstances. The situations are: events in a boat, in a smoothly moving carriage.\textsuperscript{69} The counter-inductive argument, which was proposed by Galileo, is: you cannot discern the whirling of the earth and to rock placed on to the tower, because in common with the rock, you possess from the earth that motion which is required for following the tower; you do not need to move your eyes. Next, if you add to the rock a downward motion which is peculiar to it and not shared by you, and which is mixed with this circular motion, the circular portion of the motion which is common to the stone and the eye continuous to be imperceptible. The straight motion alone is sensible, for, to follow that you must move your eyes downward. This is strong persuasion indeed. Yielding to this persuasion, we now quite automatically start considering the conditions of the two cases and becomes relativist. His is the essence of Galileo's trickery.\textsuperscript{70}

New theory is less competent, so if it is to succeed, the only way is to resort to means other than arguments. It will have to be brought about by irrational means such as propaganda, emotion, ad hoc hypothesis, and appeal to prejudices of all kinds. We need this irrational means in order to uphold
what is nothing but a blind faith until we have found the auxiliary science, the facts, the arguments that turn the faith into sound knowledge.\textsuperscript{71} No discovery comes with full-fledged adequacy at the start. Only in the future research through ad hoc hypotheses, which opens up the possibilities, theory gets maturity. Copernicus who invents a counter induction as opposed to Ptolemy’s becomes successful leading to progress in science, acted simply on faith. And on Galileo provides support to Copernicus by his new dynamics and new instruments, telescope—both of which were again, based on psychological trickery, propaganda and so on. The new dynamics removes the inconsistency between the motion of the earth and the conditions affecting ourselves, and those in the air above us. But the new dynamics, by which he offers support to Copernicus, was not adequate to follow according to scientific method.\textsuperscript{72}

We start with a strong belief that runs counter to contemporary reason and contemporary experience. The belief spreads and finds support in other beliefs which are equally unreasonable. Research now gets deflected in new directions, new kinds of instruments are built, evidence is elated to theories in new ways until there arises an ideology that is rich enough to provide independent arguments for any particular part of it. We can say today that Galileo was on the right track. For, his persistent pursuit of what once seemed to be a silly cosmology has by now created the material needed to defend it. And this is not an exception—it is the normal case: theories become clear and reasonable only after their incoherent parts have been used for a long time. Such unreasonable, nonsensical, unmethodological foreplay thus turns out to be an unavoidable precondition of clarity and of empirical success.\textsuperscript{73}

Kuhn holds that the transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be performed on the basis of method. Any scientist—particularly whose productive career has committed him to the older tradition—put up a life-long resistance to an upcoming paradigm. Like a determination on religious faith, he would not be accused of violating the standards, because there are no such standards to be violated. This is why Laudan says that Priestley’s continued refusal to accept the theory of Lavoisier though unreasonable, was neither illogical nor unscientific.\textsuperscript{74}
Kuhn says that the source of resistance is the assurance that the older paradigm will ultimately solve all its problems. Inevitably, at the time of a revolution, scientists sometimes becomes stubborn and pigheaded. But it is also something more, for, some assurance is what makes normal or puzzle-solving science possible. It is only through normal science professional community exploits the potential scope and precision of the older paradigm; and then isolate the anomalies out of which a new paradigm may emerge. So another pigheadedness towards the anomalies is necessary for the new paradigm to emerge. It is 'belief' on which the second kind of pigheadedness lies—belief that the new will solve all the problems.

He says, when a new candidate for paradigm is first proposed, it has seldom solved more than a few of the problems it confronts; and most of those solutions are still far from perfect. Until Kepler, the Copernican theory scarcely improved upon the prediction of planetary position made by Ptolemy. When Lavoisier saw oxygen as 'the air itself entire' his new theory could cope not at all with the problems presented by the proliferation of new gases, a point that Priestly made with great success in his counter attack. It is only much later, after the new paradigm has been developed, accepted, and exploited that the new arguments are developed. Though even in the area of crisis the new paradigm is little superior to its traditional rival, it is 'belief' — that the new will solve all the problems in future—on which the proponents hold on to the new. Of course, the new handles some problems better and has disclosed some new regularity. But the older paradigm can presumably be articulated to meet these challenges as it has met other before. Both Tycho Brahe’s earth-centered astronomical system and the later versions of the phlogiston theory were responses to challenges posed by a new candidate for paradigm, and both were quite successful.75

In addition, Kuhn holds that the defenders of the old paradigm can almost always point to the problems that its new rival has not solved and that are the strong support for the old. Until the discovery of the composition of the water, the combustion of hydrogen was a strong argument for the phlogiston theory and against Lavoisier’s. After oxygen theory had triumphed it could still not explain the preparation of a combustible gas from carbon.
which is a strong support for phlogiston theory. For Kuhn, even in the area of crisis the balance of arguments and counter-arguments is very close indeed. In addition, outside that area, the balance will often decisively favor the old. In short, if a new candidate for paradigm had to be judged from the start by hardheaded people who examine only relative problem-solving ability, the science would experience very few major revolutions. 

Therefore, we see in the history of science that method cannot guide research and psychological tricks can bring the success of the theory. There are some situations when our most liberal judgments and our most liberal rules would have eliminated an idea which we regard today as essential for science. The ideas survived, but they cannot be said to be in agreement with reason. They survived because prejudices, passion, conceit, errors, sheer pigheadedness, in short all the elements that criticize the context of discovery and are opposed to the dictates of reason were permitted to have their way. To express it differently: Copernicanism and other irrational views exist today only because reason was overruled at some time in their past.

It follows that paradigm debates are not really about relative problem-solving ability. Instead, the issue is which paradigm should, in future, guide research on problems many of which neither competitor can yet claim to resolve completely. So the man who embraces a new paradigm at early stage, must have faith that the paradigm will succeed with many large problems. A decision of that kind can only be made on faith. Kuhn says, for revolution, there must be at least few scientists who feel that the new proposal is on the right track and sometimes only personal and inarticulate aesthetic considerations can do that. Neither Copernicus’ astronomical theory nor DeBroglie’s theory of matter had many other significant grounds of appeal. Even today, Einstein’s general theory attracts men principally on aesthetic ground - an appeal, which few people previously had been able to experience. Arnson says, ‘if truth, logic, and experimentation would not be the primary considerations in a debate over paradigms, then scientific progress would be like the development of taste in an aesthetic community.’

However, this is not to suggest that new paradigm’s triumph ultimately comes through some mystical aesthetics. On the contrary, very few men
desert a tradition for reasons alone. Often those who do, turn out to have been misled. But if a paradigm is ever to triumph it must gain some first supporters—men who will develop it to the point where hardheaded arguments can be produced and multiplied. And even those arguments, when they come are not individually decisive, for, scientists are reasonable men, one or the other argument will ultimately persuade many of them. However, there is no single argument that can persuade them all. Rather than a single group conversion, what occurs is an increasing shift in the distribution of professional allegiance.

Kuhn also says that what intervenes between the first sense of trouble, and the recognition of an available alternative, must be largely unconscious. Among other cases, however, Copernican, Einsteinian, and contemporary nuclear theory can be cited to show that considerable time elapses between the first consciousness of breakdown and the emergence of a new paradigm.

At the start, a few candidates for paradigm may have few supporters. Moreover, on occasions, supporters’ motive may be suspect. Nevertheless, if they are competent, they will improve it, explore its possibilities, and show what it would be like. And as that goes on, if the paradigm is once destined to win its flight, the number and strength of the persuasive arguments in its favor will increase. More scientists will then be converted, and the exploration of the new paradigm will go on. Gradually, the number of experiments, instruments, articles, and books based upon the paradigm will multiply. Still more men, convinced of the new view’s fruitfulness, will adopt the new mode of practicing normal science, until at least only a few elderly hold-outs remain. Moreover, even they, we cannot say, are wrong. Though historian can always find men —Priestley for example — who were unreasonable to resist for as long as they did, he will not find a point at which resistance becomes illogical or unscientific. At the most, he may wish to say that the man who continues to resist after his whole profession has been converted has ipso-facto ceased to be a scientist.

This is why Aronson says, debate over theory choice, cannot be cast in form that fully resembles the logical or mathematical proof. Rather debate is about premises, and its recourse is to persuasion as a prelude to the possibility
of proof. The debate over theory acceptance is ultimately about values and it
cannot be settled either by proof or by experiment. The superiority of one
theory to another cannot be proved in the debate. Instead, each party must try
by persuasion to convert the other.\textsuperscript{83}

This pattern of scientific change follows three things which are part of
the human psychological move:

1. The criteria of choice between theories function not as rules, which
determine choice, but as values, which influence it.\textsuperscript{84}

2. Tests are being conducted all the time, but these tests are of a particular
sort, for, in the final analysis, it is the individual scientist rather than
current theory, which is tested.\textsuperscript{85}

3. Kuhn’s view seems to imply that a theory is not accepted because it is
better than any alternative; on the contrary, it is called ‘better’ because it
is accepted.\textsuperscript{86}

In the final analysis, it is asserted that there is no logic of discovery
that would assure us that our invention does achieve some bits of truth (or,
nearer to truth) than the rival position. But great science and scientists, like
great poets, are often inspired by non-rational intuition.\textsuperscript{87}

\textit{Seeing Something As}

We have seen that all the scientific activities are determined by
paradigm. And we have also seen that paradigm is product of human
intellectual work. Given the facts stated here, the reality, in other words,
‘thing-in-itself’, is beyond the scientific capacity, for, all scientific activities
are closed into the paradigm. We can understand something more about
paradigm when we look at the Nature through some paradigm; we shall
realize that our understanding of the world is paradigm-dependent. There is
no paradigm-free understanding. We do not see a thing as it is, we see what
our paradigm suggests us to see.

Kuhn exemplifies the matter with ‘playing-card experiment’. The
players, until taught by prolonged exposure that universe contains anomalous
cards, saw only the types of cards for which previous experience had
equipped them. Once they are provided the requisite additional categories, they are able to see all anomalous cards in the first inspection. Still other experiments demonstrate that the perceived size and color of experimentally displayed objects also vary with subjects’ previous training and experience. However, scientific experiments are also like ‘playing card experiment’. Paradigm provides the categories for scientists to see the anomalies.

Kuhn mentions the example of the discovery Uranus. This is the example of seeing something as paradigm suggests. Europe’s most eminent observers, astronomers had actually seen the star in the position that we now suppose to must have been occupied by Uranus. One of the best observers in this group had actually seen the star on four successive nights in 1769; he saw it without noting the motion that could have suggested another identification. Herschel first observed the same object twelve years later with much improved telescope of his own manufacture. As a result he was able to notice an apparent dish-size that was at least unusual for a star. Something was awry, and, therefore, he postponed identification pending further scrutiny. That scrutiny disclosed Uranus’s motion among the stars, and Herschel therefore announced that he had seen a new comet. Only several months later, after fruitful attempt to fit the observed motion to a cometary orbit, did Lexell suggest that the orbit was probably planetary. When that suggestion was accepted, there were fewer stars and one more planet in the world of the professional astronomers.

However, it means that a celestial body that had been observed for almost a century was seen differently after 1781. Like anomalous playing card, it could no longer be filled to the perceptual categories (star or comet) provided by the previously prevailing paradigm. This happened also in chemistry. With the change of vision, Priestly had seen dephlogisticated air where others had seen nothing at all. In learning to see oxygen, Lavoisier had to change his view of many other more familiar substances. So Lavoisier saw nature differently. 88

What is discovered with the emergence of paradigm is not the part of the world, but it is an interpretation of observation of the world, which comes with new paradigm. Observations themselves are not fixed once and for all by
nature of the environment and of the perceptual apparatus. According to
Kuhn, what new comes with paradigm is an interpretation of the observations.
On this view, Priestly and Lavoisier both saw oxygen, but they interpreted
their observations differently. Aristotle and Galileo both saw pendulums; but
they differed in their interpretation of what they both had seen. This discovery
is neither wrong nor a mere mistake; this is a shift of scientist’s mind about
fundamental matter. Even individual data are not unequivocally stable. A
pendulum is not a falling stone, nor is oxygen dephlogisticated air.
Consequently, the data that scientists collect from direct observation are
themselves different. More importantly, the process—by which scientists
make transition from constrained fall to the pendulum, or, from
dephlogisticated air to oxygen—is not one. There may be alternative
processes to do the job. However, Kuhn says that the scientist who embraces
a new paradigm is like the man wearing inverting lenses. He confronts the
same constellation of objects, he nevertheless finds them transformed through
and through in many of their details.89

For Feyerbend, if we become clear about the nature of the total
phenomena, we will find that what we achieve by theory is different from
‘thing-in-itself’. There are two things: one, noticing a phenomenon, the other,
expressing it with the help of an appropriate statement. But when saying in a
certain observational situation—‘the moon is following me’ or ‘the stone is
falling straight down’, we may, of course, abstractly subdivide this process
into parts, and we may also try to create a situation where statement and
phenomenon seem to be psychologically apart and waiting to be related. But
under normal circumstances such a division does not occur—statement and
phenomenon are firmly glued together. Sensation and the mental process are
so firmly connected with their reactions that a separation is difficult to
achieve. The sensation and mental process together produce the unit of
phenomenon. Thus produced phenomenon, considering the origin and the
effect of such operations, is called ‘natural interpretation’. It is an idea that is
so closely connected with observations. In the history of thought natural
interpretations have been regarded as a priori presuppositions of science.90

The history of science, after all, does not just consist of facts and
conclusions drawn from facts. It also contains ideas, interpretations of facts,
problems created by conflicting interpretations, mistakes, and so on. On
closer analysis we even find that science knows no 'bare facts' at all but that the facts that enter our knowledge are already viewed in a certain way and are, therefore, essentially ideational. This being the case, the history of science will be as complex, chaotic, full of mistakes, and entertaining as the ideas it contains, and these ideas in turn will be as complex, chaotic, full of mistakes, and entertaining, as are the minds of those who invented them. History also tells us more things. The standards that justify the theory compete just as much as theories compete and we choose the standards most appropriate to historical situation in which the choice occurs. Knowledge so conceived is an ocean of alternatives subdivided by an ocean of standards. It forces our mind to make imaginative choices and thus makes it grow. It makes our mind capable of choosing, imagining, criticizing. Boltzman writes, 'we cannot utter a single statement that would be a pure fact of experience'. For Duhem, ‘primary qualities are only provisional’ and can be subdivided by further research. For Mach, ‘all concepts are theoretical’.

There was no independent evidence in favor of the heliocentric theory. This was, at least initially, a conjecture that had no foundation in empirical facts. The only favorable remark that could be made was that it somewhat simplified calculations by a suitable coordinate transformation. This is a phenomenon well known in mathematical physics. There are many problems which admit of immediate solution, and whose solution is very cumbersome in different coordinates. Such choice and resulting mathematical success does not imply that the coordinate system chosen has any dynamical preference over other coordinate system. After all, the solubility of a problem, depends as much upon the mathematical formalism as upon nature. They exist asymmetric in the formalism, which do not exist in nature. The fact that the problem of positional astronomy can be dealt with in a more simple manner in a coordinate system, in which the Sun is at rest, therefore does not imply that the Sun is actually at rest and that the Earth moves.

It can be argued that a paradigm can touch the actual world if there is within its framework, increase of predictive accuracy, scope, and fertility of theories which ensure progress. This argument is quite similar to the argument that theory touches the reality because it survives. Nevertheless, this 'survival
value’ will not attest to the truth or approach to the truth of hypothesis. For, history of science tells us that ‘even false theory may well have survival value’. Both wave theory of light and corpuscular theory, have been grounds for many technologies. Ptolemy’s theory is still used for many successful predictions.

We also cannot attribute truth to any theory that is accepted after the crisis, for, defeat or success of a theory is not a necessary sign of either falsehood or truth. Success or defeat happens to a theory in some circumstances in the history. In the history of science, we find that ideas are often rejected before they can show their strength. Even in a fair competition, one ideology, partly through accident, partly because greater attention is devoted to it, may assemble success and overtake its rivals. This does not mean that the beaten rivals are without merit and have ceased to be capable of making a contribution to knowledge. It only means that they have temporarily run out of the steam. They may return and defeat their defeaters. The philosophy of atomism is excellent example. It was introduced in antiquity with the purpose of saving microphenomena such as motion. It was overtaken by the dynamically more sophisticated philosophy of the Aristotelians, returned with the scientific revolution was pushed back with the development of continuity theories and returned again late in the nineteenth century, was again restricted by complimentarity etc. Or take the idea of motion of the Earth. It arose in antiquity, was defeated by the powerful arguments of the Aristotelians, regarded as an incredible ridiculous view by Ptolemy, and yet staged a triumphant comeback in the seventeenth century. On the other hand, the rise, success and triumph of a new theory, point of view, or philosophy almost always leads to a considerable decrease of rationality and understanding. When the view is first proposed, it faces a hostile audience. The reasons are often disregarded, or laughed out of court, and unhappiness is the fate of the bold inventors. But if the reasons are understood and accepted, if there is some temporary success, then interest may be aroused and people may devote themselves to the study of the theory. Professional groups form and make the theory sufficiently respectable to be represented in conferences and meetings. Scientists and philosophers from distant fields
trying to show their erudition, drop a hint here and there. This is quite an uninformed desire to be on the right side which is in turn taken as a sign of the great importance of the theory. But this increase in importance unfortunately is not accompanied by better understanding. Quite the contrary, problematic aspects which were originally introduced with the help of arguments, now become basic principles. Doubtful points, having been generally accepted, turn into slogans. Debates with opponents become standardized and also more and more unrealistic; for, the opponents, now seem to raise only quibbles. So, finally we have success but it is the success of a manoeuvre carried out in a void; not the success of a reasoned view overcoming difficulties.  

We simply cannot uncover nature, as it is, in order to compare our theories. Comparison is limited to the features among our theories, certainly not between theories and objective reality. Paradigm may be wrong, but there is no way to correct it; for, to correct something presupposes another paradigm which exists only in so far as that old paradigm is rejected. Therefore, there is acceptance and rejection, but not correction.

Regarding, pure observational language; one has yet to be devised. The ‘duck-rabbit’ shows that two men with the same retinal impressions can see different things; the inverting lenses show that two men with different retinal impressions can see the same thing. Actual language of observation has not ever supplied a generally applicable language of pure percepts. Therefore no language can produce mere natural and objective report on ‘the given’.  

Like a collection of data, in laboratory, scientists create the things. The operations and measurements that a scientist undertakes in the laboratory are not ‘the given’ of experience but rather ‘the collected with difficulty’. There is not what the scientist sees. Rather they are concrete indices to the content of more elementary perceptions. And as such they are selected for the scrutiny, because they promise opportunity for the fruitful elaboration of an accepted paradigm. Therefore, operations and measurements are paradigm-determined. Science does not deal in all possible laboratory manipulations. Instead, it selects those manipulations, which are relevant to the juxtaposition of a paradigm with the immediate experience, which is again determined, by the
paradigm. The measurements to be performed on a pendulum are not the ones relevant to the case of constrained fall. Nor are the operations relevant for the elucidation of oxygen’s properties uniformly the same as those required for investigating the characteristics of dephlogisticated air.\textsuperscript{99}

Since every part of scientific understanding is determined by paradigm, the achievements of scientific research become relative to the paradigm. In Aronson’s language, not only experimental procedures are value-laden, but also standards, rules, values, and commitments—all are value-laden in the sense that they are determined by paradigm. Paradigm determines what the relevant data are, what questions we should ask, what experiments we to perform, how to handle the new data; what counts as evidence for or against a supposition, what counts as good or bad experimental result, and so on.\textsuperscript{100} So, different paradigms make different understandings about the same world. It entails that what we achieve by our scientific equipments is not ‘thing-in-itself’. Rather we see the things as paradigm represents them. In Margolis language: scientific terms are not synonymous with description.\textsuperscript{101} Therefore, in the process of paradigm-change we neither come ‘closer to truth’ nor ‘more accurate description’ of the world. Paradigm solves the puzzles whereas puzzles are artifacts. Therefore, what paradigm does is to supply the tools that construct a solution to the puzzle.

To conclude, we can say that for Kuhn, paradigms are some accepted examples of actual scientific practice. They include law, theory, application and instrumentation. Particular scientific community acknowledges them as supplying the foundation for its further practice. Men whose research is based on a shared paradigm are committed to the same rules and standards for scientific practice. This commitment and the apparent consensus are prerequisite for normal science. Normal science means research firmly based on paradigm. The most important part of the nature of paradigm is its determination of all scientific activities. All activities of normal science are determined by paradigm.

Further, we have discussed whether or not a paradigm-shift results in scientific progress. The relation of successive paradigms is stated as paradigm-shift versus scientific progress. The scientific march is sometimes
caught in a critically anomalous situation leading to the breakdown of the existing paradigm and proliferation of competing but incommensurable paradigms for the resolution of the crisis. There are no transparadigmatic rational standards leading to such a resolution. In view of the same we cannot decide as to which of the competing paradigms is better. Scientific progress can only be ganged if we can somehow deem successive scientific paradigms to be increasingly or exceedingly better than the preceding ones. Since there is no common standard to make a judgment about the merits of the successive paradigms, we just can not assert whether or not paradigm-shift entails progress. Furthermore, a paradigm-shift is a replacement without retention. When paradigms change, the world itself changes with them, for during revolutions scientists see new and different things around them. What were ducks in the scientists' world before the revolution are rabbits afterward?

Thirdly, we have discussed the question of realism: does paradigm-shift entail the gradual attainment of truth? There are reasons to maintain that paradigm-shift does not entail the gradual attainment of truth. The first and foremost reason is that since there is no progress, there is no gradual attainment. Secondly, scientists are not committed to truth in their choice of paradigms. No discovery comes with fully-fledged adequacy at the start. In this condition, persuasion, propaganda, emotion, faith, etc. motivate the scientists to adhere to the new paradigm in the hope that in future it will become dominant. For Kuhn, transfer of allegiance from paradigm to paradigm is a religious conversion experience which cannot be performed on the basis of method. Lastly, since when we see the world through a paradigm, we do not see the world as it is, but rather we see the world as the paradigm suggests. Therefore, 'seeing something as' deflects us from the 'true description' of the world.
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Chapter – 5

The Research Programme and the Pattern of Progress

5.1 Pattern of Progress in Research Programme

Theory as a Research Programme

Lakatos’ research programme view stands on two things: first, the historical facts which Popper fails to accommodate in his view and second, the rationality of science. Regarding the first, he says, history of science tells us that scientists have thick skin. They do not abandon a theory merely because facts contradict it. They invent some rescue hypothesis to explain the anomalies, or if they cannot explain the anomaly, they ignore it, and direct their attention to other problems. Scientists talk about anomalies, recalcitrant instances, but they do not reject the theory merely for that. History of science, of course, has recorded the account of how crucial experiments killed theories, but this is an alleged reason for that, for such accounts are fabricated long after the theory had been abandoned. History is witness that Newtonians were not prepared to reject their theory in the face of recalcitrant evidence—they did not specify, as Popper suggests, under what conditions they would abandon Newtonian theory. Even some Newtonian scientists would have been exactly as nonplussed as are some Marxists. When Newton published his ‘principia’, it could not properly explain even the motion of the moon; in fact, lunar motion refuted Newton. Not only that, the same thing happened to the relativity theory of Einstein; Kaufmann, a distinguished physicist, refuted Einstein’s relativity theory in the very year it was published.¹ Historical evidence can be marshalled to show that scientists do not reject the theory with anomalies instantly.

Regarding the second, Lakatos holds that scientists cannot be irrational about theory choice. We cannot, as opposed to Kuhn, capitulate and agree that a scientific revolution is just a psychological change in commitment. He says blind commitment to a theory is not an intellectual virtue; it is an intellectual crime. There must be the explicit demarcation between science and pseudo-science, scientific progress and intellectual decay. So, there should be an objective standard of honesty.²
However, these two things; historical facts and intellectual demand should be accommodated in our understanding of the scientific change. Only then can we try to grasp how science progresses and what it achieves through this progress. In doing this work, Imre Lakatos, the philosopher of science, proposes a reconstruction of the history of science and claims that unit of scientific consideration is not an isolated theory, but rather a series of theories which he calls the ‘scientific research programme’.

Lakatos says that the typical descriptive unit of great scientific achievement is not an isolated hypothesis, but rather a research programme. Science is not simply trial and error, as Popper holds; a series of conjectures and refutations. For, the proposition ‘all swans are white’ may be falsified by the discovery of one black swan, but such trivial trial and error does not rank as science. Newtonian science could not have been science if it was just a set of four conjectures—three laws of mechanics and the law of gravitation. But rather it has been science because, these four laws constitute only the ‘hard core’ of the Newtonian programme, and there is something more than ‘hard core’, which is called auxiliary hypotheses that make a vast protective belt to keep the hard core protected from refutation. Even more importantly, the research programme also has a heuristics, that is, a powerful problem solving machinery that digests anomalies and turns them into positive evidence. For instance, if any phenomenon say motion of a planet, goes against Newtonian theory, does not occur as it is expected by the Newtonian theory, the Newtonian scientist checks his conjectures concerning atmospheric refraction, propagation of light in magnetic storms, and hundreds of other conjectures which are all part of the Newtonian programme, so that he can explain away the difficulty. Even he may imagine a hitherto unknown planet and calculate its position, mass and velocity in order to explain the anomaly. This is the heuristics power of the programme that is supplied by the auxiliary hypotheses building a protective belt round the hard core.

For this character of the theory, Lakatos claims that Newton’s theory of gravitation, Einstein’s theory of relativity, quantum mechanics, Marxism, Freudianism, all are research programmes. Each of them consists of a characteristic hard core stubbornly defended, a flexible protective belt, and its
elaborate problem-solving machinery. Each of them at any stage of its development has unsolved problems and undersigned anomalies. Lakatos says that all theories are *born refuted and die refuted*. However, they are not equally good; some have more powerful heuristic capacity to explain away the unsolved problems and some have less powerful heuristic capacity. But all research programmes have one common characteristic. They all predict novel facts—facts which had been undreamt of or contradicted by the previous theory. For instance, in 1686 Newton published his theory of gravitation. Newtonian scientists predicted the existence and exact motion of small planets that had never been observed before. Hally, working on Newtonian programme, calculated on the basis of observing a brief stretch of a comet’s path that it returns in seventy-two years’ time. He calculated to the minute when it would be seen again at a well-defined point of the sky. This was incredible. But seventy-two years later, when Newton and Hally were long dead, Hally’s comet returned exactly as Hally predicted. Similarly, Einstein’s programme made the stunning prediction that if one measures the distance between two stars in the night, and if one measures the distance between the two stars during the day, the two measures will be different. This is a novel fact that nobody had thought to make such an observation before Einstein’s programme. These are the discoveries of the novel facts that every research programme does as common character.

Lakatos also says that a research programme that does not discover any novel fact, or stops to discover novel facts, is a degenerating research programme. On the other hand, any research programme that continues to discover novel facts is a progressive research programme. In degenerating research programmes theories are fabricated only in order to accommodate known facts. For instance, Marxism never predicted a stunning novel fact; but rather it has some famous unsuccessful predictions. It predicted the absolute impoverishment of working class. It predicted that the first socialist revolution would take place in the industrially most developed society. It predicted that socialist society would be free of revolutions. It predicted that there would be no conflict of interest between socialist countries. Thus the early predictions of Marxism were bold and stunning but they failed. Marxists
explained all their failures. They explained the rising living standards of the working class by devising a theory of imperialism; they explained why the first socialist revolution occurred in the industrially backward Russia. They explained Berlin 1953, Budapest 1956, and Prague 1968. They explained the Russo-Chinese conflict. But their auxiliary hypotheses explaining away the anomalies were all cooked up after the event to protect Marxian theory from the facts. The Newtonian theory led to novel facts; the Marxian lagged behind the facts and has been running fast to catch up with them. So the Newtonian programme is progressive research programme, whereas the Marxian is degenerating programme.

Therefore, according to Lakatos, the hallmark of empirical progress is not trivial verifications, but heuristic power. So, it is no success for Newtonian theory that stones, when dropped, fall toward the Earth, no matter how often this is repeated. Its success is the discovering of the novel facts. On the other hand, so-called refutations are not the hallmarks of empirical failure, for all programmes grow in a permanent ocean of anomalies. What really counts are dramatic, unexpected, stunning predictions of novel facts. In this nature of the research programme there is the answer to the question as to how scientific revolutions come about. If we have two rival research programmes, where one is progressive and other is degenerating, scientists tend to join the progressive programme. This is the rationale of scientific revolutions. In this regard, Lakatos says that it is matter of intellectual honesty to keep the record of degenerating programme public, but it is not dishonesty to stick to a degenerating programme and try to turn it into a progressive one. This means that a theory does not change instantly. One must treat a budding programme leniently. For, a programme may take decades before they get off the ground and become empirically progressive. There may be criticism, but it cannot result into the rejection instantly. Unless there is no better theory, merely some isolated facts can never make any criticism effective. Programme criticizes another programme. The progressive programme replaces the degenerating programme, neither instantly nor irrationally.¹

Lakatos explains why the change does not occur instantly. According to him, no experimental result can kill a theory in a full blow. For, a research
programme has a heuristic power to solve the problems. The auxiliary theories of the programme makes the protective belt round the programme, and explain away the recalcitrant evidence. A programme can be saved from counter-instances either by some auxiliary hypotheses or by a suitable interpretation of its terms. However, saving a theory may be of two kinds: one is progressive and other is degenerating. If the auxiliary theory does explain the anomalies and at the same time discover some novel facts some of which are corroborated, then this is a progressive problem-shift of the programme. On the other hand, if the auxiliary theory does explain the anomalies and at the same time it explains some known facts rather than discover novel facts, then this is a degenerating problem-shift of the programme.\textsuperscript{5}

Therefore, any scientific theory has to be appraised together with its auxiliary hypotheses, its initial conditions, and even together with its predecessors, so that we may see by what sort of change it was brought about. It means that we appraise not any isolated theory but a series of theories. Let us take a series of theories, $T_1, T_2, T_3, \ldots$ where each subsequent theory results from adding auxiliary clauses to the previous theory in order to accommodate some anomalies. Lakatos calls such a series as theoretically progressive. If each new theory has some excess empirical content, that is some novel hitherto unexpected facts, over its predecessors, and if some of its excess empirical content is also corroborated and leads us to actual discovery, then, for Lakatos, it is called empirically progressive. Finally Lakatos calls a problem-shift progressive if it is both theoretically and empirically progressive, and degenerating if it is not. For him, we accept a problem-shift as scientific only if it is, at least, theoretically progressive; if it is not we reject it as pseudo-science. Progressiveness is measured by the degree to which the series of theories leads us to the discovery of novel facts. We regard a theory in the series ‘falsified’ when it is superseded by a theory with higher corroborated content. This type of falsification thus shifts from the problem of how to appraise ‘theories’ to the problem of how to appraise ‘series of theories’. Not an isolated theory, but only a series of theories can be said to be scientific or unscientific: to apply the term ‘scientific’ to a single theory is a category mistake.\textsuperscript{6}
However, the heuristic power of the programme has two different functions: negative heuristic and positive heuristic. The negative heuristic of a research programme isolates a ‘hard core’ of propositions, which are not exposed to falsification. These propositions are accepted by convention and are deemed irrefutable by those who implement the research programme. The positive heuristic is a strategy for constructing a series of theories in such a manner that shortcomings at any particular stage can be overcome. The positive heuristic is a set of procedural suggestions for dealing with anticipated anomalies. Anticipated anomalies means that anomalies that are anticipated by the programme when it is proposed. That is, when a programme is first produced, it is produced in an ocean of anomalies; some of them are corroborated and some are expected to be so. In other words, the existence of those anomalies is suggested by the programme itself. However as a research programme unfolds, a protective belt of auxiliary hypotheses are created around the hard core of non-falsifiable propositions. So Lakatos’s research programme goes like this:

For example the Newtonian research programme for the calculation of planetary and lunar orbits may be reconstructed as follows:

<table>
<thead>
<tr>
<th>Theory</th>
<th>Auxiliary hypothesis</th>
<th>Result of applying theory</th>
</tr>
</thead>
<tbody>
<tr>
<td>$T_1$</td>
<td>Sun stationary,</td>
<td>Kepler’s laws deduced.</td>
</tr>
<tr>
<td></td>
<td>Sun and planet are</td>
<td>Fit only approximate.</td>
</tr>
<tr>
<td></td>
<td>Point-masses such that $m_\text{s} \gg m_\text{p}$</td>
<td></td>
</tr>
<tr>
<td>$T_2$</td>
<td>Sun and planet move about common centre of gravity.</td>
<td>Improved fit, but motions of Jupiter and Saturn are anomalous.</td>
</tr>
</tbody>
</table>
T₃
Acknowledge perturbations
Seek approximate solutions to 3-body interaction.

Fit further improved.
Anomalous motions of Jupiter and Saturn described by T₃.
Motion of Moon anomalous.

T₄
Correction introduced for Asymmetric mass-distribution

Motion of Moon described with improved accuracy by T₄.
Anomalous motion of Uranus noted as more data becomes available.

T₅
A trans-Uranic planet exists.

Neptune discovered near predicted location.

For Lakatos test of a research programme is directed at the protective belt of auxiliary hypotheses. He emphasizes that a single negative test result does not refute an entire research programme. Given a negative test result, a fruitful strategy may modify the protective belt of auxiliary hypotheses to accommodate the anomaly. Lakatos insisted that there are rules of appraisal of sequences of theories. Some sequences constitute ‘progressive problem shift’ and others constitute ‘degenerating problem shift’.

A sequence of theories T₁, T₂, T₃, ... is progressive if the following conditions are fulfilled:

1. Tₙ accounts for the previous success of Tₙ₋₁;
2. Tₙ has greater empirical content than Tₙ₋₁; and
3. Some of the excess content of Tₙ has been corroborated. Otherwise the problem shift is degenerating.

<table>
<thead>
<tr>
<th>Domain accounted for by Tₙ₋₁ and also by Tₙ</th>
<th>Excess content of Tₙ</th>
</tr>
</thead>
</table>

Map of progressive problem shift

Lakatos’s criterion of incorporation with corroborated excess content. He emphasizes that this criterion is an objective criterion. A research programme receives an affirmative evaluation only so long as it displays the
power to anticipate and accommodate additional data. However, this objective criterion must be applied at a particular time. And a research programme that is judged degenerating at a particular stage of its development might stage a comeback years later.⁹

**Negative and Positive Heuristic**

However, we have shifted our problem from ‘theory’ to a ‘series of theories’ – Research programme. Such series in the growth of science, is characterized by a certain continuity, which connects their members. This continuity evolves from a genuine research programme. This programme consists of methodological rules: Some tell us what path of research to avoid (negative heuristic), and others what paths to pursue (positive heuristic).⁹ Huristic of a particular research programme, for example, Cartesian metaphysics is: the universe is a huge clock work where push is the only cause of motion. This is a powerful heuristic principle. It discouraged work on scientific theories like Newton’s theory of action of distance, which are inconsistent with it. This kind of guidance is negative heuristic. On the other hand, it encouraged work on auxiliary hypotheses, which can save it from apparent counter evidence like Keplerian ellipses. This kind of guidance is positive heuristic.

The negative heuristic of the programme forbids us to direct the modus tollens at this hard core. Instead, we must use our ingenuity to articulate or even invent auxiliary hypotheses, which form a protective belt around this core, and we must redirect the modus tollens to these. It is this protective belt of auxiliary hypotheses which has to bear the brunt of tests and adjusted and readjusted, or even completely replaced, to defend the hardened core. A research programme is successful if all this leads to a progressive problem shift; unsuccessful if it leads to a degenerating problem shift. The classical example of a successful research programme is Newtonian gravitation theory - possibly the most successful research programme ever. When it was first produced, it was submerged in an ocean of anomalies and opposed by observational theories supporting these anomalies. But Newtonian theory turned, with brilliant tenacity and ingenuity, counter instances one after another into corroborating instances. It did it primarily by overthrowing the
original observational theories in the light of which this contrary evidence was established. In this process they themselves produced new counter-examples, which they again resolved. They turned each new difficulty into a new victory of their programme.¹⁰

In Newtonian programme for example, the negative heuristic bids us to divert the modus tollens from Newton’s three laws of dynamics and his law of gravitation. This core is irrefutable by methodological decision of its proponents. Anomalies must lead to changes only in the protective belt of the auxiliary, observational hypotheses and initial conditions. But its each successive link in this exercise predicts some new facts; each step represents an increase in empirical content. Each prediction is in the end verified, though they may have seemed momentarily to be refuted. As against empirical progress, theoretical progress may be verified immediately. We may be frustrated by a long series of ‘refutations’, but a lucky content-increasing auxiliary hypothesis may turn a chain of defeats into a resounding success story, either by revising some false facts, or by adding novel auxiliary hypotheses. We then say that we must require that each step of a research programme be consistently content-increasing. Each step constitutes a consistently progressive theoretical problem shift. All we need, in addition to this, is that every increase in the 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 should produce immediately an observed new fact. The term intermittently gives sufficient rational scope for dogmatic adherence to a programme in the face of prima facie refutations. We maintain that if and when the programme ceases to anticipate novel facts, its hard core might have to be abandoned.

Research programmes, besides their negative heuristic, are also characterized by their positive heuristic. Even the most rapidly and consistently progressive research programmes can digest their counter-evidences only piecemeal. Lakatos pointed out that a programme emerges among the ocean of anomalies and the anomalies are never completely exhausted, though the heuristic effort keeps solving them. But it should not be thought that yet unexplained anomalies are taken in random order, without
any preconceived order. The order is usually decided in the theoretician’s cabinet, independently of the known anomalies. Few theoretical scientists engaged in a research programme pay undue attention to refutation. They have a long-term research policy, which anticipates these refutations. This research policy, or order of research, is set out more or less in detail in the positive heuristic of the research programme. The negative heuristic specifies the hard core of the programme that is refutable by the methodological decision of its proponents; the positive heuristic 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.11

Here, we are to remember that the word ‘heuristic’ and the word ‘methodology’ are not the same thing. Although their function is in general sense the same, but there is some essential difference. The word ‘heuristic’ is also used by Whewell. The meaning of this word is ‘the art of discovery’. This meaning is not far from what we commonly mean by ‘methodology’. The two words run alongside in Lakatos work. Heuristic is a theory of finding out, or, advice on ‘how to solve the problem’. Heuristic is forward-looking advice, telling how to get on with the job. But ‘methodology’ is backward-looking advice—it always suggests to scientist the pre-established rules. But heuristic may invent its own rules for its own purpose to solve the problem. Each research programme has it own forward-looking heuristic. The unspecific maxims about proliferation of programme, modesty and pigheadedness that are seen in the history of science, are a matter of heuristic rather than that of methodology.12

However, the positive heuristic of a programme saves the scientists from becoming confused by the ocean of anomalies. The positive heuristic sets out a programme which lists an ocean of ever more complicated models simulating reality. The scientists’ attention is riveted on building these models. And these models follow the instructions that are laid down in the positive part of his programme. They ignore the actual counter examples, and also the available data. For example, Newton first worked out his programme for a planetary system with a fixed point like Sun and one single point—like
planet. It was in this model that he derived his inverse square law for Kepler's ellipse. But this model was forbidden by Newton's own third law of dynamics. Therefore, the model had to be replaced by one in which both Sun and planet revolve round their common center of gravitation. This change was not motivated by any observation but by a theoretical difficulty in developing the programme. Then he worked out the programme for more planets as if there were only heliocentric but no interplanetary forces. Then he worked out the case where the Sun and planets were not mass-points but mass-balls. Again, for this change he did not need the observation of an anomaly; infinite density was forbidden by a touchstone theory, therefore planets had to be explained. This change involved considerable mathematical difficulties, held up Newton's work and delayed the publication of the *principia* by more than a decade. Having solved this puzzle, he started working on spinning balls and their wobbles. Then he admitted interplanetary forces and started work on 'perturbation'. At this point he started to look more anxiously at the facts, many of them were beautifully explained by this model, many were not. It was then that he started to work on bulging planets, rather than round planets, etc. This is why Newton despises people who stumbled on a first naïve model but did not have the tenacity and ability to develop it into a research programme. He despises people who thought that a first version, which is a mere aside, constituted a discovery. Newton held up publication until his programme had achieved a remarkable progressive shift.¹³

Most, if not all, Newtonian 'puzzles', which lead to a series of new variants of the programme superseding each other, were foreseeable at the time of Newton's first naïve model. No doubt Newton and his colleagues did foresee them. Newton must have been fully aware of the blatant falsity of his first variants of the programme. So there is another thing in the programme, which is called 'model'. A model is a set of initial conditions, which is bound to be replaced during the further development of the programme, and one even knows, more or less, how. From this point of view, there may be many irrelevant refutations of any specific variant of the programme. But the existence of these irrelevant refutations is fully expected and there is positive heuristic as a strategy both for predicting and digesting them.¹⁴ Let us imagine
a situation, which will illuminate the positive heuristic. Firstly, the three consecutive versions T1, T2, T3, predict some new facts successfully but others unsuccessfully, that is, each version is both corroborated and refuted in turn. Finally, H4 is proposed which predicts some novel facts but stands up to the severe tests. Problem-shift of this kind is progressive. This intermittent success is due to the positive heuristic.

Positive heuristic is very flexible guidance. We may formulate the positive heuristic of a research programme as a metaphysical principle. For instance, one may formulate Newton’s programme like this: the planets are essentially gravitating spinning-tops of roughly spherical shape. In research programme, Lakatos says that this idea was never rigidly maintained. The planets are not just gravitational; they have also, for example, electromagnetic characteristics, which may influence their motion. Positive heuristic is thus, in general, more flexible than negative heuristic. Moreover, it occasionally happens that when a research programme gets into a degenerating phase, a little revolution or a creative shift in its positive heuristic may push it forward again. Therefore, the ‘hard core’ is separate from the more flexible, metaphysical principles expressing the positive heuristic.15

This considerations show that the positive heuristic forges ahead with almost complete disregard of refutations. Although one must point out that any verification of the \((n+1)\)th version of the programme is a refutation of the \(n\)th version, some defeats of the subsequent versions are always foreseen. It is the verifications that keep the programme going, recalcitrant instances notwithstanding. We may appraise their heuristic power even after their elimination: how many new facts did they produce, how great was their capacity to explain refutations in the course of their growth, etc.16 We may also appraise them for the stimulus they gave to mathematics. The real difficulties for the theoretical scientists arise rather from the mathematical difficulties of the programme than from anomalies. The greatness of the Newtonian programme comes partly from the development—by Newtonian—classical infinitesimal analysis which was a crucial precondition of its success.17
**Relative Autonomy of Research Programme**

Pattern of scientific progress in the change of research programme is understood on the basis of two main ideas: relative autonomy of research programme and the status of crucial experiment. Both these ideas establish the fact that a research programme does not become rejected with the emergence of a crucial experiment showing the counter-evidence. But rather every research programme is possessed of some degree of autonomy for which a research programme ignores the counter-evidence. However, this basic idea makes scientific progress rational. The relative autonomy of theoretical science is a historical fact. Autonomy of a research programme means that a research programme can continue to exist even in the face of a huge number of anomalies or counter-instances. It alters not for any modus tollens to itself, but for the modus tollens from it to the anomalies. This is called the autonomy of the programme. Lakatos claims that this very phenomenon of the history of science has been accounted for by his research programme. Which problems scientists will work on is determined by the positive heuristic of the programme rather than by psychologically worrying anomalies. The anomalies are listed but saved aside in the hope that they will turn, into corroborations of the programme. Only those scientists have to rivet their attention on anomalies who are either engaged in trial and error exercise or who work in a degenerating phase of a research programme when the positive heuristic runs out of steam. It is quite unlike naïve falsificationism which holds that once a theory is refuted by experiment, it is irrational to develop it further. One has to replace the old refuted theory by a new unfuted one.\(^{18}\)

However, the autonomy comes from the dialectic of positive and negative heuristic in a research programme. Lakatos analyses a few aspects of two spectacularly successful research programmes, where their heuristic power gives them autonomy. One is Prout’s programme based on the idea that ‘all atoms are compounded of hydrogen atom’ and another is Bohr’s programme based on the idea that ‘light-emission is due to electrons jumping from one orbit to another within the atoms’.

Prout, in 1825, claimed that the atomic weights of all pure chemical elements were whole numbers. He knew very well that anomalies abounded.
but said that these arose because chemical substances as they ordinarily occurred were impure. That is, the relevant experimental techniques of the time were unreliable, or, to put it in other terms, the contemporary ‘observational’ theories in the light of which the truth-values of the basic statements of his theory were established were false. The champions of Prout’s theory therefore embarked on a major venture; to overthrow those theories, which supplied the counter-evidences to their thesis. For this, they had to revolutionize the established analytical chemistry of the time and correspondingly revise the experimental techniques with which pure elements were to be separated. Prout’s theory, as a matter of fact, defeated theories previously applied in purification of chemical substances one after another. Even so some chemists became tired of the research programme and gave it up, since the successes were still far from adding up to a final victory. For instance, Stas, being frustrated by some stubborn, recalcitrant instances, concluded in 1860 that Prout’s theory was ‘without foundations’. But others were more encouraged by the progress than discouraged by the lack of complete success. For instance, Marignac immediately retorted that although the experiments of Monsieur Stas are perfectly exact, there is no proof that the differences observed between his results and those required by Prout’s law cannot be explained by the imperfect character of experimental methods. Crookes says that ‘not a few chemists of admitted eminence consider that we have here an expression of the truth, masked by some residual or collateral phenomena which we have not yet succeeded in eliminating. In Crookes view in 1886 some present atomic weights merely represented a mean value. Indeed, Crookes went on to put this idea in a scientific form; he proposed concrete new theories of ‘fractionation’. But his new observational theories turned out to be as false as they were bold. His theories being unable to anticipate any new facts, they were eliminated from the history of science.¹⁹

However, a generation later, it was found that a very basic hidden assumption failed the researchers. The assumption was that: ‘two different pure elements may behave identically in all chemical reactions but can be separated by physical methods’. This idea required a change of the concept of ‘pure element’, which constituted a change of the research programme itself.
This revolutionary, highly creative shift was taken only by Rutherford's school. After many vicissitudes and the most convincing apparent disproofs, the hypothesis was thrown out by Prout. And a century later, this became the cornerstone of modern theories of the structure of atoms. This is the relative autonomy of a research programme, which is performed by the negative and the positive heuristic.

Due to the relative autonomy not only the anomalies but also the inconsistencies are overlooked by the research programme. History of science shows that some of the most important research programmes were grafted on to the older programme with which they were blatantly inconsistent. For example, Copernican astronomy was grafted on to Aristotelian physics, Bohr's programme on to Maxwell's. It does not mean that consistency does not remain a regulative principle and inconsistency does not remain a problem. But it is other way round. The reason is simple. In the view of a research programme, it may be rational to put the inconsistency into some temporary or ad hoc quarantine, and carry on with the positive heuristic of the programme. But the ever more sterile inconsistencies and ever more ad hoc hypotheses is the degenerating phase of the programme and in this condition the programme loses its empirical character. One thing more about inconsistency in the research programme rather than any isolated theory is that such an inconsistency cannot be 'crucial' to the programme. Crucial experiment described by an accepted basic statement that is inconsistent with a theory cannot refute the programme, whereas it can refute an isolated theory. According to methodology of scientific research programme, any accepted basic statement may make a clash, but it alone can never entitle the scientist to reject a theory. Such a clash may present a problem, but can in no circumstances be deemed a 'victory' against, and thereby a rejection of the programme. Progress is marked by instances verifying excess content rather than by falsifying instances. That is, if any programme keeps discovering new facts some of which are corroborated, then the programme is progressing and no counter-evidence can prevent it.

It is heuristics of the programme, which make the 'relative autonomy' possible for the programme. The negative heuristics save the 'hard core' by
producing auxiliary hypotheses, which, on the one hand, explain away the counter-instances, and on the other hand, discover some novel facts some of which are corroborated. The positive heuristic ignores the recalcitrant evidences, puts them aside in the hope that they would be solved in the course of time. Positive heuristic defines problems, outlines the construction of a belt of auxiliary hypotheses, foresees anomalies, and finally turns them victoriously into positive evidences. Scientists list anomalies but as long as their research programme sustains its momentum, they may freely put them aside. In these two ways, a programme maintains autonomy from the crucial experiments or counter instances. Lakatos says that “It is primarily the positive heuristic of his programme, not the anomalies, which dictates the choice of his problems. Only when the driving force of the positive heuristic weakens, may more attention be given to anomalies”.24

In this way a programme obtains a high degree of autonomy from the theoretical science. The relative autonomy of the programme also comes about from the idea that mere falsification must not imply rejection. Mere falsification is to be regarded but not to be acted upon. For, crucial experiments is an honorific title, which may, of course, be conferred on certain anomalies, but only long after the event, only when one programme has been defeated by another one. Thus, nature may shout ‘No’, but human ingenuity may always be able to shout louder. With sufficient resourcefulness and some luck any theory can be defended progressively for a long time, even if it is false. No experiment is crucial at the time, let alone before.25

According to Lakatos, such an autonomy of the programme makes a rational ground to give it some time for progress. This is the intellectual honesty that one should not reject a programme just with appearing a counter instance against it, without giving it the time and chance to show its competence to solve that. A research programme is said to be progressing as long as its theoretical growth anticipates its empirical growth, that is, as long as it keeps predicting novel facts with some success. This condition of the programme is called by Lakatos ‘progressive problem shift’. It is stagnating if its theoretical growth lags behind its empirical growth. that is, as long as it gives only post hoc explanations either of chance discoveries or of facts
anticipated by, a rival programme. This condition is called by him ‘degenerating problem shift’. If a research programme progressively explains more than a rival, it supersedes it, and the rival can be eliminated. The rivalry of two research programmes is, of course, a protracted process during which it is rational to work in either, or in both. Within a research programme a theory can only be eliminated by a better theory - by one, which has excess empirical content over its predecessors, some of which is subsequently confirmed. And for this replacement of theory by a better one, the first theory does not even have to be falsified. Therefore, empirical falsification and actual rejection become independent of each other. For him, before a theory has been modified we can never know in what way it had been refuted, and some of the most interesting modifications are motivated by the positive heuristic of the research programme rather than by anomalies.

Lakatos also says that it is very difficult to decide when a research programme has degenerated hopelessly or when one of the two rival programmes has achieved a decisive advantage over the other. Neither the logician’s proof of inconsistency nor the experimental scientist’s verdict of anomaly can defeat a programme in one blow. One can be wise only after the event. Here modesty plays a greater role than codes. One must realize that one’s opponent, even if lagging badly behind, may still stage a comeback. No advantage for one side can ever be regarded as absolutely conclusive. There is never anything inevitable about its defeat. Thus, pigheadedness, like modesty has more rational scope. Since there is no instant rejection of any programme, it means that every programme maintains an autonomy of its own.

Therefore, rejection of one programme is not just an instant activity. The better programme falsifies the other, but this falsification cannot be decided at some preconceived particular time. This falsification is declared only with hindsight. Since falsification is understood only after the event, there remains the situation for two different programmes to exist together at the same time – not only two; there may be more than two programmes. Krajewski calls this view ‘pluralistic hypothetism’. because there is a competition among different hypotheses. This is unlike ‘dychotomic
hypothesism' where competition occurs only between two hypotheses. Falsification of this kind—that is, falsification among different hypotheses, with hindsight, after existing long together is called by Lakatos 'sophisticated falsification'. 28 'Sophisticated falsification' denies that in the case of scientific theory our decision of theory-choice depends upon the result of experiments. No experiment or experimental report or observational statement etc. alone can lead to falsification. There is no falsification before the emergence of a better theory. After long time competition scientists can understand which theory is better. Thus falsification can be said to have a historical, in other words, time-bound character. The elimination of theory by test is neither sufficient, nor necessary. Progressive problem-shift does not have to be interspersed with 'refutations'. Science can grow without any refutations leading the way. This shows that proliferation of theories is much more important for sophisticated falsification. Proliferation of theories cannot wait until the accepted theories are refuted. The intellectual honesty demands that one should try to look out things from different points of view, to put forward new theories. 29

In accordance with sophisticated falsification, Einstein's theory is not better than Newton's because Newton's theory was refuted but Einstein's was not. There are many known anomalies to Einsteinian theory. Einstein's theory is better than Newton's because it explained everything that Newton's theory had successfully explained and forbade events about which Newton's theory had said nothing but which had been permitted by other well corroborated scientific theories of the day. Moreover, at least, some of the unexpected excess Einsteinian content was in fact corroborated. 30 All this shows the autonomy of the programme. This kind of autonomy of the 'mature science' may only be maintained by such a scientific unit as 'research programme', which consists of 'hard core', auxiliary hypotheses with positive and negative heuristic power. It is not any isolated theory that can maintain this autonomy.

However, the main contribution of 'research programme' to the rational reconstruction of history of science is that it incorporates both rational and irrational moves of scientists' activities. Kuhn maintained the irrational aspect and Popper did the rational aspect, but Lakatos maintains
both by giving rational ground for irrationality of scientists’ activities. For
him, rational reconstruction of science cannot be comprehensive since human
beings are not completely rational animals. And even when they act rationally
they may have a false theory of their own rational actions. What was external
history for the falsificationist, may well be explained as internal history in
terms of a promising research programme. For instance, Plank was discontent
with his own 1900 radiation formula, and regarded it as ‘arbitrary’. For the
falsificationist, the formula was a bold falsifiable hypothesis and Plank’s
dislike of it a non-rational mood, explicable only in terms of psychology.
However in research pogramme view, Plank’s discontent could be explained
rationally (internally): it was rational condemnation of ‘ad hoc’ theory. To
mention yet another example, for falsificationism, irrefutable metaphysics is
external intellectual influence, in our approach here, it is a vital part of the
rational reconstruction of science.\textsuperscript{31}

However, according to autonomy of research programme, we should
be modest in our hopes for our own projects because rival pogramme may
turn out to have the last word. There is place for pigheadedness when one’s
programme is through a bad patch. The mottos are to be proliferation of
theories, leniency in evaluation, and honest ‘score-keeping’ as to which
programme is producing results and meeting new challenges.\textsuperscript{32}

\textbf{Crucial Experiment and Hindsight Appraisal}

The status of crucial experiment determines the process in which
science makes progress. This is most important character of a research
programme that there are no such things as ‘crucial experiment’. There is no
experiment which can instantly overthrow a research programme. The issue
of ‘crucial experiment’ is one, which is claimed to maintain the relation
between the theory constructed by man and the nature considered as reality.
Lakatos says that an experiment is seen as crucial only long after the event.
When a research programme suffers defeat and is superseded by another one,
one may only with long hindsight—call an experiment crucial. Moreover,
scientists, of course, do not always judge heuristic situation correctly. Their
temperament affects their decision. As Lakatos says, a rash scientist may
\textit{claim} that his experiment defeated a programme, and parts of the scientific
community may even, rashly, accept his claim. But if a scientist in the 'defeated' camp puts forward, a few years later, a scientific explanation of the allegedly ‘crucial experiment’ within (or consistent with) the allegedly defeated programme, the honorific title may be withdrawn and the crucial experiment may turn from a defeat into a new victory for the programme. He mentions, for example, that there were many experiments in the eighteenth century, which were, as a matter of historico-sociological fact, widely accepted as crucial evidence against Galileo’s law of free fall, and Newton’s theory of gravitation. In the nineteenth century there were several experiments based on measurements of the velocity of light which disproved the corpuscular theory and which turned out later to be erroneous in the light of relativity theory. These experiments were later deleted from the justificationist’s textbooks as manifestations of shameful shortsightedness or even of envy.\textsuperscript{33}

In the light of these considerations, the idea of instant rationality can be seen to be utopian. Rationality, reason of theory change, works much slower than most people think, and even then, fallibly. Therefore, Lakatos, says that the continuity of science, the tenacity of some theories, the rationality of certain amount of dogmatism, can only be explained if we construe science as a battleground of research programmes. The battleground implies the theoretical pluralism rather than theoretical monism. This conception allows the scientists to adhere to a research programme despite anomalies or inconsistencies. Scientists may introduce or adhere to any programme with its own heuristic power. Therefore, no experiment can monopolise any programme.\textsuperscript{34} If the experiment is not crucial, if degenerating problem-shift is no more sufficient reason to eliminate a programme, if Popperian refutation and Kuhnian crisis are not at work in the rejection of a programme, it leads us to the question, how are research programmes eliminated? According to research programme view, an objective reason is provided by a rival programme, which explains the previous success of its rival and supersedes it by a further display of heuristic power. But the criterion of heuristic power depends on how we construe ‘factual novelty’. Lakatos points out that it is not immediately ascertainable whether a new
theory predicts a novel fact or not; but novelty of a factual proposition can frequently be seen only after a long period has elapsed.

In this regard, some examples are mentioned by Lakatos. Kepler’s ellipses were generally admitted as crucial evidence for Newton and against Descartes only about one hundred years after Newton’s claim. The anomalous behavior of Mercury’s perihelion was known for decades as one of the many yet unsolved difficulties in Newton’s programme; but only long after Einstein’s theory explained it better, a dull anomaly was transformed into a brilliant refutation of Newton’s research programme. Young claimed that his double-slit experiment of 1802 was a crucial experiment between the corpuscular and the wave programmes of optics; but his claim was acknowledged only much later, after Fresnel progressively developed the wave programme further. All this means that the anomaly, which had been known for decades, received the honorific title of refutation; and the experiments received the honorific title of crucial experiment only after a long period of uneven development of the two rival programmes. Thus, ‘crucial’ character of the experiment becomes evident only retrospectively.36

This consideration led to new emphasis on the hindsight element in our appraisal and led to further liberalization of our standards. For Lakatos, a new research programme, which has just entered the competition, may start by explaining ‘old facts’ in a novel way but may take a very long time before it is seen to produce genuine novel facts. He mentions that the kinetic theory of heat seemed to lag behind the result of the phenomenological theory for decades before it finally overtook it with the Einstein-Smoluchowski theory of Brownian motion in 1905. After this, what had previously seemed a speculative reinterpretation of old facts, turned out to be a discovery of novel facts.37

Considering all this, Lakatos holds that we should not reject anything instantly. He says that we must not discard a budding programme simply because it has so far failed to overtake a rival power, simply because it has been refuted by a crucial experiment. We should not abandon it if it constitutes a progressive problem shift. And we certainly regard a newly interpreted fact as a new fact, ignoring the insolent priority claims of amateur
fact collectors. As long as a budding research programme can be rationally reconstructed as a progressive problem-shift, it should be sheltered for a while from a powerful established rival. But it does not mean that crucial experiments have no force to overthrow a research programme and therefore anything goes. For him, when two research programmes compete, they gradually encroach on each other's territory. An experiment is repeatedly performed and as a result, the first is defeated in this battle, while the second wins. But in this case, the war is not over if any research programme is allowed a few such defeats. All it needs for a comeback is to produce a content increasing version and a verification of some of its novel contents. If such a new comeback, after sustained effort, is not forthcoming, the war is lost and the original experiment is seen, with hindsight, to have been crucial. He further describes that it is very difficult, even with such a crucial experiment, to defeat a research programme supported by talented and imaginative scientists. Alternatively, stubborn defenders of the defeated programme may offer ad hoc explanations of the experiments or a shrewd ad hoc reduction of the victorious programme to the defeated one. But he calls such efforts as unscientific.

From these considerations we see that there are two different issues: (1) the methodological appraisal of a research programme, and (2) the decision whether to continue to apply a research programme. With regard to first issue, Lakatos specifies the rules of appraisal for research programme. But this appraisal-verdict is not a static judgment; rather it may change with time. For, any crucial negative evidence may change into positive evidence in the course of long competition. Moreover, a negative experimental finding may come to be regarded as 'crucial' against a programme only in retrospect. With regard to the second issue, it is not the duty of a philosopher of science to recommend research decisions to the scientist. Some scientists may choose to pursue a degenerating research programme in the hope that further work will reestablish the programme as progressive. For Lakatos, it is perfectly rational to play a risky game; what is irrational is to deceive oneself about the risk.
The scientific progress conceived through the characteristics of research programme, such as heuristic, autonomy and leniency towards crucial experiment, occurs not in the way of instant revolution and does not result in a complete rejection of the previous programme. Unlike Kuhn and Popper, in such a pattern of progress, every defeated programme has the possibility to be powerful once again – a research programme change is not a permanent revolution. This kind of process of progress Lakatos calls sophisticated falsification. Here progress is not determined by experiment, but by the heuristic power of the programme. For Agassi, we may stick to the hypothesis in the face of known facts in the hope that the facts will adjust themselves to theory.  

5.2 Question of Realism and Research Programme

**Growth and Truth**

To grasp the issue of ‘truth’ in the ‘growth’ of science, we need to analyze the conception of ‘sophisticated falsification’. For, it is that concept which specifies the pattern of growth in the research programme view. According to ‘sophisticated falsificationism’ we may reject, in the long course of time, the previous theory, but we are never entitled to consider the rejected theory as false theory, for, the methodology of scientific research programme declares the scope that a rejected theory may well again be a champion, and the champion theory may be defeated in the constant competition of different research programmes. Since the rejected is not declared false, the accepted cannot be declared as true in the same token. It is necessary for something to be true to be proven, for mere claim or convention cannot be taken as true. But the research programme view does not provide us with any proven knowledge. ‘Proof’ is a conclusive decision about theory choice, whereas research programme does not give us any such conclusive decision—neither failure nor success is final in our approach.

For centuries knowledge has meant proven knowledge—proven either by the power of intellect or by the evidence of the senses. Wisdom and intellectual integrity demanded that one must desist from unproven utterances and minimize the gap between speculation and establish knowledge. But
proving power of the intellect or the senses was questioned by the replacement of Newtonian theory by Einsteinian one. Lakatos mentions that Newtonian theory gave, in the physical science, a static understanding, but Einstein's theory gave a relative understanding, resulting in the turning of tables, and now very few philosophers or scientists thinking that knowledge is, or can be proven knowledge. Lakatos says that intellectual honesty does not consist in trying to entrench or establish one's position by proving it. Rather intellectual honesty consists in specifying precisely the conditions under which one is willing to give up one's position. Committed Marxists and Freudians by refusing to specify such conditions, showed their intellectual dishonesty. For him, belief may be a regrettable unavoidable biological weakness to be kept under the control of criticism, but commitment is an outright crime. Which of the theories are true, demands a justification of theories. But a research programme does not leave any scope for such justification, for justification of scientific knowledge consists of proven knowledge and a research programme does not construct such knowledge. Popper has scrutinized and said that under very general conditions all theories have zero probability; whatever the evidence, all theories are not only equally improvable but also equally improbable.

Therefore, the attempt for discovering truth shifts from justification to dogmatic falsificationism. According to dogmatic falsificationism, though science cannot prove a theory, it can disprove - it can work out with complete logical certainty repudiation of what is false. For this view, the form of statement of scientific hypotheses is a human device that we hand over to nature for the task of deciding whether any of the contingent lowest-level conclusions are false. Man invents scientific systems and then discovers whether or not it accords with the observed facts. In this way, dogmatic falsificationism rejects the false theories, which indirectly claim to go nearer to truth. But this method has no place in the research programme view. For, it rests on the assumption that is not warranted in our approach of sophisticated falsificationism. The assumption is that any 'hard fact' which is contradicting the theory is entitled to falsify the theory, that is, being contradicted with hard fact the theory is considered as false. But in sophisticated falsification, a
theory may be refuted by the hard fact but may not be rejected; even if it is rejected it is not conclusively declared as false, for the rejected theory may be champion again. A theory’s rejection or acceptance does not depend upon ‘truth’ or ‘falsity’, but it depends on whether it is degenerating or progressive. Again this depends on the negative and positive heuristic offered by how efficaciously the auxiliary hypotheses are constructed. Lakatos by his research programme says that Modus tollens does not go from fact to theory, but from theory to fact. Therefore, there is no question, according to research programme, of a theory being attested by the facts.

Even methodological falsificationism cannot offer us truth by falsification. According to methodological falsification, the basic statement that is taken as established facts to falsify the theory is taken by decision, that is, it is conventionally granted. Therefore, if this empirical basis clashes with theory, the theory may be called ‘falsified’, but it is not falsified in the sense that it is disproved. If a theory is falsified, it may still be true. If we follow up this sort of falsification by the actual elimination of a theory, we may well end up eliminating a true and accepting a false theory. On the other hand, the basic statement itself need normally be interpreted as a separate premise.46

All this tells us that no other method of theory-choice on the basis of ‘truth value’ is warranted by the research programme. Now we will examine the nature of a research programme that will tell us that the consideration of theory-choice does not depend on ‘truth value’. About Lakatos’s programme, Krajewski says ‘permanent revolution of Popper is desirable neither in society nor in science—a period of stability is also necessary. Revolutionary periods are more favorable to theoretical aims, whereas evolutionary periods are more favorable to practical aims of science. Thus Popper and Kuhm are both one-sided; one sees only the truth, another sees the practice (puzzle-solving). But Lakatos advocates a synthesis of both.47 This combination is constructed by research programme, which is committed neither to truth nor to practice—according to research programme commitment is outright crime. Research programme may be powerful by its heuristic capacity, but may not be true. Research programme may be progressive or degenerating, but not true or false.
Fact versus Truth

It is generally believed that nature (facts) attests the theory constructed by human mind whether it is true or false. But in the research programme view, facts do not deserve such a status. For, facts have such status only in the monotheoretic model, but here in our approach there can exist different research programmes together at the same time. Lakatos opts for, rather powerfully advocates a pluralistic model. For Lakatos, in pluralistic model there are two types of theory; the interpretative theory and the explanatory one. The fist provides the hard facts and the second explains them. So here facts have no such independent status. In the monotheoretic model we regard the higher-level theory as an explanatory theory to be judged by the facts delivered from outside. In the case of clash we reject the explanation. In the pluralistic model we may decide, alternatively, to regard the higher-level theory as an interpretative theory to judge the facts derived from outside. In case of clash we reject the facts as monsters.

According to Lakatos, it is not that we propose a theory and nature may shout 'No'; rather we propose a maze of theories, and nature may shout 'Inconsistent'. So, what nature shouts about a theory is not 'truth', but 'consistency'. Now the question is as to which of the mutually inconsistent theories should be eliminated? The sophisticated falsificationist can answer this way: one has to try to replace first one, then the other, then possibly both, and opt for that new set up which provides the biggest increase in corroborated content, which provides most progressive problem-shift. Secondly, does consistency follow truth? Answer to this question, in my opinion is that truth must be consistent, but consistency may not be truth. For, sense of consistency depends on our knowledge; we judge any theory as consistent or inconsistent on the basis of the knowledge of the day. Thus, we may consider any inconsistent theory as consistent due to the lack of the knowledge. The same consideration is applicable to 'truth'. 'Truth' and 'falsity' are considered on the basis of the knowledge of the day.

To understand the nub of this point, Lakatos needs take a close look at the falsifying hypothesis in this way. Let us take the theory T and the refuting hypothesis R. How well corroborated is R? R is the verdict of the
experimental scientists after a rigorous application of the ‘experimental techniques’ of the day—this is R. The experimentalist while testing T, applied T_1 — theories of the day. He interpreted what he saw in the light of T_1. But what if T_1 is false? Why not apply T_2 rather than T_1 and claim that atomic weights according to T_2, for example, must be whole number? Then this will be a hard fact in the light of T_2, and T_1 will be overthrown. So, the clash is not between theories and facts but between two higher level-theories. The clash is then not any more between a logically higher-level theory and the lower level-falsifying hypothesis. The problem should not be put in terms of whether a refutation is real or not. The problem is how to repair an inconsistency between the interpretative theory and the explanatory one. Explanatory theory is what explains some higher phenomena on the basis of known facts; an interpretative theory is what recognizes those facts. Interpretative theory produces facts, explanatory theory solves the problem on the basis of those facts. Any theory is inconsistent if it builds up an explanation on the basis of facts that are not recognized by the interpretative theory. Moreover, a fact can be interpreted in different ways. Which of the interpretations should be applied depends on the particular time when it is applied. Interpretative theory is speculative theory. Our acceptance of any interpretative theory rather than other is not infallible either.

Agassi says that when we decide whether it is the replacement of the interpretative or of the explanatory theory that produces novel facts, we must take a decision about the acceptance or rejection of the basic statement. When we reject, then we have only postponed the decision, not avoided it. We cannot get rid of the problem of the empirical basis, if we want to learn from experience. We cannot avoid the decision about which sort of propositions should be the observational ones and which theoretical ones. We cannot avoid either the decision about the truth-value of some observational propositions. These decisions are vital for the decision whether a problem-shift is empirically progressive or degenerating. Although testing the basic statement has no natural end, we always come to a point when there is no further disagreement. This is the status of a factual statement. So, theory may clash with a factual statement, but it does not necessitate the rejection of
the theory. ‘Theory can only be eliminated by a better theory, that is, by one which has excess empirical content over its predecessors, some of which is subsequently confirmed. And for this replacement of one theory by a better one, the first theory does not even have to be falsified in Popper’s sense of the term. Thus, progress is marked by instances verifying excess content rather than by falsifying instances; empirical falsification is independent of actual rejection.’

Lakatos says that when scientists tend to ignore counterexamples and bypass the emergent problems and apply their theories regardlessly, they are not being irrational in doing so. They frequently and rationally claim that the experimental results are not reliable, or that discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with advance of our understanding. Scientists do continue to use theories in the face of evidence that seems to refute them. Newtonian mechanics is a case in point. Scientists in the nineteenth century recognized that the anomalous motion of mercury counted against the theory. Nevertheless they continued with it. And they were not acting irrationally in so doing; because refutation and rejection are not the same and refutation neither is nor should be followed invariably by rejection. Theories should be allowed to flourish even within an ‘ocean of anomalies’. Such a position tells us that facts discovered by our research programme are not entitled to ensure us truth.

All this means that progressive shift ensures the progress of science, but progress does not mean the ‘approach to reality’. Research programme view tells us that knowledge does grow whatever we think about truth or reality. One can see by direct inspection that knowledge has grown. This is not a lesson to be taught by general philosophy or history but by detailed reading of special sequences of texts. For instance, we know more about polyhedra or atomic weights than we once did. There is no doubt that more knowledge is available to us than was grasped even by the then geniuses. All this only means that knowledge is growing. But it does not mean that knowledge is justified true belief - at least the research programme view does not assert that.
In the final analysis, for Lakatos the fundamental scientific products are not isolated theories, but a series of theories which he calls research programme. For him, through the change of research programme science makes progress. Now what is the pattern of that progress – does it ensure the gradual attainment of truth? For Lakatos scientific progress can not be irrational as Kuhn holds and such progress is not committed to attainment of truth as Popper holds.

These Lakatosian judgements about scientific progress solely depend on the nature and characteristics of research programme. The research programme consists of a hard-core hypothesis and auxiliary ones. The auxiliary theories of the programme makes the protective belt round the hardcore, and explain away the recalcitrant evidence. A research programme can be saved from counter-instances by auxiliary theories. History of science tells us that scientists do not abandon a theory merely because facts contradict it. For Lakatos as long as a programme continues to discover new facts, the programme is considered as progressive. Such a progressive programme is not rejected just because of a contradicting fact. It is rejected if it fails to continue to discover new facts. Then such a degenerating programme is replaced by the progressive one. This is the rational change, for it is rational to adopt a progressive programme in place of the degenerating one. This progressive vs degenerating problem-shift is the rationality of theory choice. Therefore, for Lakatos, scientific change is rational and progressive. Pattern of such progress is characterized chiefly by its ‘relative autonomy’. The crucial experiments may falsify the programme, but cannot make scientists reject it.

Now what is the position of truth in such a pattern of progress? Is a progressive research programme accepted because of its commitment to truth over the degenerating one? For Lakatos, growth of science is directed towards discovering more and more novel facts, and acceptance and rejection of a theory do not depend on the truth value. He says that we may reject the previous theory, but we are never entitled to consider the rejected theory as false. For, the methodology of scientific research programme assures us that a rejected theory may well again be a champion, and a pioneering theory may
be defeated in the constant competition of different research programmes. Since the rejection is declared false, the acceptance cannot be declared as true in the same token. Truth is premised on proof or conclusive evidence about a theory choice, whereas a research programme does not give us any such conclusive evidence or definitive proof. Neither failure nor success is final in change of programmes. Moreover, research programme view asserts that nature (facts) does not attest a theory constructed by humans either as true or false. It is not that we propose a theory and nature may shout 'No'; rather we propose theories (pluralism), and nature may shout 'inconsistent'. So, what nature shouts about theory is not 'truth', but 'consistency'. Truth must be consistent, but consistency may not be truth. For, consistency depends on our knowledge, we judge any theory as consistent or inconsistent on the basis of our research programme to the best of our knowledge and belief.
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Chapter – 6

The Research Tradition and the Character of Scientific Progress

6.1. Character of Progress in Research Tradition

Theory as Research Tradition

Scientific research starts from encountering of a problem and ends with the solution of that problem. Thus Laudan says that scientific activity is problem-solving activity; how science solves the problems. But there are mainly two kinds of problems for science to solve: the empirical problems and the conceptual problems. The empirical problem is the object of solving and the conceptual problem is the way of solving. It does not mean that the empirical problem and conceptual one are entirely two distinct problems; they both are inter-connected—empirical problem is oriented by the conceptual framework, and the conceptual framework is designed with a view to solve the empirical problem.

Now when is a question solved? An empirical problem is solved, for Laudan, when scientists properly no longer regard it as an unanswered question, i.e. when they believe they understand why the situation propounded by the problem is the way it is. It is the theory which provides us with such an understanding. A problem is solved with reference to some theory that purportedly solves the problem in question. So, he says, when we ask whether a problem has been solved, we are really asking whether it stands in a certain relationship to some theory or other. So it is nothing but theory which solves the problem.

But there are two types of theories; the term ‘theory’ refers to two types of things. According to Laudan, we often use the term ‘theory’ to denote a specific set of related doctrines, which can be utilized for making specific experimental predictions and for giving detailed explanation of natural phenomena. Example of this type of theory would include Maxwell’s theory of electromagnetism, the Bohr-Kramers-Slater theory of atomic structure, Einstein’s theory of photoelectric effect, Marx’s labour theory of
value, Weginer’s theory of continental drift, and the Freudian theory of the Oedipal complex. By contrast, the ‘theory’ is also used to refer to much more general, much less easily testable, sets of doctrines or assumptions. For instance, one may speak about theory of evolution or the kinetic theory of gases. In each of these cases, Laudan says that, we are referring not to a single theory, but to a whole spectrum of individual theories. The term ‘evolutionary theory’ for instance, does not refer to any single theory, but to an entire family of doctrines that are historically and conceptually related. These doctrines work from the assumption that organic species have common lines of descent. Similarly, the term ‘atomic theory’ generally refers to a large set of doctrines, all of which are predicated on the assumption that matter is discontinuous. He mentions that a particularly vivid instance of a theory, which includes a wide variety of specific instantiations, is offered by recent ‘quantum theory’. Since 1930, that term has included quantum field theories, group theories, so-called s-matrix theories, and renormalized field theories. That is, it is a grand theory which embodies huge conceptual divergence. So the second type of theory is different not only in generality but also in the modes of appraisal and evaluation. This kind of theory is the primary tool for understanding. Larry Laudan calls this type of theory scientific research tradition. Among these two type of theories the first is specific theory in contrast to research tradition. Research tradition provides the specific theory, and specific theory touches the empirical problem with a view to solving it. A research tradition is the very base of the answer to an empirical question, it is the source of the answer to the question. Therefore, if we ask what kinds of things solve the problems, the answer of this question is no more than the research tradition; for solution is the understanding of why the problem is the way it is, and it is, research tradition which, provides all the components of such an understanding.

To understand anything we need ontology and methodology about that thing; and it is the research tradition, which provides the same. For Laudan, a research tradition provides a set of guidelines for the development of specific theories. Part of those guidelines constitutes an ontology, the types of fundamental entities, which exist in the domain or domains within which the research tradition is embedded. The function of specific theories within the
research tradition is to explain all the empirical problems in the domain by ‘reducing’ them to the ontology of the research tradition. For example, if the research tradition is behaviorism, it tells us that the only legitimate entities, which behavioristic theories can postulate, are directly and publicly observable physical and physiological signs. If the research tradition is that of Cartesian physics, it specifies that only matter and mind exist, and the theories that talk of other types of substances are unacceptable. Moreover, the research tradition outlines the different modes by which these entities can interact. Thus Cartesian particles can only interact by contact, not by action at a distance. Entities within Marxist research tradition can only interact by virtue of the economic forces influencing them. This understanding of ontological level is provided by research tradition.

The research tradition, very often, will also specify certain *modes of procedure* which constitute the legitimate methods of inquiry open to a research within that tradition. These methodological principles will be wide-ranging in scope, addressing themselves to experimental techniques, modes of theoretical testing and evaluation, and the like. For instance, the methodological posture of the scientist in a strict Newtonian research tradition is inevitably inductivist, allowing for the espousal of only those theories, which have been inductively inferred from the data. The mode of procedure outlined for a behavioristic psychologist is what is usually called ‘operationalism’. Put simplistically, a research tradition is thus a set of ontological and methodological do’s and don’ts. To attempt to talk about what is forbidden by the metaphysics and methodology of research tradition is to put oneself outside that tradition and to repudiate it. If, for instance, a Cartesian physicist starts talking about forces acting-at-a-distance, the scientist in question then goes beyond the pale. By breaking with the ontology and methodology of the research tradition within which he has worked, he has violated the strictures of that research tradition and divorced himself from it. Needless to say, for Laudan, that is not necessarily a bad thing. Some of the most important revolutions in scientific thought have come from thinkers who had the ingenuity to break with the research traditions of their day and to inaugurate new views. He also says that one may break with any tradition and
create another, but when he will go to solve any empirical problem, he must be within some or the other research tradition. For his understanding of the question is provided by the tradition—without tradition he cannot have any understanding, good or bad. So, a primary working definition of a research tradition, according to Laudan, could be put as follows: a research tradition is a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain.  

Generally research tradition consists of two components: One, a set of beliefs about what sorts of entities and processes make up the domain of inquiry and two, a set of epistemic and methodological norms about how the domain is to be investigated, how theories are to be tested, how data are to be collected, and the like. Research traditions are not directly testable, both because their ontologies are too general to yield specific predictions and because their methodological components, being rules or norms, are not straightforwardly testable assertions about matters of fact. Research traditions serve several specific functions. Among others: (a) they indicate what assumptions can be regarded as uncontroversial ‘background knowledge’ to all the scientists working in that tradition; (b) they help to identify those portions of a theory that are in trouble and should be modified or amended; (c) they establish rules for the collection of data and for the testing of theories; (d) they pose conceptual problems for any theory in the tradition which violates the ontological and epistemic claims of the parent tradition, etc.  

A specific theory is a kind of bridge between the empirical problems and the research tradition. The specific theory, which gives an explanation of the empirical problem and thus solution to the problem, is provided by research tradition. So we need to see the relation between the specific theories and the tradition in order to appreciate how our understanding originates from tradition and through specific theory goes to empirical level, in the process, solving the problem or the problems under investigation.  

Every research tradition, however, will be associated with a series of specific theories, each of which is designed to particularize and illustrate the ontology of the research tradition. The individual theories will be empirically
testable for they will entail some precise predictions about the objects in the domain. By contrast research traditions are neither explanatory, nor predictive, nor directly testable. Laudan says that their very generality as well as their normative elements, preclude them from leading to detailed accounts of specific natural processes. The whole function of a research tradition is to provide us with the crucial tools we need for solving problems, both empirical and conceptual. A successful research tradition with its corresponding theories leads to the adequate solution of an increasing range of empirical and conceptual problems.\(^6\)

Then he illustrates the relation of research tradition and specific theories, and says that research traditions do not entail their component theories; nor do those theories entail their parent research traditions. A research tradition at best specifies a general ontology for nature, and a general method for solving natural problems within a given natural domain. A theory, on the other hand, articulates a very specific ontology and a number of specific and testable laws about nature. To be told, as the nineteenth century mechanical research tradition tells us, that heat is simply a form of motion, does not deductively lead us to Boltzmann’s version of the kinetic theory of gases or to statistical thermodynamics. Similarly, there is the reverse relation between tradition and theory. For example, given the theory of impact as developed by Huygens, we cannot deduce the basic assumptions of the research tradition within which Huygens worked. It is never possible to deduce the whole of the research tradition from one, or even all, of the theories allied to it.

However, Laudan explains that the entailment relation does not work here for the reason that there are a number of mutually inconsistent theories which can claim allegiance to the same research tradition, and there are a number of different traditions which can, in principle, provide the presuppositional base for any given theory. Laudan mentions at least two specific modes by which theories and research traditions are related: one is historical and the other is conceptual. It is a matter of historical fact that most of the major theories of science have emerged when the scientist who formulated them was working within one or other specific research tradition.
For instance, Boyl’s theory of gases was developed within the framework of the mechanical philosophy, Buffon’s embryological theories were developed as efforts to apply the Newtonian tradition to biological phenomena. Historical research can always identify the research tradition with which a particular theory has been associated. In this sense, Laudan says, the connection between a theory and research tradition is as real as any fact of the past, and it is as important as the most important facts of the past. 7

Though they are two different types of theories, there is an interaction between them. The most important modes of interaction are generally influences of research tradition upon its constituent theories. These influences take a variety of forms:

**Problem Determining Role**: Before the scientific theories are formulated within a tradition, a research tradition will often strongly influence the range and the weighting of the empirical problems with which the specific theories must grapple. Laudan says that either the ontology or the methodology of the research tradition can influence upon the decision as to what are to count as legitimate problems for constituent theories. Research tradition does this work by indicating that it is appropriate to discuss certain classes of empirical problems in the given domain, while others belong to foreign domain, or are pseudo-problems which can be legitimately ignored. He mentions in this regard that the rise of the Cartesian mechanical research tradition in the seventeenth century radically transformed the accepted problem domain for optical theories. It did so by arguing, or rather by simply postulating, that problems of ‘perception and vision’—problems which had classically been regarded as legitimate empirical problems for any optical theory---should be relegated to psychology and to physiology, fields outside the domain of optics, so that such empirical problems could be safely ignored by the mechanistic optical theorist. 8

**Constraining Role**: According to Laudan’s research tradition view, it is the primary function of research tradition to establish a general ontology and methodology for tackling all the problems of a given domain, or set of domains. By this work a tradition acts as a constraint on the theories: as to which of the theories can be developed within the domain. If the ontology of a
research tradition denies the existence of the forces acting-at-a-distance, then it clearly rules out as unacceptable any specific theory which relies on no-contact-action. Any research tradition, which has a strongly inductivist or observationalist methodology, will regard specific theories postulating entities that cannot be observed, as unacceptable. Much of the opposition to subtle fluid theories, atomic theories, etc. was due to the fact that the dominant methodology of the period denied the epistemic and scientific wellfoundedness of theories, which dealt with unobservable entities.  

*Heuristic Role*: The research tradition plays also a heuristic role for the specific theories. Since specific theories postulate certain types of entities and certain methods for investigating the properties of those entities, a research tradition can play a vital heuristic role in the construction of specific scientific theories. Although, specific theories cannot be deduced from research tradition yet a tradition can provide vital clues for theory construction. Laudan mentions that when Descartes attempted to develop a theory of light and colours, he had already defined his general research tradition. His research tradition amounted to the assertion that the only properties that bodies can have are those of size, shape, and motion. He did not specify precisely what sizes, shapes, positions, and motions particular bodies could exhibit. However, he did make it clear that any specific physical theory would have to deal exclusively with these four parameters. As a result, Descartes knew that his optical theories would have to be constructed along such lines. So, he sought to explain colours in terms of the shape and rotational velocity of certain particles. Since his general tradition made it clear that particles of light are exactly like other material bodies, he recognized that he could apply general mechanical theories to a theoretical analysis of light. 

Laudan shows another kind of heuristic role for the research tradition which arises when one of its constituent theories requires modification. He says that any sound tradition will contain significant guidelines about how it can be modified and transformed, so as to improve its problem-solving efficacy. When early version of kinetic theory of gases was confronted by some serious predictive failures, there was enormous flexibility within the tradition that paved the way towards possible modifications. If more degree of
freedom was needed to accommodate seeming energy losses, kineticists could introduce molecular spin or alert their assumptions about molecular elasticities. If gases did not condense in accordance with theoretical predictions, the addition of weak intermolecular attractions could do the job. All these and many similar gambits emerge quite plausibly from regarding matter as possessing a molecular and mechanical composition. By such a heuristic role research tradition tries to solve the anomalies.

**Justificatory Role**: It is one of the important functions of research traditions to rationalize or justify theories. Specific theories make many assumptions about nature, assumptions that are generally not justified either within the theory itself or by the data that confirm the theory. These are usually assumptions about basic causal processes and entities, whose existence and operation the specific theories take as 'given'. Laudan points out that it is the research tradition which rationalizes these assumptions. He says that when, for instance, Sadi Carnot developed his theory of the steam engine, his was a working out of the theory, which presupposed that no heat was lost in performing the work of driving a piston. Carnot offered no rationale for that assumption, and quite rightly felt no need to do that. Caloricist research tradition, within which he was working, laid it down as a primary postulate that heat was always conserved. Carnot was thus able to presuppose, in developing this theory, certain things about nature, which his theory could not itself establish. That is, the caloricist research tradition rationalizes that energy is not lost. These are the functions through which research tradition produces an understanding about scientific problems; this understanding is, in other words, called the solution to the problems.

**Rationale of Theory-choice**

From the above consideration we are now entitled to stipulate that the aim of science is the resolution or clarification of problems. The more crucial implication of this view is that scientific progress does not become parasitic upon 'rationality', which is outside the scientific activity itself. In classical view, rationality has tended to be viewed as an a-temporal concept; the idea was that we can determine whether a statement or theory is rationally credible independently of any knowledge of its historical career. Here the
progressiveness is totally dependent upon the rationality. To be progressive is to adhere to a series of increasing rational beliefs. But a worrying difficulty with it is that whereas progress is readily understood, rationality is far more obscure.

Since science is an aim-oriented enterprise and if its aim is problem solving, then problem-solving effectiveness would be the rationality of theory-choice. This type of rationality is not outside of scientific activity itself; it is immanent rather than transcendental one. If science makes advancement in solving the problems, then this advancement is the rationale of theory-choice. Now progressiveness will determine rationality rather than rationality determining progressiveness. If any theory is effective in solving the problem, then it is rational to accept that theory. Now the question arises as to when is a problem solved? It depends on the research tradition that tells us about the nature and status of the problems, about the rules of appraisal and about other methodological criteria. When a research tradition changes, rationality of theory-choice also changes. This amounts to saying that rational is progressive and progressive is rational. Any theory that is effective in solving the problems, is progressive and thus rationally acceptable. So problem-solving effectiveness is the rationale of theory-choice.

'Problem-solving effectiveness' of a theory is assessed by recording how many problems are solved by it in comparison to its competitors. In this regard, Laudan says that we have three types of problems: solved problems, unsolved ones, and anomalous instances. For him, among them all, it is solved problems that determine whether a theory is more effective than its rival or not. For, 'problem solving effectiveness' of a theory is understood only when a theory solves some problems. He also points out that one should not conclude from this that unsolved problems are unimportant for science. Solved problems are nothing but problems that have been transformed from unsolved to solved ones. But unsolved problems are not like solved ones in showing effectiveness of theory, for 'a theory's failure to solve some unsolved problems generally will not weigh heavily against that theory. For, we usually cannot know a priori that the problem in question should be solved by that sort of theory. Also, the problems matter which are already solved by
other theory. So, in appraising the relative merits of theories, the class of unsolved problems is altogether irrelevant. What matter for purpose of theory evaluation are only those problems that have been solved, not necessarily by theory in question, but some known theory.\textsuperscript{12}

Given that science aims at solving the problems and that solved problems are its career, we can now talk about the model of progress. The core assumptions of such a model, according to Laudan, are simple: (1) the solved problems—empirical or conceptual—is the basic unit of scientific progress; (2) the aim of science is to maximize the scope of solved empirical problems and minimize the scope of anomalous and conceptual problems. The problem solving effectiveness of a theory depends on the balance it strikes between its solved problems and its unsolved ones. This balance can be understood in the light of following: Imagine some domain in which we notice a certain puzzling phenomenon, $P$. The phenomenon $P$ constitutes the unsolved problems for the scientist who wishes to develop a theory, $T_1$, specifically with a view to resolving $P$. Once $T_1$ is announced, several things are likely to happen simultaneously. Some fellow scientists may observe that $T_1$ predicts other phenomena in a domain beside $P$. These predictions will be tested, and very often, some of them will not be borne out in our observation. Thus the observation of these discrepant results will constitute one or more anomalies for $T_1$. At the same time, it may be pointed out that $T_1$ makes certain assumptions about natural processes which run counter to some of our most widely accepted theories, or are incompatible with our methodological norms. This will constitute one or more conceptual problems for theory $T_1$.

Laudan explains that it is true that $T_1$ has solved its initial empirical problems, $P$, and to that extent, we can say that 'progress' has been made. But unfortunately, $T_1$ has generated several others anomalies and conceptual problems. It is entirely possible that more serious problems have been generated than resolved by the invention of $T_1$. Suppose that a second theorist comes along who is convinced that he can improve $T_1$. Such improvement would be exhibited by showing that a new theory, $T_2$, could explain the initial empirical problem of $T_1$ without generating as many anomalies and conceptual problems as $T_1$ has produced. Now we could all agree that it
would be more reasonable to accept \( T_2 \) than to accept \( T_1 \). Therefore, Laudan holds that the acceptance of \( T_2 \) is progressive and the continued espousal of \( T_1 \) is unprogressive or regressive. \(^{13}\)

From this analysis Laudan defines an appraisal measure for a theory in the following way: the over-all problem solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves, and reducing the number and importance of the anomalies and conceptual problems which the theory generates. Problem solving effectiveness may also come about simply by an expansion of the domain of solved empirical problems with all the other vectors remaining fixed. In such a case the replacement of \( T_1 \) by \( T_2 \) is clearly progressive. Effectiveness can also result from a modification of the theory, which eliminates some troublesome anomalies, or which resolves some conceptual problems. If the aim of science is problem solving, progress can occur if and only if the succession of scientific theories registers an increasing degree of problem solving effectiveness. \(^{14}\) This position is like that of Kuhn. He holds that the ability to solve problems is neither the unique nor unequivocal basis for theory (paradigm) choice. \(^{15}\)

Here one thing should be remembered that a specific theory can or cannot be progressive, such theories are designed to explain only a particular empirical problem. As against such theories a research tradition is a productive one. For, only a research tradition can be held accountable with regard to the number of specific theories. So, degree of effectiveness can be a character of such a qualitative theory as research tradition. Therefore, 'problem solving effectiveness' of a theory means 'problem solving effectiveness' of a research tradition. One research tradition (RT) can be replaced by another RT. Thus it is RT which is the unit of scientific change. Important and substantive changes can occur within an on-going research tradition.

In the final analysis, 'problem solving effectiveness' as the rationale for theory choice, in other words, tradition-choice successfully explains the historical fact that reason of theory-choice is relative to milieu. For, research tradition includes the conceptual problems of the day also. Laudan says that
Aristotle was not being irrational when he claimed, in the fourth century B.C., that the science of physics should be subordinate to, and legitimated by, metaphysics. Thomas Aquinas or Robert Grosseteste were not merely stupid or prejudiced when they espoused the view that science must be compatible with religion. The view that science is quasi-independent of non-scientific disciplines is itself a research tradition that generated considerable degree of progress and that is why it may be rational in the twentieth century to accept it. But the fact that a belief is rational in the present age or in any age for that matter, does not necessarily entail that it was rational at other times and places. Quite the reverse is more often the case. No research tradition can claim to be an eternal and universal problem solver.  

However, to make a choice between two theories, they should be comparable. For, rationale of theory-choice can only be applied if theories are comparable. Traditions being the supplier of assumptions at ontological and methodological levels, they cannot have any theory-free observational language; neither can there be any correspondence rules to appraise the tradition for the same reason. Then how do two research traditions become comparable? Since, according to Laudan, aim of scientific theories is problem-solving, theories do not require observational language or correspondence rules—we can talk about different theories, for, all theories aim at the same thing i.e. problem-solving. However, it can genuinely be asked as to how we can show that different theories address the same problem?

Laudan’s answer to this question is straightforward. He says that the terms in which a problem is characterized will generally depend upon the acceptance of range of theoretical assumptions, T₁, T₂, T₃, ...Tₙ. These assumptions may, or may not, constitute the theories, that solve the problem. If a problem can be characterized only within the language and framework of a theory that purports to solve it, then clearly no competing theory could be said to solve the same problem. But, so long as the theoretical assumptions necessary to characterize the problem are different from the theories that attempt to solve it, then it is possible to show that the competing explanatory theories are addressing themselves to the same problem. Laudan considers the
following elementary example: Since antiquity scientists have been concerned to explain why light is reflected off a mirror or other polished surface according to a regular pattern. Relating the incident to the reflected angle, the problem of reflection, thus characterized, involves many quasi-theoretical assumptions, e.g., that light moves in straight lines, that certain obstacle can change the direction of a ray of light, that visible light does not continuously fill every medium, etc. Now the theories that solve the problem are not inconsistent with those relatively low level theoretical assumptions required to state the problem. Throughout the seventeenth century, for example, numerous conflicting theories of light addressed themselves to the problem of reflection. The various optical theories were all regarded as solving the problem of reflection, because that problem could be characterized in a way that was independent of any of the theories, which sought to solve it. It does not suggest that all the problems which a theory or tradition attempts to solve can be characterized independently of the theory which tries to solve them. We want to say just that there are far more problems common to competing research traditions than there are problems unique to single one. These shared problems provide a basis for rational appraisal of the relative problem solving effectiveness of competing research traditions. When early nineteenth century geologists debated the explanation of stratification, they could all—whether uniformitarian or catastrophist, Neptunist or vudeanist, Huttonian or Wernerian, God fearing or God denying, French, English or German—agree that one problem for any geological theory was that of explaining how such uniform and distinct layers had been formed.

Another part of Laudan’s argument is the answer to the question, viz., can two different theories agree to accept some particular problem as a solved one. It may happen that a problem is considered as has been solved by one theory, but not by another. Then how will we measure their relative merits? His answer to this question is that since rationality consists in accepting research traditions which had the highest problem solving effectiveness, an appropriate determination of the effectiveness of a research tradition can be made within the research tradition itself without reference to other tradition. We simply ask whether a research tradition has solved the problems that it
sets for itself; we ask whether it generated any anomaly or conceptual problem in its own process; we ask whether, in the course of time, it has expanded the domain of solved problems and minimized that of anomalies and conceptual problems. Laudan hopes that if we did this for all the major research traditions in science, then we should be able to construct something like a progressive ranking of all research traditions at a given time. For him, it is thus possible at least in principle and perhaps in practice, to be able to compare the progressiveness of different research traditions, even if those traditions are utterly incommensurable in terms of the substantive claims they make about the world. 17

**Continuity and Change**

However, Laudan tries to carry out an analysis of his notion of a research tradition and says that change of research tradition takes two distinct forms. First, the most obvious way in which a research tradition changes is by a modification of some of its subordinate or specific theories. For him, research traditions are continuously undergoing changes of this kind. Researchers often discover that there is, within the framework of the tradition, a more effective theory for dealing with some of the phenomena in the domain than they had realized previously. They think that by slight alternation in previous theories, or modification of boundary conditions, or revision of constants proportionately, or minor refinements of terminology, they will be able to improve on the problem solving success of any of the theories within the research tradition. Whenever they discover a theory, which is a significant improvement on its predecessor, they drop the latter immediately. This is the reason why theories change so rapidly; the history of any flourishing tradition will exhibit a long succession of specific theories.

Second, another important way in which research traditions evolve is a change of some of its most basic core elements. If one looks at the great research traditions in the history of scientific thought—Aristotelianism, Cartesianism, Newtonianism to mention only a few—one can see immediately that there is scarcely any interesting set of doctrines which characterizes any one of these traditions throughout its entire history. Certain Cartesians, at times, repudiate the Cartesian identification of matter and
extension. Huygens admits the possibility of void space, yet he remains Cartesian.\textsuperscript{18} Scheffler puts it in this way: underlying historical changes of theory, there is a constancy of logic and method, which verified each scientific change with that which preceded it and with that which is yet to follow.\textsuperscript{19}

These are the changes within a research tradition, but in course of scientific development research tradition itself changes. As ongoing traditions and their theories encounter a number of problems; anomalies are discovered and basic conceptual problems arise; proponents of the tradition find themselves unable to eliminate these anomalies and conceptual problems by modifying specific theories within the tradition. For Laudan, in such circumstances, it is common for partisans of the tradition to explore what sort of minimal changes can be made in the deep level methodology and ontology of that tradition to eliminate the anomalies and conceptual problems. Sometimes scientists find it impossible to tinker with one or another assumption of the research tradition with a view to eliminate its anomalies and conceptual problems. At this point, scientists may most likely abandon the tradition. However, even at this stage, they can choose to solve the anomalies and conceptual problems by introducing one or two modifications in the core assumptions of a tradition. This way they preserve the bulk of assumptions of a research.\textsuperscript{20}

Laudan further holds that 'Research tradition undergoes a natural evolution—an evolution which represents a change that is far from repudiation of former tradition and the creation of a new one. For him, if the research tradition has undergone numerous evolutions in the course of time, there probably can be many discrepancies between the methodology and ontology of its earliest and its latest formulation. Thus, Newtonian research tradition in Michael Faraday's hands is a far cry from that of Newton's first followers. But he points out that, a finger grained analysis of the historical evolution will show that there was a continuous intellectual descent from Newton to Faraday, but they were as different as their end points look from their beginnings. ' We can see no degree of similarity between earlier and later stages in the development of a tradition, but at the same time they remain the same entity.
If a tradition remains the same entity in the course of development, how do we understand the replacement of one tradition by another? Laudan says that at any given time certain elements of research tradition are more central to, and more entrenched within, the tradition than other elements. It is these more central elements, which are taken, at that time, to be more characteristic of the research tradition. To abandon them is indeed to move outside the tradition, whereas the less central tenets can be modified without repudiation of the research tradition. But what are the unrejectable elements and how are they rejected? He says, when it can be shown that certain elements, previously regarded as essential to whole enterprise, can be jettisoned without compromising the problem solving success of the tradition itself, these elements cease to be a part of the unrejectable core of the research tradition.21 So, it is the old tradition out of which the new emerges. The old changes into the new tradition. They are different traditions. Since the new emerges from the old, in course of time, there is continuity between every successive stage from the beginning to the end.

It can be asked as to what happens to the theory when it is faced by an anomaly? Do we then experience a break in the change and continuity of a theory? According to Laudan, the occurrence of an anomaly raises doubts about, but need not compel the abandonment of theory exhibiting the anomaly. For him, this is because of two things: (1) In any empirical test, it is a complex conjunction of a variety of theories that is required for deriving any experimental predicting. For instance, in order to test a theoretical statement as simple as Boyl’s law, we must invoke theories about the behavior of our measuring instruments. Boyl’s law by itself predicts nothing whatever about how those instruments will behave. Now in this condition, if the prediction turns out to be erroneous, we do not know where to locate the error within the complex. It follows that we can never legitimately claim that any theory has ever been refuted.22 The decision that one particular theory within the network is false is completely arbitrary. On the other hand, to abandon a theory because it is incompatible with the data assumes that our knowledge of the data is infallible and veridical. But once we realize that data themselves are only probable, the occurrence of an anomaly does not necessarily require the
abandonment of a theory. (2) One of the most cognitively significant activities of scientists is the successful transformation of a presumed empirical anomaly for a theory into a confirming instance for that theory. There are many instances in history of science that anomalous instance has turned out to be the confirming one. For example, Prout’s view was that all the elements were composed of hydrogen and consequently the atomic weights of all elements should be integral multiples of the weight of hydrogen. Rerzelius and others found that several elements had atomic weights incompatible with Prout’s theory. These results constituted very serious anomalies for Proutian chemistry. But discovery of isotopes and refinement of isotopic techniques enabled physical chemists to separate out the isotopes of the same element. Each isotope was found to have an atomic weight that was an integral multiple of hydrogen. Previously anomalous results could now be explained on Prout’s hypothesis. Thus, the very phenomenon that had earlier constituted anomalies for Prout’s hypothesis becomes positive instance for it. This is why, Laudan says, a scientist can adhere to a theory of less effectiveness in the hope that he will improve it. He holds that the choice of one tradition over its rival is a progressive choice precisely to the extent that the chosen tradition is a better solver than its rivals. He calls this kind of appraisal the context of acceptance. Another mode of appraisal is context of pursuit. There are many historical cases where scientists have investigated and pursued theories or traditions that were potentially less acceptable, less worthy of belief, than their rivals. Scientists often begin to pursue and explore a new research tradition long before its problem-solving success qualifies it to be accepted. Prout did not leave his theory as he saw it in anomalies. He was rationally justified in doing this, for a scientific theory aims at problem solving and Prout also aimed at problem solving while adhering to the theory. His adherence to the theory culminated into a very important progress of science. Thus making of progress does not necessarily entail rejection of theory. Scientists may rationally or justifiably continue to hold the old theory.

‘Problem solving effectiveness’ of a theory means the theory maximizes the solved problems and minimizes the anomalous and conceptual problems—a theory that does this work better is a better problem-solver. But
there are two subordinate measures about whether 'problem solving effectiveness' of the tradition increases or decreases in the course of time. This account will make us understand why a scientist may continue to hold the research tradition of less ability or effectiveness:

a. The general progress of a research tradition: this is determined by both older and recent versions of the tradition. This is momentary adequacy—total progress obtained by the tradition thus far.

b. The rate of progress of a research tradition: This is about the changes in the momentary adequacy of the tradition during any specific time span.

It is important to note that the tradition may show a high degree of general progress, yet show low rate of progress. Alternatively, research tradition may have a high rate of progress during its recent past while exhibiting limited general progress.25

This subordinate measure can explain the historical fact that a scientist can often be working alternatively in two different, and even mutually inconsistent, research traditions. Particularly, during periods of scientific revolution, it is commonly the case that a scientist will spend part of his time working on dominant tradition and a part of his time working on one or more of its less successful, less fully developed rivals. It may seem to be an irrational move by a scientist. But Laudan says that scientists can have good reason for working on theories that they would not accept. To see what could count as 'good reason' here we need only consider the following general kind of case. He argues that we have two competing research traditions, RT and RT'; suppose further that the momentary adequacy of RT is much higher than that of RT', but that the rate of progress of RT' is greater than the related value for RT. So far as acceptance is concerned, RT is clearly the only acceptable one of the pair. We may nonetheless decide to work on RT' to further articulate and explore its problem solving merits. This is on the ground that it has recently shown itself to be capable of generating new solutions to problems at an impressive rate. Because most new research traditions bring new analytic and conceptual techniques, and thereby are likely to pay problem solving dividends.26
This kind of rationality of pursuit, Laudan says, is nothing other than what is implicitly described in scientific usage as ‘promise’ or ‘fecundity’. Daltonian atomism generated so much interest in the early years of the nineteenth century largely because of its scientific promise rather than its concrete achievements. At Dalton’s time, the dominant chemical research tradition was concerned with elective affinities. But Dalton’s early atomic doctrine could claim nothing like the over-all problem solving success of elective affinity chemistry; still worse Dalton’s system was confronted by numerous serious anomalies. But this does not entail that atomists shall not be able hereafter to explain these apparent anomalies in satisfactory manner. Although most scientists refused to accept the Dalton’s approach, many nonetheless were prepared to take it seriously, claiming that the serendipity of the Daltonian system made it at least sufficiently promising to be worthy of further development and refinement. Thus the pursuit of any research tradition—no matter how regressive it may appear—can always be rational.27

Laudan claims that a scientist may not only pursue a regressive tradition, he may also amalgamate two traditions for the same reason i.e., with an eye at problem-solving thereby leading to another kind of continuity. There are two ways in which different research traditions can be integrated. In some cases one tradition can be grafted onto other, without any serious modification in the presuppositions of either. Thus, in eighteenth century natural philosophy, many scientists were simultaneously Newtonians and subtle fluid theorists. Their adherence to the research tradition of subtle fluid led them to postulate imperceptible aetherial fluids in order to explain the phenomena of electricity, magnetism, heat, etc. Their Newtonianism led them to assume that the constituent particles of such fluids interacted not by contact but rather by means of strong forces of attraction and repulsion, acting-at-a-distance across empty space. The fusion of these traditions was to constitute itself a major research tradition—one which Schofield has labelled materialism. So it would be serious error to assume that a scientist cannot consistently work in more than one research traditions.

In other cases, however, the amalgamation of two or more research traditions requires the repudiation of some of the fundamental elements of
each of the traditions being combined. In these cases, the new tradition, if successful, requires the abandonment of its predecessors. In the eighteenth and nineteenth centuries, geological followers of Hutton were hammering out a new tradition, which drew on elements of Caloricist heat theories and Vuleanist geology. These research traditions could not be preserved intact and, as result, the Huttonians had to forge what was regarded as an evolutionary research tradition which incorporated elements of traditions which had been previously incompatible.28

**Non-cumulative Progress**

It is generally accepted that cognitive progress is possible if knowledge is acquired through purely cumulative theories. Cumulative theory means a theory which may add to the store of solved problems, but which never fails to solve all the problems successfully solved by its predecessors. Put slightly differently, a necessary condition for one theory, \( T_2 \), to represent progress over another, \( T_1 \), is that \( T_2 \) must solve all the solved problems of \( T_1 \) and some excess content. So a series of theories is progressive if each later member in the series must entail all the corroborated content of its predecessors and some excess content.

But this pattern of progress is rarely satisfied in the history of science. Prior to Hutton, Cuvier and Lyell, geological theorists had been concerned with a very wide range of empirical problems, such as how deposits get consolidated into rocks; how the earth originated from celestial matter and slowly acquired its present form; when and where the various animals and plants originated; how the earth retains its heat; the subterranean origins of volcanoes and hot springs and the origin and constitution of igneous rocks. Solutions of varying degree of adequacy had been offered in the eighteenth century to each of these problems. Yet after 1830, particularly with the emergence of stratigraphy, there were no serious geological theories, which addressed themselves to many of the problems mentioned above. Laudan argues that does it mean that geology was not progressive between 1830 and about 1900?, whereas geological theories after Cuvier and Lyell successfully addressed themselves to a very different set of empirical problems, including those of bio-graphy, stratigraphy, climate, erosion, and long-sea distribution.
The same is illustrated within physics by the failure of Newton's optics to solve the problem of refraction in Iceland spar which had been explained by Huygens' optics; and by the failure of early nineteenth century caloric theories of heat to explain phenomena of heat convection and generation, which had been solved by Count Rumford in the 1790's. Within chemistry, many problems had been solved by the early theories of elective affinities that were not solved by Dalton's later atomic chemistry. A still better example is offered by Franklin's electrical theory. Prior to Franklin, one of the central solved problems of electricity was the mutual repulsion of negatively charged electrical bodies. Various theories, especially vorticular ones, had solved this problem by the 1740's. Franklin's own theory, which was widely accepted from the middle to the end of the eighteenth century, never adequately came to grips with this problem. These examples show that knowledge of the relative weight or the relative number of problems can allow us to specify those circumstances under which the growth of knowledge can be progressive even when we lose the capacity to solve certain problems.

Therefore, Laudan maintains that progress is an aim-theoretic concept. To say that 'X represents progress' is an elliptical way of saying 'X represents progress towards goal Y'. If our concern is with problem-solving, we can meaningfully speak of progress without reference to accumulation. Suppose, for instance, that T₁ has already solved problems a, b, and c, while T₂ solved a, b, d, f, and g. If our cognitive aim is to possess solutions to the largest number of problems, then clearly T₂ is progressive in comparison to T₁. In problem-solving model of progress we assess the number and the weight of the empirical problems a theory is expected to solve. Similarly we assess the number and centrality of its conceptual difficulties. Constructing the appropriate scales, our principle of progress tells us to prefer that theory which comes closest to solving the largest number of important empirical problems while generating the smallest number of significant anomalies and conceptual problems. So, there is no need to satisfy the 'condition of accumulation' to make progress in science.
6.2 Question of Realism and Research Tradition

Science as Problem-oriented Enterprise

In Laudan’s analysis of the model of progress, we see that progress is essentially related to the solution of problem, and science is essentially a ‘problem-solving activity’. Laudan emphasizes that science is problem oriented enterprise. If it is established that aim of scientific research is solving the problem and nothing else, our general idea of truth as an aim of scientific discourse should be given up. However, this position is outright anti-realism. Laudan’s anti-realism is established on two grounds. One, if science is essentially a problem oriented enterprise, it by-passes truth as its aim. Two, scientific activity is essentially problem-solving activity. But solution of a problem does not necessarily mean the achievement of truth.

We will see here how Laudan regards science as problem-oriented enterprise. Scientists work on problems, and problems are not objective; rather problems arise when they experience or feel the tension. He claims that any answer whatever to the question is regarded as solution to that problem, the answer is called theory in scientific parlance. Any theory is constructed to solve the problems. Theories matter cognitively, insofar as, and only insofar as, they provide adequate solutions to problems. If problems constitute the questions of science, it is theories, which constitute the answers. The function of a theory is thus to resolve ambiguity, or to reduce irregularity into uniformity, or to show that what happens is some how intelligible and predictable. It is this complex of functions to which we refer when we speak of theories as solutions to problems. Therefore, Laudan says that the first and essential acid test for any theory is whether it provides acceptable answers to interesting problems; in other words, whether it provides satisfactory solutions to important problems. So the aim of a theory is solving the problems felt by us. He mentions four types of problems and maintains that functions of scientific theories are to solve these problems: scientific problems, intra-scientific problems, normative problems and world view problems.

Empirical Problem: Scientists encounter two types of problems which scientific theories are designed to solve: empirical problems and conceptual
ones. For example, Laudan illustrates, when we observe that heavy bodies fall towards the earth with amazing regularity and ask how and why they so fall, we pose an empirical problem. We observe that alcohol left uncovered in a glass soon disappears. To ask an explanation for that phenomenon is again to raise an empirical problem. We may observe that the offspring of plants and animals bear striking resemblances to their parents. To inquire into the mechanisms of trait transmission is also to raise an empirical problem. More generally, Laudan says that anything about the natural world that strikes us as odd, or otherwise in need of explanation, constitutes an empirical problem.\(^{33}\)

We can roughly divide empirical problems into three types, relative to the function they have in theory-evaluation: (1) Unsolved problems — problems that have not yet been adequately solved by the theory. This is also called potential problems, these constitute what we talk to be the case about the world, but for which there is as yet no explanation, (2) Solved problems — those empirical problems that have been adequately solved by a theory. These are also called actual problems, (3) Anomalous problems — those empirical problems that a particular theory has not solved, but that one or more of its competitors have. An anomalous problem does not mean a falsifying instance.\(^{34}\) For Laudan, solved problems count in favour of a theory, anomalous ones constitute evidence against a theory, and unsolved problems simply indicate lines for future theoretical inquiry. Therefore, the hallmark of scientific progress is the transformation of anomalous and unsolved empirical problems into solved ones. However, these empirical problems can not be deemed to be objective problems, because empirical problems are themselves theory-dependent. Unless our theoretical framework indicates any phenomenon as a problem, the phenomenon remains unproblematic. This is why fall of Apple was simple event to us but was a great and important problem to Newton. Laudan says that whether a given phenomenon is a genuine problem, or how important it is, or how heavily it counts against a theory - these are very complex questions. We are unsure if an empirical effect is genuine; because many experimental results are difficult to reproduce, because physical systems are impossible to isolate, because measuring instruments are often unreliable, because the theory of error even
leads us to expect ‘freak’ results etc. Moreover, it often takes a considerable time before a phenomenon is sufficiently authenticated to be taken seriously as a well-established effect. Secondly, even when an effect has been well authenticated, it is very unclear as to which domain of science it belongs to, and therefore, which theories should seek, or be expected, to solve it. For instance, it is unclear whether the problem ‘the moon seems larger near the horizon’ is an astronomical, or an optical, or a psychological problem or what. As a result of this uncertainty, no one could show convincingly that theories in any particular domain should be expected to solve such problems.35

Laudan holds that an empirical problem, depends in part on the theory we possess. In this sense, an empirical problem is not empirical as such. But it is called empirical because it is a problem about the world. If we ask ‘how fast do bodies fall near the earth?’, we are assuming that there are objects akin to our concepts ‘body’ and ‘earth’ which move towards one another according to some regular rule. That assumption, of course, is a theory-laden one, but we nonetheless assert it to be about the physical word. For, empirical problems are first order problems; they are substantive questions about the objects that constitute the domain of any given science. All this means that the problems are different from facts, and solving a problem cannot be reduced to explaining a fact. For Laudan, a problem need not accurately describe a real state of affairs. All that is required is that it be thought to be an actual state of affairs by some agent. There are many facts about the world which do not pose empirical problems simply because they are unknown. Even many known facts do not necessarily constitute empirical problems. To regard something as an empirical problem, we must feel that there is a premium on solving it. At any given moment in the history of science, many things will be well-known phenomena, but will not be felt to be in need of explanation or clarification. It was known, since the earliest times, for instance, that most trees have green leaves. But such a fact only becomes an empirical problem when someone decided it was sufficiently interesting and important to deserve explanation. Finally, problems recognized as such at one time can cease to be problems at later times. The staggering problem ‘how the earth took its shape’ within the last 6,000 to 8,000 years, no longer remained a problem to be
solved. This is about empirical problems. They are not entitled to represent reality, therefore, solution of them is not endorsed with truth of the world.

The second type of scientific problems is conceptual one. With reference to conceptual problems, Laudan cites the example of epicyclic astronomy of Ptolemy. Its core criticism did not deal with its adequacy to solve the chief empirical problems of observational astronomy. It was readily granted by most of Ptolemy's critics that his system was perfectly adequate for 'saving the phenomena'. Rather the bulk of criticism was directed against the conceptual credentials of the mechanisms Ptolemy utilized for solving the empirical problems of astronomy. In the same way, the debates between Copernican and Ptolemaic astronomers (1540-1600), between Newtonians and Cartesians (1720-1750), between wave optics and particle optics (1820-1850), between atomists and anti-atomists (1815-1880), etc. are all examples of important scientific controversies where the empirical support for rival theories was essentially the same. Such problems are called conceptual problems.

For Laudan, the most vivid type of internal conceptual problem arises with the discovery that a theory is logically inconsistent, and thus self-contradictory. A second class of internal conceptual problems arises from conceptual ambiguity or circularity within the theory. Unlike inconsistency, the ambiguity of concepts is a matter of degree rather than of kind. Faraday's early model of electrical interaction was designed to eliminate the concept of action-at-distance. As it happened, Robert Hare showed, Faraday's model required short range actions-at-distance. Faraday had merely replaced one otiose concept by its virtual equivalent. This kind of criticism led Faraday to re-think his views on matter and force and was eventually responsible for the emergence of Faraday's field theory.

A conceptual problem is a problem, which is exhibited by some theory or the other. Conceptual problems have no existence independent of the theories that exhibit them. If empirical problems are first-order questions about the substantive entities in some domain, conceptual problems are higher-order questions about the well-foundedness of the conceptual structures that have
been devised to answer the first-order questions. Conceptual problems arise for a theory, T, in one of the following ways:

a. When T exhibits certain internal inconsistencies, or when its basic categories of analysis are vague and unclear; these are internal conceptual problems.

b. When T is in conflict with other theory or doctrine T', which proponents of T believe to be rationally well founded: these are external conceptual problems.

c. When T violates principles of research tradition of which it is a part.

d. When T fails to utilize concepts from other, more general theories to which it should be logically subordinate.39

However, such conceptual problems emanating from the disagreement with a worldview, or with an accepted normative theory, or with the theory accepted in other scientific disciplines, are finally produced by the imaginative mind of the scientist with a view to solve the empirical problems thereof. They are produced from the disagreement with existing, dominating way of understanding. Therefore, conceptual problems are independent of physical order of scientific investigations. All four types of conceptual problems mentioned above are very much theoretical problems. Therefore, any solution of them by some other theory cannot be deemed to be describing or representing the phenomenal features of the real world.

Intra-scientific Problem: According to Laudan, this kind of problem is produced when the imagination of a scientist goes counter to the theory accepted in other parts of science. For instance, Ptolemy’s system, for all its empirical values, made the assumption that certain planets move around empty points in space, that planets do not always move at constant speed, and the like. But these were in flagrant contradiction with the then universally accepted physical and cosmological theories about the nature and motion of the heavy bodies. In spite of ingenious efforts to reconcile these differences by Ptolemy and others, most of the crucial conceptual problems remained, and were to plague the development of mathematical astronomy until the end of the seventeenth century. To mention another instance, the astronomical
system of Copernicus's made a number of assumptions about the motion of bodies which were inconsistent with the then accepted Aristotelian mechanics. One of the strongest sixteenth century arguments against the Copernican system consisted in pointing out that the theory of Copernicus was unacceptable because it ran counter to the tenets of the best-established physical theory. It was Galileo's signal contribution to deal with this conceptual problem. He recognized the incompatibility between Aristotelian physics and Copernican astronomy and remedied the situation by designing a new physics that was independently plausible as well as compatible with Copernican astronomy. Intra-scientific conceptual problems occur because scientific disciplines and domains are never completely independent of one another. The chemist will look to the physicist for ideas about atomic structure; the biologist will utilize chemical concepts when talking about organic microstructures. These intra-scientific problems are no more than theoretical problems or human intellectual problems having no necessary relationship with the real world.

**Normative Problem:** Laudan points out that science is an activity conducted by seemingly rational agents. Therefore, science has certain aims and goals. A rational assessment of science must, therefore, articulate those goals. But there are questions with regard to the nature of these goals. The question of the appropriation of the most effective method for achieving these goals is also of vital significance. Only through an effective or appropriate methodological strategy can we be oriented as to what or what not should be done by the scientists, in order to achieve the cognitive, epistemic, and practical goals of the scientific enterprise. As scientist is to bear these norms in his assessment of theories. History of science tells us that these norms have been perhaps the single major source for most of the controversies in theory acceptance. If a scientist has good grounds for accepting some methodology and if some scientific theory violates that methodology then it is entirely rational for him to have grave reservations about the theory. This situation constitutes the conceptual problems or what we call normative problems. An example of this kind of conceptual problem is the development of Newtonian theory. The development of this theory goes counter to the dominant
inductivist methodology of the times. This incompatibility produced an acute conceptual problem. Some of Newtonians sought to resolve this problem by simply repudiating their physical theory. On the other hand, some insisted the norms themselves should be changed so as to bring them into link with the best available physical theories. In the light of further conceptual problems, the latter group devised a new methodology, i.e. the hypothetico-deductive methodology. From normative kind of problems we know that science is goal oriented activity where goals and the methods both are settled by scientific minds. Human goals finally operate within human domains. So solution of such problems cannot be deemed to be resolving the problems pertaining to real physical world.

*Worldview Problem:* The world view problem is very important for the question of realism. Laudan says that this type of external conceptual problem arises when a particular scientific theory is seen to be incompatible with some other body of accepted but prima facie nonscientific beliefs. For him, within any culture, there are widely accepted beliefs that go beyond the scientific domain. Such beliefs fall in areas as diverse as metaphysics, logic, ethics, theology, and social and moral ideologies. Although the total populations of reasonable beliefs changes with time, there has never been a period of history of thought when the theories of science exhausted the domain of rational beliefs. One of such problems is the confrontation of Newtonian physics with Cartesian ontology of force. How can bodies exert at points far removed from the bodies themselves? He says that Buchdoh, Heimann, and McGuire have convincingly shown that this issue had become the central philosophical and scientific problem of the Enlightenment. Not satisfied with Cartesian ontology, philosophers and scientists all over Europe began to re-evaluate such traditional issues as the nature of substance, the relation of properties to substances, and particularly, the nature of our knowledge of substance. What resulted from this reappraisal at the hands of Kant, Priestley, Hutton, and others was a new ontology which argued for the priority of force over matter and which made the powers of activity into the basic building blocks of the physical world. Similarly, one of the most persistent sets of conceptual problems in twentieth century physics has been the dissonance between
quantum mechanics and our philosophical beliefs about causality, change, substance and reality. These are the world view problems.

However, these are the problems which scientists seek to solve. The aim of science is problem-solving, whether the problems are empirical or conceptual. The empirical problems represent the ‘correspondence’ and conceptual problems represent the ‘coherence’ about our knowledge. History of science reveals that a worldview problem is more serious a problem than an empirical one. For instance, no one proposed abandoning Newtonian mechanics because it could not accurately predict the motion of the moon; but many thinkers were seriously prepared to dismiss Newtonian physics because its ontology was incompatible with the accepted metaphysics of the day. This is not because science is more rationalistic than empirical; but rather because it is usually easier to explain away an anomalous experimental result than to dismiss out of hand a conceptual problem. From this point of view, we see that metaphysics and other ideologies have an effective influence on theory formulation. The theories of this kind cannot necessarily claim to be committed to the reality of the world. Rather they produce such knowledge which is constructed within human ideology and metaphysics.

Significance of Problem: However scientific activity, for Laudan, is no more than solving these problems—the products of science are solutions of these problems. In fact, all our scientific achievements are solutions of problems. However, there is another thing that matters significantly: all problems and all solutions are not of equal weight. Therefore, the relative importance of problems and the degree of adequacy of solutions have become the crucial points of debate in contemporary scientific discourse. Some solutions are decidedly better and richer than others. In assessing the adequacy of any theory we will ask not how many facts confirm it, but how important those facts are. We will ask not how many problems the theory solves, but about the significance of those problems. So there is a dialectic between challenging problems and adequate theories. This is a historical fact that scientists have often abandoned a theory in the face of only a few anomalies and have at other times retained a theory in the face of an ocean of empirical refutations. So it is not so much how many anomalies a theory
generates, but rather how cognitively important those particular anomalies are.\textsuperscript{46} This means that a problem is valued in terms of its significance, and it is no more than relative importance of problems understood in the light of our knowledge at a particular time. So problems and their solutions are not impersonal.

This point will be clearer in the light of another feature of our understanding of the scientific problems. It is often unclear whether a seeming problem really is an empirical problem, i.e. whether there is any natural phenomenon there to explain at all. For example, experiments in extrasensory perception is a case in point. Most scientists today would claim to be unsure that there is any indication of ESP, which is in need of theoretical explanation (The so-called pseudo-sciences generally flourish on just such cases, where it is unclear whether there’s, at the outset, any problem which needs to be solved). So, whether a problem is a genuine problem depends on the theoretical understanding about that problem. Before the time of Descartes, problems of the impact and collision of bodies were at the periphery of the concerns of writers on motion and mechanics, scarcely even recognized as problems that a theory of motion should resolve. But the mechanical philosophy of Descartes promoted problems about impact to the forefront of mechanics. And the problems of impact and collision were regarded as among the most urgent in physics. It means that ‘recognition’ and ‘importance’ of a problem depends on theoretical understanding. Without an appropriate type of theory, modes of problem weighting would be impossible.\textsuperscript{47}

Like empirical problems, there are certain circumstances that tend to promote or demote initial importance of conceptual problems also. Laudan mentions some such situations as follows that may show that conceptual problems also become significant in the context of their importance accorded to them through and by human understanding.

1. The nature of the logical relation between two theories exhibiting inconsistency instead of being mutually supportive produce conceptual problems. Other things being equal, the greater the tension between two theories, the weightier the problem will be.
2. When a conceptual problem arises as a result of a conflict between theories, \( T_1 \) and \( T_2 \), the seriousness of that problem for \( T_1 \) depends on how confident we are about the acceptability of \( T_2 \). If \( T_2 \) has proven to be extremely effective at solving empirical problems and if its abandonment would leave us with many anomalies, then matters are very difficult for the proponents of \( T_1 \). If, on the other hand, \( T_2 \)'s record as a problem solver is very modest, then \( T_2 \)'s incompatibility with \( T_1 \) will probably not count as a major conceptual problem for \( T_1 \). In both of the cases, a feeling of tension, or confidence, points to the role of the scientific community's socio-psychology in the crystallization or development of scientific problems. This reminds us of the Kuhnian sociology of science.

3. If a theory poses a certain conceptual problem, there is usually some ground for hope. We think that with very minor modification in the theory, we can bring it into line and thus eliminate the problem. If, on the other hand, a theory has been known to have a particular conceptual problem for some length of time, if partisans of that theory have repeatedly and unsuccessfully tried, to make the theory consistent with our norms and other accepted beliefs, then that problem assumes an ever greater importance. It also assumes an ever-greater significance in debates about the acceptability of the theory. Solution of such a problem depends on the ability of the concerned scientists: we are again reminded of Kuhn's example of tools in the hand of carpenters. It indicates the instrumental dimension of scientific theories too that assumes significance in our efforts to resolve scientific problems.

The essential task of science is to solve problems, be they empirical or conceptual. Scientific research is a problem-solving activity. Science may have as wide a variety of aims as individual scientists have a multitude of motivations. For Laudan, science aims to explain and control the natural world. Truth is not the only interesting motive for scientists. Social utility and prestige too motivate them in theory-formulation and theory choice.
Solution versus Truth

The same thing can be discussed from another perspective. The objective of science is solving the problem even when problems themselves indicate the anti-realistic nature of scientific discourse. The solution of a problem can not claim to be an attainment of truth. The research tradition view of scientific progress tells us that science is essentially the 'problem-solving activity' and its aim is solving of the problems, whether empirical or conceptual. From beginning to the present, history of scientific activities shows that all scientific achievements are but the solutions of the problems. Those achievements are considered as advancement of science because they have solved the problems that scientists encountered. Progress of science means to increase the amount of the solved empirical problems and decrease the amount of the anomalous problems and the unsolved ones. So, scientific progress means progress in solving the problems. Now the question is that do the solutions of problems amount to achieving the truth, or even approaching the truth? Do the solutions of problems represent the real picture of the world?

According to Laudan, one of the richest and healthiest dimensions of science is the growth through time, and the standards it demands for something to count as a solution to a problem also change with time. What one-generation of scientists will accept as a perfectly adequate solution, will often be viewed by the next generation as a hopelessly inadequate one. The history of science is replete with cases where solutions whose precision and specificity were perfectly adequate for one epoch were deemed totally inadequate for other. In physics, Aristotle cites the problem of fall as a central phenomenon for any theory of terrestrial mechanics. Aristotle himself sought to understand both 'why bodies fall downwards' and 'why they accelerate in fall'. Aristotelian physics provides answers to these questions, which were taken seriously for over two millennia. For Galileo, Descartes, Huygens, and Newton, Aristotle's views were not really solutions to the problem of fall at all, for they failed utterly to explain the uniform character of the fall of a body. Therefore, a solution is always a solution of the time.
For a logician, the explaining theory must entail an exact statement of fact to be explained and the theory must be either true or highly probable. On the other hand, Laudan says, a theory may solve a problem so long as it entails even an approximate statement of affairs concerning the problem. He says that in determining whether a theory solves a problem, it is irrelevant whether the theory is true or false, well or poorly confirmed. Moreover, what counts as a solution to a problem at one time may not necessarily be regarded as such at all times. He also says that facts are very rarely, if ever, explained because there is usually disconcordance between what a theory entails and our laboratory data. By contrast, empirical problems are frequently solved for problem solving purposes. So we do not require an exact, but only an approximate, resemblance between theoretical results and experimental ones. Newton did solve, and widely regarded as having solved, the problem of the curvature of the earth—even though his results were not identical with observational findings.\(^5\)

Furthermore, for Laudan, the notion of solution is highly relative and comparative in a way that the notion of explanation is not. We can have two different theories that solve a problem, and yet say one is a better solution than the other. For instance, philosophers of science have been very troubled by the relationship of Galileo and Newton’s theories of fall and the data thereof. They were unable to say that both theories explained the phenomenon of fall. Whereas it is, for Laudan, surely more natural historically and more sensible conceptually, to say that both theories solved the problem of free fall, one perhaps with more precision than the other. It redounds to the credit of both that each provided an adequate solution to the problem at hand. We can all agree, for instance, that Ptolemy’s theory of epicycles solved the problem of retrograde motion of the planets, regardless of whether we accept the truth of epicyclical astronomy. Equally, every one agrees that Thomas Young’s wave theory of light—whether true or false—solved the problem of the dispersion of light. Lavoisier’s theory of oxidation, whatever its truth status, solved the problem of why iron is heavier after being heated than before.\(^5\) So, a solution targets a problem, not the reality of the world.
Laudan also points out that determining whether a tradition is successful in solving any problem does not mean that tradition has been ‘confirmed’ (or ‘refuted’). Nor can such an appraisal tell us anything about the truth (or falsity) of the tradition. He shows that a research tradition may be enormously successful at generating fruitful theories and yet flawed in its ontology and methodology. Equally a research tradition might be true, and yet unsuccessful at generating theories that were effective problem solvers. Thus when we reject a research tradition, we are merely making a tentative decision not to utilize it for the moment because there is an alternative to it that has proven to be a more successful problem solver.\textsuperscript{52}

Moreover, there are cases in the history of science where the success of a theory is highly suspect if the theory is linked to an unsuccessful research tradition—even though the theory is possessed of great problem solving merits. Laudan mentions that Rumford’s theory of heat conduction and convection was far superior to any alternative theories of thermal flow in fluids available in the period from 1800 to 1815. Nonetheless, few scientists took Rumford’s theory seriously because the research tradition in which he worked had been discarded by the emergence of a rival research tradition. Contrarywise, a theory even an inadequate one, will have some strong arguments in its favor if it is linked with a research tradition that is otherwise highly successful. For instance, theories of mechanistic physiology in the late seventeenth century were highly regarded in many circles where the mechanistic research tradition was flourishing even though they were significantly inferior to certain theories in other less successful research traditions.\textsuperscript{53} All this asserts that success of the tradition cannot be regarded as the success in achieving truth. In the same token, failure does not necessarily assert the falsity of tradition. When we say that $a$, is an anomaly for a theory $T$, we are not saying that $a$, falsifies $T$; rather we are saying that $a$, the sort of problem which a theory such as $T$ ought to be able to solve has not been solved by it as yet. Such an anomaly does not prove that $T$ is false; it only raises the doubts about the problem-solving effectiveness of $T$.

Therefore, Laudan says that research traditions should not be judged in terms of truth or falsity, for, research traditions are historical creatures. Since
they are created for solving the problems, they have merits or demerits with regard to that purpose—not truth-value. Research traditions are articulated in a particular intellectual milieu, and like all other historical institutions they wax and wane. Just as surely as research traditions are born and thrive, so they die and cease to be seriously regarded as instruments for furthering the progress of science.\textsuperscript{54}

In the final analysis, to suppose truth to be a goal of science is to portray the activity of science as irrational. Truth can never be recognizably negotiated by a scientist. Rational behaviour demands that we have a recognizable goal. The achievement of the goal should be publicly worthy of celebration. The failure to attain the goal, too should be, in principle, confirmable. Problem-solving rather than truth-finding can be deemed to be such a recognizable goal. Truth is not a goal of science and of individual scientist.\textsuperscript{55} History has a tendency to characterize the aim of science in terms of such transcendental properties as truth and apodictic certainty. If science is so conceived, it emerges as non-progressive since we evidently have no way of ascertaining whether our theories are more truth-like or nearly certain than they formerly were. We do not yet have a satisfactory semantic characterization of truth-likeness, let alone any epistemic account of when it would be legitimate to judge one theory to be more nearly true than another.\textsuperscript{56} If science aims at any recognizable goal, then that is not ‘truth’ but ‘solution’ to the problems about the world.

To conclude, for Laudan, the fundamental scientific products are research traditions. According to him, there are two types of theories. One of them denotes a specific set of related doctrines which can be utilized for making specific experimental predictions and for giving detailed explanation of natural phenomena; for instance, theory of electromagnetism. By contrast the term ‘theory’ also denotes much more general, much less easily testable, sets of doctrines or assumptions; for instance, theory of evolution or kinetic theory of gases. In this case we are referring not to a single theory, but to a whole spectrum of individual theories. The term ‘evolutionary theory’ for instance does not refer to any single theory, but to an entire family of doctrines that are historically and conceptually related. This kind of theory is
the primary tool for understanding. Larry Laudan calls this type of theory scientific research tradition. Problem selection, heuristics, standard of appraisal – all are determined by research tradition. For Laudan, scientific research starts from encountering of a problem and ends with the solution of that problem. So, scientific activity is problem-solving activity. It is rational to accept a research tradition which has greater problem-solving ability in place of a research tradition which has less problem-solving ability; problem-solving effectiveness is the standard of judgement in theory choice. Therefore, for Laudan scientific change is rational and progressive.

Progress occurred in this way is yet non-cumulative progress. The principle of accumulation says that the later theory is supposed to add to the store of solved problems, but never fails to solve all the problems solved by its predecessors. According to research tradition view, a theory is better only for its greater problem-solving ability, though it fails to solve exactly those problems that are solved by its predecessors.

The most important part of the character of progress is associated with the position of truth in the progress. For Laudan, science is essentially a problem-solving activity. The aim of scientific research is solving the problems and nothing else. Science solves empirical problems, conceptual problems, intra-theoretic problems, world-view problems, etc. All these problems are created by us when we come across the world with our own intellectual capacities. So, none of these problems is necessarily related with reality. Our research tradition which is the primary tool of understanding determines which of the problems are significant for our research. This indicates that problems are not objective, rather selective. Furthermore, the solution of these problems does not entail truth. Laudan holds that the richest and healthiest dimensions of science is the growth through time, and the standards it demands for something to count as a solution to a problem also changing with time. For him, a theory solves a problem so long as it entails an approximate statement of affairs concerning the problem. Therefore, in determining whether a theory solves a problem, it is irrelevant whether the theory is true or false. It is research tradition which determines when a scientist is to be satisfied with the solution of the problem. Therefore, a solution can not describe the real state of affairs.
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Chapter – 7

The Structure of Progress in Evolutionary Change

7.1. Change and Continuity in Evolution

*Concept: Neither Static nor Dynamic*

Toulmin’s view of evolution of scientific change is based on the reconciliation of two opposite extreme views of the standard of rationality. On the one hand, arguments are given for static rationality that are not changeable in the course of time; on the other hand, arguments are given for dynamic rationality that have no constant meaning of them, and they are as changing as time does. However, neither of them does conform to the history of science. Toulmin criticizes these views and shows that concepts are neither static nor dynamic but concepts evolve in the processes of evolution.

The argument for ultimate, fixed, ahistorical static standard of rational judgment is put forward among others, mainly by Gottlob Frege. He holds the argument that if everything were in continual flux and nothing maintained itself in a fixed state for all times to come, then there would no longer be any possibility of getting to know about he world. Everything would plunged into confusion.¹ In view of the same, we must concern ourselves only with timeless ideas. Accordingly, human Mind must struggle towards the same, little by little. This immense intellectual effort at last will achieve knowledge of a concept in its pure form, by stripping off the irrelevant accretions which veil it from the eye of the Mind.²

However, Toulmin says, reality is very different. He stresses that outside logic and mathematics, this Olympian stance is not so easily maintained. On the other hand, historical realization cannot be escaped by this conviction of ultimate principle. For him, Frege’s method distracts us from the process of conceptual change. For instance, if we compare the dynamical concepts of Buridan, Aristotle and of Einstein we will find that no such standard is operating in their work at all. The search for a perennial standard in the works of Buridan, Aristotle and Einstein or for that matter, in the political ideas of Machiavelli, Plato and of Marx is a fruitless venture. For Toulmin, any
attempt to judge intellectual boundaries between rival theories, soon drives us beyond the range of a purely formal analysis. What entities we shall accept in our account of the world, depends on what formal language we decide to use in describing it. In this sense, we are condemned to an ontological relativity. In addition to this, there is a general complaining that the formal system is so abstract that it has no bearing on the arguments used in the contemporary theoretical physics. For example, Quantum mechanics had never been formulated according to the rigorous standards of modern logic.

Therefore, the rationality we are concerned with is neither a formal nor a pragmatic matter. The pragmatists and positivists advocate ‘simplicity’ and ‘convenience’ to be the reason for theory change. However, even these criteria are violated with impunity by the actual history of science. History tells us that Copernicus’s new heliocentric system was far more specific, varied and sophisticated than what is hinted by such vague terms as ‘simplicity’ and ‘convenience’. Especially at the outset, the Copernican theory was by many tests substantially less simple or convenient than the traditional Ptolemaic analysis.

Historically speaking, successive theories are first applied and later modified in course of the ongoing evolution of relevant intellectual activities. No theory emerges once and for all; every theory matures in the course of time. Even man’s faculties of understanding develop historically. Toulmin says, the problem of human understanding is two-fold; ‘man knows’ and ‘he is conscious that he knows’. We acquire knowledge and at the same time we are aware of our own activities as a knower. It means that knowledge and the knower both are the object of knowledge. Knowledge grows, and by reflection upon knower, it deepens as well. Looking outside ourselves we extend our knowledge, looking inwards we deepen our knowledge. When our knowledge grows, we reflect upon knower accordingly; when our reflection deepens, we acquire knowledge accordingly. Throughout the history of thought these twin activities have gone on continuously. Change in one results in the change of other. Accordingly, we are busy reconsidering our picture of ourselves as knower in the light of recent extensions to the actual content of our knowledge. Toulmin argues that the same type of object will fall within the domains of several
different sciences, depending on what questions are raised about it. Also, what it is that makes a problem ‘problematic’ will be from the point of view of specific descriptions. Thus, the nature of an intellectual discipline always involves both its concepts and also the men who conceive them. Thus knowledge and knower both are the subject matter of a discipline.\(^5\)

So, the standards for appraising our existing stock of ideas are not self-evident. Toulmin says that questions about the processes, procedures, and mechanics by which our concepts are developed, acquired, used and improved may be topics for particular sciences or disciplines. They may be discussed from the perspective of neurophysiology or logic, cultural anthropology or the sociology of science, etc. However, the central issues of epistemic philosophy—justification and appraisal, judgment and criticism etc. — have never been concerned with factual matters alone. By contrast, the philosophical probing into epistemology also goes as far as it can go what sources our concepts ultimately derive their intellectual authority from. Like the question of the ultimate source of moral and political authority, this question too has generated great controversies of judgment and evaluation.\(^6\)

In this regard, we may consider the influence of the belief in a ‘fixed order of nature’. Toulmin says that until almost 1800 A.D., most scholars and scientists were restricted within an ahistorical worldview. Fixed laws in physics and fixed principles of understanding were deemed to be operating universally and eternally. This fixed order of nature was immutable also in human body and brain along with the rest of the material world. This is again a short step to assuming that human nature was similarly fixed and permanent. So the fundamental epistemological task was to discover the principles or processes by which human mind acquires intellectual mastery over the order of nature. Since ‘the intelligence of man’ and ‘the intelligibility of nature’ presumably are operating on stable unchanging principles, the relationship between them is presumably stable and unchanging as well. Fixed mind masters fixed nature according to fixed principles.\(^7\)

But Toulmin says the position of these presuppositions of seventeenth century natural and social sciences has changed drastically. The ultimate unchanging particles of matter, the stable planetary systems and animal species.
the timeless imperatives of morality and social life--- all assumed or presumed, supposed or proposed eternal principles and values lost their vigour and rigour and shine and stream. For Toulmin all principles and values are historically developing or evolving. Today physical sciences have broken their earlier ties with theology. Even the scientific phrase ‘laws of nature’ is no longer thought of as implying the ‘sovereign will’ of creator. These laws are, by now, considered as merely working assumptions. This is only a part of the picture. The older axioms have long since been called into question. Their implications have been discounted. The structural invariants of the seventeenth century world picture have all crumbled under the ongoing march of history and been transformed into historical variables.

Toulmin says that historical realization is diametrically opposite to the philosophy of Plato who deemed rationality to be ultimately associated with certain ‘ideas’ that are independent of our individual Mind. It goes against the medieval theological approach as well which argues that the objective ground of rational knowledge lays in the divine Mind. It also goes against the Castesian contention that rationality lays in the harmony established by God between those ideas which human mind finds totally ‘clear and distinct’. The Kantian definition of rationality as structuring of our experiences can also not be acceptable to the advocates of historically evolving rationality. Whatever the so-called ultimate sources of rationality that were deemed to be historically invariant during nineteenth century came into head-on collision with discoveries of history and anthropology. Montesquieu had made it familiar that human populations and cultures become different in accordance with different geographical locations climates, soils, resources and traditions. But even in his view, Reason and Nature remained as fixed as ever. He offered environmental explanations of differences. If such and such environmental situations are offered then such and such life will develop. It sounds to be geometrical reasoning. The problem of human understanding today cannot be set in Aristotelian terms also. In Aristotelian philosophy, man’s epistemic task is to cognize the fixed essence of nature. Today’s epistemology is no longer Hegelian as well. For Hegel human mind alone develops historically against a static background of nature.
Historical change is beyond human control. Some new factor or influence or idea enters — imperceptibly but irreversibly—into the course of our intellectual development. For example, Crook’s voyage was planned to establish finally the eternal structure of God’s creation. But its real outcome was concentration of attention on the variety and apparent inconsistency of man’s moralities, cultures, and ideas. In this way, the most forcible impression of the voyage had come not from its intended astronomical results, but rather from some curious anthropological by-products. Moreover, theoretical as well as practical disagreements are often decided by the balance of power rather than by the principles. There is a refusal to listen to the opponent’s case, an obsession with power rather than principle, and a willingness to impose by violence or threats opinions which have failed to carry conviction on their merits. Such factors cannot be ignored in the intellectual articulation and historical development of any discipline.

Therefore Toulmin says that we should come to terms with the phenomenon of inevitable interaction between human Ideas and Natural world; neither of them is an invariant. Instead of a fixed mind, gaining command over fixed nature by applying fixed principles, we should expect to find variable epistemic relationship between a variable Man and a variable Nature. That is, the relations between outward-looking and inward-looking activities changes from one period of intellectual history to another.

For Toulmin, the discoveries of ethnology and anthropology have given us a more profound understanding of the past. They have also given us powerful explanations for cultural variability. Contemporaneously, even the basic categories of human thought are deemed to be the products of a historical sequence rather than qualifications of ‘pure reason’. Toulmin says that since the beginning of twentieth century, men have finally become aware that the relativity of human thought affects all types of concepts; be they moral or mathematical. What concepts a man employs, what standards of rational judgment he accepts, how he conceives his life and the world outside, etc., depend not only on the universal human nature, but also on when he happens to be born and where he happens to live. We learn to regard certain methods as superstitious only to find them operating somewhere else. At
various periods of history different methods have carried equal conviction and authority. Therefore it is clear that conceptual diversity is a characterizing feature of the history of science. It is implausible to suppose that there is any fixed, absolute and unchanging set of concepts adequate for doing science.

**Revolution to Evolution**

But this historical evolution does not culminate into relativism and hence into the denial of continuity of scientific change. R.G. Collingwood who is a giant advocate of relativism holds that we are 'acculturated' by our upbringing. As a result, we think according to intellectual presuppositions characterising our own culture. These are just like ethical presuppositions that determine not only what kinds of conduct we consider right or wrong, but also what kinds of phenomena we regard as puzzling or self-explanatory. Similarly in science, such presuppositions in which we are brought up, determine not only what kinds of things we consider true or false, but also what kinds of phenomena we regard as puzzling or self explanatory. For him, what type of picture of the world we use to interpret our experience, what type of scientific arguments and evidence we find cogent or plausible etc., are determined by these presuppositions. Also these presuppositions of men, in other words, their intellectual standards, have varied between different historical and cultural milieus in just the same way as their ethical and aesthetic standards. Therefore, the search for a rationality transcending a particular milieu is the pursuit of a will-o-the-wisp.

Collingwood also holds that our concepts form not axiomatic system, but system of presuppositions; and the logical relations between them are not truth-relations but meaning-relations. In this system, these presuppositions remain merely relative ones, because the same concepts that are presupposed at one level will, at another level, be dependent on yet more general presuppositions. At this level, the summit of the conceptual hierarchy the most general presuppositions do not depend on a yet more general kind. Collingwood calls these concepts and principles the 'absolute presuppositions'. Here their validity does depend on their own culture.
Since the meaning of the presuppositions depends on the varying cultural milieus, there cannot be continuity in the development through the successive concepts. Whereas the fixed principles do recognize the continuity, it cannot do so to the relevance of time of scientific change. Again, a relative presupposition system does recognize the relevance of time, it cannot do so to the continuity or development in theory change. The formal analysis of pure concepts and the historical analysis of absolute presuppositions are two opposite extremes. Neither of the systems can succeed to reconstruct the actual history of science. For Toulmin, they fail due to their acceptance of the familiar assumption, viz., ‘rationality must be equated with logicality’. Accordingly, they both assume that different concepts and beliefs can be compared rationally only so long as they can be referred to a ‘logical system’. The only difference between these two systems is that while formal analysis conforms to ‘logical system’, historical analysis cannot do so. A single ‘logical system’ is presupposed both by conforming and non-conforming stances. The absolutists assert that the universal impartial, and rational judgments can be formulated into eternal principles, whereas, the relativists simply deny that any such standpoint can have any universal validity. This common assumption prevents both men from coming to terms with the rationality of conceptual change. Toulmin says that rationality lies neither in a single ‘logical system’ nor in a temporal sequence of such systems but rather in the process of evolution. In the process of evolution, there is both continuous development and the relevance of history or time. Therefore, rationality in the growth of scientific concepts is to be understood in terms of ‘process’ instead of ‘system’. This process is evolutionary process that shows the relativity of scientific concepts without falling into relativism.

To understand the process which maintains rationality within relativity through time Toulmin gives an example from the practice of law. In the process of legal history, the practical procedures which lawyers, judges, and professors of jurisprudence have dealt with, come up for formal philosophical analysis only later. Jurisprudence corresponds to the historical or cultural milieus of legal practice. In the legal practice, lawyers and judges manage to work their way through problems, for which philosophers have not as yet stated any
coherent or satisfactory theoretical solution. Furthermore, if we demand exact
criteria for distinguishing successive periods of legal history, then philosophers
will tend to be question-begging. He argues that the phrases like ‘the peri clean
era’, ‘the high middle ages’, and ‘the Enlightenment’ are notoriously vague.
The culture of Spain is, no doubt, different from the culture of Finland; but
what about Galicia and Castile, Catalonia and Roussillon. Are we there
concerned with so many different cultures, or rather with so many variants of a
single culture? Evidently, neither ‘culture’ nor ‘period’ can be divided off with
complete theoretical precision; and ‘historico-cultural milieus’ are doubly ill-
defined, combining the vagueness of both. Another aspect of this example is
that judicial separations and judicial comparisons go on simultaneously. That is,
periodically different courts of the same country, or different courts of the
different countries maintain the independence from each other, and at the same
time hold the options of considerations of the judgments of the other courts.

This example tells us more. Toulmin argues that the boundaries
between jurisdictions are normally clear, sharp, and agreed upon. If it would
not have been so, a rival court might claim judicial authority over the same
territory or case. It would damage the authority of both courts. Secondly, within
the common law tradition earlier decision may always be called up for
reconsideration, or may be cited as bearing on a case presently before the court.
Thus highest courts have a standing right to reconsider decisions arrived at in
any earlier decade. Even they can declare the judges responsible for some
previous decisions ‘mistaken’—however well that ruling was supported by
arguments accepted at the time. Ancient rulings and decisions from remote
jurisdictions are scrutinized with particular care, before being accepted as
bearing in a case to be decided here and now. A decent respect for judicial
relativity never plunges the courts into mere judicial relativism. On the
contrary, the judicial experience of all mankind is kept available as reserve, on
which the court can call—with due attention to historical and cultural
differences—in arriving at a just resolution of current cases.20

The method of science is much more like that of common law tradition.
In practice, physical scientists are accustomed to drawing a similar subtle yet
fundamental distinction between relativity and relativism. They are compelled
to acknowledge the relativity of the judgment and measurement. They acknowledge it not just as an abstract conundrum, but also as a substantive difficulty arising in their own professional work. Therefore, they have developed procedures with a view to do justice to this relativity, without falling into the trap of outright relativism. From this point of view, Toulmin says that Einstein’s theory of relativity—every thing is relative—is entirely misconceived. The intellectual strategy of relativistic physics is quite contrary to relativism. Its method is like judicial court. Einstein began by recognizing that ‘measurements of spatio-temporal magnitude’ involve a hitherto unsuspected relativity to the choice of ‘reference frame’ or ‘reference object’. They are made and considered relative to, say, the surface of the earth or a free falling elevator or a high-energy particle or a distant galaxy. If we understand it as involving an outright relativism we should then have to dismiss physical comparisons between frames of references as meaningless, which was not the contention or conclusion of Einsteins. On the contrary, he set out to establish general impartial procedures for making just these comparisons. Einstein was, after all, a physicist. He could never have agreed to treat spatio-temporal measurements as comparable only within a single reference system, for that would have meant abandoning his fundamental obligations as a natural philosopher. Instead, he worked out new equations for catering to the needs of one-frame measurements originally made relative to different reference frames. As a result, a spatio-temporal judgment made in different frames could be safely transformed without running into theoretical difficulties. With the actual content of our spatio-temporal judgment depending upon a particular frame, rational procedures were thus devised for comparing measurements relative to different frames. This particular example of Einstein’s work like the every-day procedures of the law, should encourage us rather to consider what rational procedures will circumvent the every real practical difficulties posed by differences in cultural and historical milieus. This is the characteristic of universality. Noam Chomsky has claimed that all human thought and language display certain universality, continuity, or patterns of grammatical structures.

The professional careers of many theoretical physicists spanned the years from 1890 to 1930 through the change over in question. Toulmin argues
that if there had, in fact, been any breakdown of communication by an authentic scientific revolution, we should be able to document it from the testimony of these physicists. In addition to this, the total effect of 450 years (from fourteenth to eighteenth century) of conceptual change in physics and astronomy would make it hard for physicists to understand each other's questions. The gulf between their theoretical positions was not rationally unbridgeable. We cannot say that Copernicus and Galileo, Kepler and Newton were the authors of a totally new and all-embracing paradigm. We cannot say that their novel world picture snapped all intellectual connections with the physicists of earlier times. If the men of sixteenth and seventeenth centuries changed their minds about the structure of the planetary system; they were not forced, motivated, or cajoled into doing so—they were driven to do so. In a word, they did not have to be converted to Copernican astronomy; the arguments were there to convince them. Toulmin also argues that today, political scientists try to avoid exaggerating the contrasts between 'normal change' and 'revolution'. Even the most unconstitutional change does not involve absolute and comprehensive breach of political continuity. In geology, Lyell's uniform causes had become sufficiently drastic, and Agassiz's catastrophes had become sufficiently uniform—the original criterion of telling normal from catastrophic changes had disappeared. So, instead of a revolutionary account of intellectual change, which sets out to show how entire 'conceptual systems' succeed one another, we therefore need to construct an evolutionary account, which explains how 'conceptual populations' came to be progressively transformed. Evolution rather than revolution maintains both the time factor and continuity over the change.

7.2. Scientific Progress in the Process of Evolution

Organismic Development

Now we will see how the process of evolution occurs in science. Toulmin illustrates that we can make a distinction that results from the shift of rationality from 'logical system' to 'procedure': the first is a 'theoretical principle' and the second a 'disciplinary one'. There are basic theoretical concepts and principles in the science, such as, Newton's principle of universal gravitation, or Mendel's genetic principle of segregation and combination, etc.
Also there are the disciplinary principles, such as ‘all physiological functions are to be explained in chemical terms’ — which defines the basic intellectual goals of a science, and gives it a recognizable unity and continuity i.e. identity of that science. The first sort of principle may change with time that is the changeable aspect of the science. The second principle is a continuous aspect of that science which remains identical through change. These two together make an organismic development. Toulmin says that in organismic development an individual passes through different stages of life, say from childhood to boyhood to manhood and so on, but at the same time, the particular individual remains one and the same. John remains John though he negotiates many twists and turns in the course of his life. When two scientific positions share similar intellectual aims and fall within the scope of the same discipline, then the historical transition between them can always be rationally discussed. It can be done even in the absence of common theoretical concepts between their respective supporters. Given the very minimum continuity of disciplinary aims, presuppositions will remain commensurable because both have a common set of ‘disciplinary task’.25

There is another distinction between ‘propositional system’ and ‘conceptual population’. Propositional system is a complete body of scientific theory (e.g. Newtonian physics), which constitutes a single, coherent logical system, which must be accepted or rejected in its entirety. On the other hand, conceptual population is something in which we can make radical changes piecemeal. While, history of science testifies that no scientific theory has been rejected in its entirety, in populational change any individual may or may not survive. There are innovations and selections over periods in populations. This process has something ‘biological’ about it. So this process of conceptual change substantially resembles the process of organic change—the process of evolution. Evolutionary change that is seen quite obviously in zoological realm can also be traced in our conceptual development. Toulmin shows that biological evolutionary change has four main theses and each one of them has a counterpart in the realm of conceptual change. These are stated in Toulmin’s book ‘Human Understanding’.26

Firstly, evolutionary change is concerned with the question: why there are recognizable organic species. This question is much like why species change as they do. How the species instead of loosing their initial distinctness.
can be transformed into other. Until nineteenth century, organic species had been thought of in two different ways, neither of which is historical. One view was that total population of living creatures can be divided into distinct and separate kinds, each of which maintains itself unchanged by producing offspring of the same kind. This view is called zoological realism. Another view was that living things are classified into species only by our own arbitrary intellectual decisions. In reality, there exist nothing but individuals. This view is called nominalism.

Secondly, continuity and change can be explained in terms of a dual process of ‘variation and selective perpetuation’. There are variant forms or features of individuals of a certain population, only some of which are transmitted to the subsequent population. If the variants are disadvantageous, they are not naturally selected and thus the generation remains stable in its character. If the variants are advantageous to an organic population, they are selected by nature, registering slow changes in its over all character.

Thirdly, a novel variation can demonstrate its advantages only in a situation which involves much of the ‘selective pressure’. Failing serious competition, individual variants have no chance to outreproduce their rivals and the species will gradually lose its distinct character. Again, natural selection can be effective, only where the ‘forum of competition’ is not too extensive. If animals and birds interbreed freely over a large area, advantageous variants within one particular locality will be swamped by cross-breeding over the larger area, in the process, failing to establish themselves permanently even within that favorable locality.

Fourthly, variants are selectively perpetuated if and only if they are sufficiently ‘well-adapted’. The word ‘adaptation’ simply refers to the effectiveness with which different variants cope with the ‘ecological demands’ of the particular environment. The term ‘demand’ itself embraces both physical conditions of life (climate, soil, and terrain) and other coexisting populations of living creatures (predators, prey, shade-plants, camouflage, parasites, intestinal flora, etc.).
Now, Toulmin contends that these characters of evolution are applicable to the conceptual development as well.

Firstly, Intellectual enterprises are not an unordered continuum. They are separate and well-defined ‘discipline’ (species) by their own body of concepts, methods, and fundamental aims. The intellectual content of such a discipline may change drastically over a long period of time. This change may happen to the methods and aims also. Yet each discipline displays a recognizable continuity. The conceptual change like evolutionary change in biology has two aspects; (i) the coherence and continuity by which we identify a discipline as a distinct entity and (ii) profound long-term changes by which a discipline is transformed or superseded.

Secondly, the continuity and the change both involve a dual process: variations and selection. In any live discipline, intellectual novelties are always entering the current pool of ideas and techniques. Only a few of these novelties establish themselves in the relevant discipline and are subsequently transmitted to the next generation. Thus, the continuing innovation is balanced by the continuing critical selection. Yet, in suitable circumstances, a discipline either gets the continued stability or is transformed rapidly into something new and different.

Thirdly, this dual process can produce a marked conceptual change. With certain conditions, at any given time, there exist enough men of natural inventiveness and curiosity to make innovations and variations. The problematic question is as to on which conditions such a novelty can prove its ‘advantage’ and so win a place in the relevant discipline. Once again, there must exist suitable ‘forum of competition’ within which intellectual novelties can survive for long to show their conquests or defeats. Within such a forum, they are criticized and weeded out with enough severity to maintain the coherence of the discipline. The dual process of ‘variation’ and ‘selection’ works like Popper’s dialectical method of ‘conjecture’ and ‘refutation’. It lays down the ecological conditions that can lead to effective scientific change.

Fourthly, the selection process picks out those novelties which best meet the specific ‘demands’ of the local intellectual environment. These
demands comprise of both the immediate issues that each conceptual variant is designed to deal with, and other concepts with which it must coexist. The terms like ‘competition’, ‘merits’, ‘demands’, and ‘success’ – all are correlative notions that can be understood as aspects of the process of conceptual variation and disciplinary selection.

From this discussion, we can see that the evolutionary account of scientific advancement characteristically is like evolutionary account of organismic development. From evolutionary point of view, science may be regarded as a means used by the human species to adapt itself to the environment; to invade new environmental niches, and even to invent new ones. There are three levels of evolutionary adaptation: genetic adaptation: adaptive behavioral leaning, and scientific discovery. These three levels play their roles through variation and selection.

Toulmin further explains that intellectual disciplines comprise of historically developing populations of concepts, as organic species comprise of organisms. In the evolutionary change, there is interplay of innovation and selection; there occurs variations in the population and at the same time there remains a characteristic unity and continuity of the discipline. It is because the available ‘ecological niches’ impose a sufficient unity and continuity on the population, despite the continual diversification of individual organisms. At this point, a populational approach debars us from giving permanent definition of the resulting disciplines. Therefore, the same features are not absolutely and eternally constitutive of physics or chemistry at every stage of their development. Scientific disciplines like organic species are evolving historical entities rather than eternal beings. The works of Buridan and Galileo, Maxwell and Feyman and so many other successive contributions have not been a common commitment to a single, permanent and unchanging or essential physics. Rather it has simply been the characteristic unity and continuity, which their common intellectual enterprise has preserved, despite all its changes through the last hundred years. In Toulmin’s evolutionary model of conceptual change, the ‘population’ is comprised of the collection of concepts, methods and fundamental aims. His ‘species’ is comprised of more or less separate and well-defined ‘disciplines’, each characterized by its own
body of concepts, methods and fundamental aims. In view of the same, he claims that the pattern of evolutionary change applies to the process of conceptual change within scientific disciplines.

Conceptual Variation

‘Organic variation’ and ‘natural selection’ are the characteristic components of the process of evolution. However, here Toulmin shows that the two components occur in detail in scientific change also. He says that any discovery of new truth or concept is considered as a real ‘possibility’. Scientists treat some new proposal as a ‘possibility’, more often than not with an eye on some other problem or group of problems. Novel suggestions become ‘possibility’ only in so far as they might contribute to a ‘possible solution’ of the problems at hand. However, random variations alone will never lead to evolution—it must be inheritable ones. For if every new generation of (say) animals comprised a fresh batch of individuals with its own entirely independent characters, the process of natural selection would have nothing to work on. Likewise, to be genuine conceptual variations there have to exist a suitable professional ‘forum’ of discussion. For, conceptual change can proceed only where transient innovations do not automatically die with their creators. The disciplinary novelties are able to prove their worth, through the necessary available professional forums, and by the shared intellectual ambitions of the scientific community. Conversely, the absence of a suitable ‘forum’ may be fatal to the proper consideration of an intellectual variant. For instance, Mendel’s isolation from his fellow scientists made it difficult for them to recognize the significance of the problems.

Therefore, intellectual innovations, like biological ones, are also inheritable. It is obvious in western astronomy. The foundation of western astronomy is the rational approach to geometry originating in classical Greece. From Heraclitus up to Kepler, the ruling theoretical problems of astronomy were guided by the astronomy of Greece. In China, by contrast, geometry remained pragmatic rather than a logical network of abstract theorems. China did not inherit Greek geometry. So the internal answer to the question, ‘Why did China never have its Galileo?’ is, ‘Because it never had its Euclid’. Their innovations were Confucian rather than Platonic. Just like
human genealogy there is conceptual genealogy of any intellectual discipline as well. 31

He further describes the character of variations. He says that conceptual variation occurs in different sciences at different rates, at different times. Sciences do not all change so that nothing remains stable. They register changes by the shifting of the center of attention from one historical epoch to another. For example, the ideas of dynamics may be in rapid development, whereas the ideas of geology may not. Astrophysics may lose momentum, whereas chemistry may pick up speed; physiology may be more active than anatomy, or vice-versa. For Toulmin, the rapid change occurs in that field whose problems are ‘ripe’ enough for solution. Scientists face a lot of problems but all the problems cannot draw their full attention—many of them are ‘unripe’. There are periods in the history of science when scientists cannot get a satisfactory grip on its problems. It is because its subject matter may be so varied and complex that it defies a fully-fledged analysis. An another reason may be that the more general concepts needed to introduce some intellectual order may still be lacking. Thus the central problems of physiology become ‘ripe’ only after a general system of chemistry had been developed. The solution of the outstanding problems may demand mathematical methods, instruments, or experimental techniques that do not yet exist, and so on. For these reasons a theoretical problem may be ‘unripe’. An unripe problem thus takes time to be ripe, and then becomes the real possibility for new suggestions. Toulmin says, scientists are like farmers. They do not waste their energies in unprofitable operations and are careful to time their activities to the immediate demands of the task in hand. This is why the problems that are readily soluble attract their attention more readily than those that are not so. Petti Medawar says that science is ‘the art of the soluble’. 32

According to Toulmin, the rate of the intellectual innovations accelerates not only for ripeness of the problems but also for the pattern of opportunity and social demand. The protest of Chinese people against western medicine paves the way for acupuncture. Sometimes fundamental innovations have sprung up from quite surprising sources. The theocratic society of
ancient Babylon provided a niche for major achievements in astronomy so much so that all subsequent physics and astronomy have been built up on the foundation of Babylonian astronomical computation. Sometimes the social context inspires the innovations. For example, in nineteenth century France, scientific physiology was a by-product of the hospitals, in Germany it was supported by higher education.\textsuperscript{33}

Therefore, the conceptual variation is a function of two factors: reasons and causes. The reasons constitute the rational process for the evolution of intellectual novelties or conceptual variations; and the causes are those various social and other factors, such as the intellectual politics of disciplines which sometimes override reason. The causes are an important force in shaping the accepted intellectual content of a discipline. The reasons correspond to the sort of the factors studied by internal history and causes concern the sort of factors studied by external history. Thus, an adequate model of a particular discipline’s course of development is to be understood in the combination of both internal and external history.\textsuperscript{34}

However, various extrinsic factors as well as intrinsic ones motivate scientific innovations. So there hardly exist authentic sciences. Toulmin holds that there are no such broader conditions within which men’s reflective curiosity will give rise to authentic sciences. There hardly exists critically controlled speculation about nature. Taking human history as a whole, heresy-hunting or intellectual conformism has been the rule. Political and ecclesiastical authorities have rarely been happy with the men who scrutinize the intellectual foundations of their conceptual inheritance with complete critical freedom. Those authorities suffer from an ideological fear that the stability of that inheritance might be put at risk. Thus, social factors limit the occasions and incentives for intellectual innovations. For Toulmin, sometimes extrinsic need and intrinsic promise coincide. Then the effort of the two sides is to favor similar lines of research. At another time, the two groups may work against each other. Then even the most intellectually promising lines of abstract inquiry may be given a low priority. For these factors, intellectual innovations in any culture or epoch reflect the combined action of these two separate filters. So, the intellectual innovations can be explained fully neither in terms of social nor intellectual considerations alone.
At the same time, Toulmin shows that the social factors are necessary and the intellectual ones are crucial. If men are given chance to speculate freely and critically, they will find some aspects of their experiences, which are ripe enough for reflective attention. Thus, social factors sometimes provide the content for the intellectual consideration of scientists. Moreover, if the institutional, social, or ideological conditions are unfavorable, even the most outstanding problems may remain scandalously long unsolved. So opportunity and promise are here of equal significance. Given 100 soluble problems, scientists will naturally pay attention to those for which there is broader support and interest. Yet beyond a certain point they will be weary of fruitless inquiries, however useful, tantalizing, or well-financed. In our innovations, therefore, the intrinsic direction is primary and the extrinsic one is secondary.

These are the characteristics of the conceptual variations that go on the lines of individual variations in biological evolution.

**Intellectual Selection**

According to Toulmin, when we ask why a particular conceptual innovation succeeded in winning a place in science, two alternative sides of that ‘why’ present themselves straightway. One side of this ‘why’ is: what considerations would have been advanced by the scientists concerned to justify accepting the particular conceptual change they did. On this interpretation, the ‘why’ is a request for reasons, and the reply must be given in intellectual, disciplinary terms. Another side of ‘why’ is: why a particular conceptual change took the form it did. Here we must begin interpreting the question in a different sense. This question now becomes one that is not about the justificatory reasons but about the scientists themselves. Different questions may arise regarding the scientists: how did they come to stop using such and such concept in the absence of any specific justification for dropping it from their repertory?, what causes led them to do so?, did they just forget about it?, etc.

For Toulmin, conceptual change (or intellectual selection) in a highly developed natural science occurs in two contrasted ways. Sometimes it is
made knowingly as deliberate steps in problem-solving activity. As science is a ‘rational enterprise’, there is a standing assumption that changes have been made for reasons emanating out of intellectually relevant considerations. At another time, it happens in ways unrelated to this problem-solving function; it occurs as the effect of fashion, prejudice, or inadvertence. In this way, the changes have been the effects of (say) prejudice or inadvertence. In short, intellectual selection is made in two different ways; rational choice and causal effect.\(^{36}\)

Many conceptual changes within scientific disciplines take place as a result of choice. In such cases, Toulmin says that the selection of one particular conceptual innovation is justified by showing that it best succeeded in resolving an outstanding conceptual problem of the science and led to an appreciable increase in ‘explanatory power’. In order for this selection to be deemed appropriate, there must be general agreement about the character of the problem and about the increase of the explanatory power. Thus, the selection criteria for judging novelties have to be understood in relation to their specific explanatory aims and ideals. The following are among the explanatory aims and ideals: to establish the explanatory procedure to cover hitherto anomalous phenomena, to make possible the unification of explanatory techniques from hitherto separate sciences, to resolve inconsistencies between the concepts of a special science and related extra-scientific concepts. If any innovation achieves one of these aims, scientific enterprise makes a ‘selection’ for it. Conversely, if the advantages of the proposed variation are minimal, or are counterbalanced by other disadvantages from which current concepts are free, or if its extra-scientific consequences would saddle us with new and terrible paradoxes, or if some rival innovation has strikingly greater disciplinary advantages; then scientific enterprise will not go in for ‘selection’ of that variation.\(^{37}\)

Therefore, all variations cannot become selected and contribute to the change through evolutionary process. According to Toulmin, in this regard, three things are closely related to the ‘selection’. Firstly, evolutions are always a matter of comparison. The operative question is never of the form, ‘Is this concept uniquely ‘valid’ or ‘invalid’?’, nor of the form, ‘Is this concept ‘true’
or ‘false’? Instead, the operative form is; Given the current repertory of concepts and available variations, would this particular variation improve our explanatory power more than its rivals would? To be more ‘explanatory’ means to be ‘relevant’ more exactly, more precisely, or in greater detail, and ‘applicable’ more generally, more extensively, or more unconditionally. Secondly, we do not have any formal standard in deciding whether a conceptual variation enables us to do a better explanatory job. So the merits of conceptual innovations must be characterized by the statements about rival theories—specifically by statements about the respective ways in which alternative theoretical change can help to fulfill the proper intellectual ambitions. Thirdly, conceptual problems and conceptual variants rarely match one another exactly. Even when a conceptual change is proposed for solving a specific problem, it will have intellectual side-effects. Sometimes those side-effects are a stronger witness for or against the innovation than its intended consequences. For instance, in regularizing the conceptual relations between the theories of electricity and magnetism, Maxwell created—almost inadvertently—a theory of radiation and radio waves. However, this theory embraces not only radiation and radio waves, but also the whole of existing physical optics; and this unforeseen by-product of his work testifies more convincingly on its behalf than his formal integration of electrical and magnetic theory.\(^{38}\)

For Toulmin, the recognized disciplinary criteria of choice (selection) are always multiple, and sometimes point in opposite direction. So, a proposed theoretical change may be highly attractive in one respect, and retrograde in another. Criteria of selection are thus informal ones. And these informal standards seek to have all explanatory virtues; universally applicable and completely coherent, predictively exact and comprehensive, notationally convenient, intuitively simple, mathematically elegant and straightforwardly computable. He says, which of the virtues scientists must choose between, depends on their actual practice. Initially, the Copernican description of the planetary orbits was physically more coherent and consistent than any description based on Ptolemaic geocentric principles; but it achieves that improvement only at the cost of simplicity. Toulmin says that even in a
straightforward situation scientists face a complex kind of accounting in selecting between conceptual variations. Any single conceptual innovation will normally improve our understanding in certain respects, and impair it in others. In this condition, it is up to the scientists concerned to decide when that improvement is worthwhile, in terms of a broader set of intellectual priorities. And it is therefore vain for us to search for any single index or measure which will indicate in all cases whether a conceptual change is to count as an improvement or not. Whatever criterion we pick out for analysis, there will always be other cases to which it does not apply; and several incommensurable considerations will be relevant to any particular choice. For him, unlike inductive logicians, our historical analysis treats scientific disciplines as comprising informal populations of logically independent concepts. Whereas the formal logicians judge the sciences of every epoch in the same a historical scale, our down-to-earth analysis demands only that the intellectual selections at any time should be adequate at the time. Like legal practice, the actual practice of science will decide which virtue would be the determiner for the intellectual selection of conceptual variations.

Yet the selective perpetuation of certain conceptual variants can be explained in ‘rational’ terms. The rationality here is as to how the successful innovation helped the scientists concerned to achieve their collective goals. The scientists working in the same discipline share agreed upon or sufficiently agreed upon conceptions of ‘explanation’. This is their common disciplinary strategy. This strategic consensus determines well-defined selection criteria for deciding between conceptual variants. They agree with these criteria because they believe that these criteria will fulfill the agreed intellectual goals of the science for the time being. Toulmin mentions some other cases that are entirely different from them. On the one hand, there are cases that involve failures of rationality; on the other hand there are cases which reflect changes in the very criteria of rationality. In the first case, conservatism, prejudice, lack of professional cohesion, political pressure etc. may frustrate the normal procedure of intellectual selection. As a result, the disciplinary merits of some new techniques of representation or methods of explanation may for the time being be disregarded, despite the fact that they could make themselves
evident—if given daylight and fair play. In the second case, by contrast, the historical development of intellectual disciplines gives rise to an intrinsic 'cloudiness'. This cloudiness springs directly from strategic disagreements between the scientists concerned. Given strategic disagreements, there will no longer be well-founded selection criteria on which all professionals in science are for the time being sufficiently agreed upon resulting in historical change.

In such a case, where the situation is intrinsically cloudy, scientists are obliged to reappraise the goals of theorizing and the standards of the judgments also. Toulmin puts the point in political terms saying that at this point ‘sovereign authority’ is under the discussion. There is no such formal demonstration that can conform to the accepted pattern as an authoritative solution. For, what is in question, is the authority of those very patterns. Such a situation has arisen in the recent physics about the status of quantum mechanical explanation. The question was: Can we continue to give quantum mechanics sovereign intellectual authority in our physical explanation? Although among the critics and defenders, it is a very big point of disagreement, it shows that this disagreement, then, calls for appeal not to the codified rubrics of an established authority, but to the world of ‘common law’. Released from commitment to any codified procedure, the disputants are compelled to discuss their disagreements in terms of ‘precedents’, ‘consequences’, and ‘public policy’. Now all the solid augments are informal and consequential ones. This is a consideration of an essentially historical kind. This special problem appeal for a special decision shows quite genuine parallel to the judicial problems and decisions arising in constitutional law when a ‘court of last resort’ reinterprets the provisions of a sovereign constitution. In this situation, judges are compelled to reanalyze the social functions of the law in its application to some novel historical situation. Toulmin says that the judges now have to take one step back, and reconsider the overall justice of the accepted legal principles and constitutional provisions. In the final judicial context, logic thus becomes the servant of those fundamental human purposes that are constitutive of the law itself. Here the theoretical jurisprudence and judicial practice are based on our developing understanding of the historical sociology of law. Judicial decisions at this
level can no longer be treated as ‘right’ or ‘correct’, yet such decisions are nonetheless rational for all that. The law should now develop in order to fulfill its most general ideals of equity, humanity, and security. The unity and coherence of a scientific discipline does not require that its intellectual ambitions should be eternal and unchanging—only that they should maintain a sufficient continuity.

According to Toulmin, on this deepest level, our criteria of conceptual choice become ‘subjective’. But this subjective judgment is not a matter of personal taste, nor stipulating scientific developments to be arbitrary products of human idiosyncrasies uncontrolled by external requirements. On the contrary, all the judgments represent the outcome of mankind’s accumulated experiences in dealing with problems raised by the corresponding aspects of the ‘external’ world. These judgments of experience are not any the less objective. Each judgment is directed to the same general and objective task of suggesting how our intellectual understanding of nature can best be improved. And the way, in which nature will actually respond to our attempts at understanding her, is something that goes beyond all human tastes. These judgments are arrived at not through the accumulation of bare ‘facts of nature’, but rather in the light of all our experiences in the enterprise of explaining such facts.

In other words, in such a case, to the extent, the evolution is rational, the reasoning consists of ‘broader agreements’ involving the comparison of alternative intellectual strategies, in the light of historical experience and precedences. This endows science with the requisite objectivity. This is not the objectivity of the empiricists and positivists which involves the matching of hypothesis against facts. Rather, the objectivity rests in the fact that our conceptual and strategic judgments are exposed to criticism in the light of experience. Then there occurs a rational bet between the alternative intellectual strategies which involves prospective estimates of the consequences expected from them.

However, Toulmin says that even though a scientific judgment may not be subjective, yet it recognizes the full ‘relativity’ of the concepts and standards accepted as authoritative for the time being in different milieus. The
actual issues in science may be authentically factual, but scientists in different periods and with different backgrounds may well end by tackling them in very different ways. Intellectual demands are absolutely identical everywhere, but environmental demands are absolutely identical throughout a particular living range. This diversity is confined not only to historical epochs or national styles, but we may find differences between the different research centers or schools, even in the same country at the same time. There are Cambridge geneticists and Edinburg geneticists, Columbian operant psychologists and Harvard operant psychologists, etc. There may well be substantial differences between the explanatory goals of different men working in the same discipline. Yet this diversity conforms the unity and continuity of a discipline, because the maintenance of a discipline is no more than the maintenance of a sufficient degree of collective agreement required for intellectual goals and disciplinary ambitions. By the word ‘sufficient’ is meant sufficient for the actual demands of the present situations.45

7.3 Evolutionary Change and the Problem of Realism

Intellectual Ideals

In the last section, we discussed how there occur theoretical variations and selections. In this section, we will discuss the ‘disciplinary principles’—how an intellectual discipline, like an organic species, holds the continuity through the individual conceptual variations and intellectual selections. We are seeking here the continuing element owing to which an academic discipline, say, atomic physics remains atomic physics despite registering multiple changes in all other respects. What is atomic physics that has been running around from 1890 through its heyday in the 1920s, up to its fragmentation into several successor sciences during the 1950s? The following discussion will provide us an insight into the question of realism with regard to the aim of science. We shall come to realize that science has no transcendental aim or superhuman purpose. It is what we think of science or make of science that determines the aim of science.

Stephen Toulmin examines some possible answers to this question. He says that if we define atomic physics in terms of some standard books, then
the recognition of ‘atomic physics’ in 1930 would amount to its denial in 1900 or 1950. For, within this long time, various disciplines have registered considerable intradisciplinary diversification as well as polarization. Rutherford himself could no longer think about atoms in the way his students or grandstudents went about defining or stipulating them. Again we may say that atomic physics is some shared commitments of the professionals to the proper concerns of the discipline. Although we may define the concepts and theories of the discipline in impersonal terms, these concerns are concerns of the people. In this way, we may be saying that a group of people with shared commitments to the subject constitutes the element of continuity in atomic Physics. If this is not proper answer to the question, what continuing element did atomic physics display over the whole period from 1900 to 1950?

Toulmin says that here we have to seek a continuing element rather than an invariant one, because the terminologies, theoretical models, and fundamental equations of atomic physics underwent several drastic changes during this period. The concepts such as ‘electron’ and ‘nucleus’ discussed by Heisenberg and Dirac during 1930s are far removed from the ones discussed by Thomson and Rutherford during 1900s. Toulmin, in the end, says that the continuing element of atomic physics is ‘problems’ faced by successive generations of atomic physicists. Thus the ideas that Bohr advanced to deal with his own ‘problems’ about atomic structure are best related to the points left over by his teacher Rutherford. Problems that Rutherford left unsolved, posed, in turn, the problems on which his won pupils had to work. In this way, the problems, around which the successive generations focus their attention despite all the changes in their actual concepts and techniques, are linked together as a continuous family tree. This continuity can be maintained even in serendipitous advancement. For, variations at the organic and the scientific levels are not generated in a vacuum or out of nothing; they are imposed on existing forms—existing genes on existing ideas—respectively. Serendipitous discovery guarantees not only independence in problem-solving, but also a continuity. A straightforward case of serendipity occurs when the theory yields an unexpected explanation for a known phenomenon or an unexpected solution for a known problem. It can be divided into two main classes: one.
intending to solve (explain) A, but solving (explaining) B instead and two, intending to solve (explain) A, and solving (explaining) B in addition to A.\textsuperscript{48}

If the same object produces different problems, then these will fall in different domains according to the differences between the problems. Any particular type of object will fall in biochemistry, for example, in so far as it is a topic for corresponding biochemical questions. The same type of object will fall within the several different sciences depending on what questions are raised about it. The behavior of muscle fiber, for example, can fall within the domains of biochemistry, electrophysiology, pathology, and thermodynamics; since questions about it can be asked from all the four points of view.\textsuperscript{49}

Now the question is as to what is a problem? According to Toulmin, a problem is the situation where our current ideas fall short of our intellectual ideals. In other words, problems arise where our ideas about the world are at variance either with nature or with one another. Problems are recognized by locating and specifying the intellectual gap between our current capacities and the explanatory ambitions defined by the scientific community’s current ideals of natural order. Therefore, the conceptual problems emerge from the comparison, not of ‘oppositions’ with observations’, but of ‘ideas’ with ‘experience’. In short,

\[
\text{Scientific problems} = \text{Explanatory Ideals} - \text{Current Capacities}.
\]

Therefore, two things are very crucial to understand the evolution of science: scientific problems and explanatory ideals. Scientific problems are the things for whose solution scientific attempts are set out; and explanatory ideals are the goals which all solutions of the problems try to arrive at. The aim of science is thus solving of the problems and reaching the intellectual ideals of the times. So this aim is essentially not any absolute, unchanging, universal one.

Toulmin also says that scientific problems and explanatory ideals are very closely related—both of them are understood in current human intellectual capacities. All these are varied in different epochs and cultures and among individuals and groups at the same time. These things always exist in human thinking and thus ensure continuity. But they themselves are
changeable in time. It is true that at every stage scientists' intellectual reach exceeds their grasp in some fundamental respect. Ideas of 'completeness' and 'perfection' are thus unrealizable. Throughout a period, the general conception of a 'complete explanation' imposes an intellectual unity on the discipline. This conception is called 'model of intelligibility' or 'Ideal of Natural Order'.

Atomic physics is an example as to how the gap between 'explanatory ideal' and 'current capacities' produces the problem. When Thomson and Rutherford invented atomic physics, their first achievement was not one of empirical observation or mathematical calculation, it was one of intellectual imagination. They imagined a novel conception that would provide a new scope for physical explanations even on the microscopic level, i.e., atomic substructure. This explanatory imagination emerges in a way that enlarges both the reasonable expectations of physicists and the rational demands by which their explanatory ambitions were directed. It follows that scientific imagination is a practical thing, because it is directed by the rational demands to explain some quite definite and identifiable phenomena. It is not conceived as a bare theoretical possibility in an intellectual void. Thomson's and Rutherford's intellectual ideal, though radically a new one, other physicists could accept this only by a corresponding effort of their imagination. Of course, there may well be some other conservative colleagues who would think the proposal of electron as a material object is some kind of practical joke.

Toulmin gives another example. The classical nineteenth century theory of matter has taken the ninety odd chemical elements as its ultimate level of analysis. At that time, no scientific opportunity existed for enquiring as to 'why' sodium vapor (say) should emit radiation in the yellow part of the spectrum, rather than elsewhere. The only available way of answering this question was: 'God alone knows'. Thomson, Rutherford, and Bohr removed this limitation. Their new explanatory ambitions carried the ultimate level of analysis below the atomic level. By conceiving of all chemical atoms as composed of common sub-atomic particles, they made it conceptually possible to treat them either as 'phenomena' or as 'problematic'.
However, if any discipline is to be identified, it is done only by matching natural phenomena against the intellectual template of the ideals. This is why, atomic physics remains atomic physics despite it having drastically transformed itself, at least, thrice from 1900 to 1950. The subject remains the same and continuous only because its intellectual ambitions for the natural phenomena remain the same through all the changes. Two things are very important here: experience of natural phenomena and the interpretation (intellectual template) of that experience. The first is discussed in impersonal terms that makes history of ideas and the second in human terms, which makes history of human activities. For Toulmin, in this way scientific activity incorporates both the objective and human parts of science. Science and its aims involve both their concepts and their men who conceive them. Therefore, the notion of discipline and its profession are correlative. There cannot be any single ideal about conceptual change applicable universally in all sciences at all times. Each effective discipline has had specific goals and ideals, which have determined its specific methods and structures; and the historical developments are the progressive refinements and clarifications of those goals and ideals. This refinement is the central activity that creates the occasions for suggesting and adopting new intellectual methods, procedures, and structures. Thus our cognitive apparatus is itself an evolutionary product on the cultural as well as organic level. However, says Toulmin, we must analyze the rational use of concepts within collective intellectual disciplines with an eye on that activity. This is the way how a discipline maintains its continuity. The concept transmits from one generation to the next by a process of ‘enculturation’. This process involves an apprenticeship by which certain explanatory skills are transferred from the senior generation to the junior. When an apprentice physicist learns the concept ‘energy’, he learns to do three things: (1) to perform the calculations, (2) to recognize the particular problems and situations, (3) to identify the empirical magnitudes. The intellectual transmit of a scientific discipline thus comprises of a particular constellation of explanatory procedures. What makes the science genuinely ‘rational’ is that the apprentice physicist or biologist learns not only how to explain phenomena by applying existing concepts, he learns also what is involved in criticizing those concepts and thus improving
their current content. So the transmission of the concepts and the creation of the new ones, aim to reach the intellectual ideals of the times, though there always will be the distance between current capacities and intellectual ideals.

**Equilibrium**

We know that scientific enterprise always seeks to reach its intellectual ideals. But we see, at the same time, that these intellectual ideals are also changeable over time. Then the question generally arises that what it is that is sought by the change of intellectual ideals over time. Here Piaget’s view is very much illuminating. He claims that ‘reason not only evolves but reason does not change without reason’. According to him, there is some direction or directional tendency in evolutionary change. This directional tendency is called ‘orthogenesis’ which works not only in biology but also in epistemic development. This orthogenesis is like a ‘press’ towards an ideal equilibrium between organism and environment, and between epistemic subject and epistemic object. So this view opposes the notion of randomness and chance, and says that evolution is not contingent but directional. Since evolutionary change is the process which is essentially different from the theory of justification, our hypothesis may be logically unjustified yet we do not arrive at it blindly. Our prior expectations and our general world picture yield algorithms or heuristics that guide us in explaining given data or in solving given problems. This is a very elaborate guiding apparatus.

Orthogenesis does not fall in teleological fatalism, because unlike teleological change this is a teleonomical one. While teleology is goal directional change controlling the future, teleonomy does not control the future. There is not any causal effect of the future acting on the present. According to Piaget, orthogenesis is neither a final cause nor an apriori agent directing evolution. So whatever validity orthogenesis has, it has only up to the level presently attained so that no future extrapolation is possible—no concrete predictions about the future are possible. One can no more predict the future evolution of the horse, and one cannot predict what theory of space-time will next appear. So the future development of cognition is ‘open’ to all possibilities consistent with the constraint that ‘reason does not change without reason’. But although future development is not predictable, it is
retrodictable—it can be rationally reconstructed after the happening of a fact.\textsuperscript{58} Science can allow itself to be swayed only by efficient causes.\textsuperscript{59}

Our intellectual selection from the different variants always seeks to make an equilibration between the intellectual ideals and current capacities by minimizing the distance between them. So in simple words, the aim of scientific evolution is to increase equilibrium. But the epistemic change can never provide us with the complete equilibrium—the ultimate picture of the reality. Because, there is always a gap between intellectual ideals and the current capacities. For this constant gap science becomes perpetually open and we will never have the final theory. There is only an increase of equilibrium instead of a complete equilibrium.\textsuperscript{60} Natural selections appear to design for maximum efficiency in the given environment, but a cognitive system of selection does not allow to appropriate all aspects of the apparently unlimited diversity and complexity of the universe.\textsuperscript{61} This is so for three reasons: (1) the object in itself has an infinite and indefinite number of properties or levels and we never know them all, (2) we only know an object by performing operations on it and when we do so, we alter the object, and (3) whenever any theory is constructed to solve certain problems and to explain certain phenomena, the theory itself produces new questions that remain unanswerable by this theory.\textsuperscript{62}

The equilibrium is like adaptation. In adapting to one’s environment, the organism is forced to accommodate itself, to change and modify itself as a result of the constraints inherent in the environmental features. At the same time, the organism must assimilate the environment into its structures. When there is a balance between these two processes of assimilation and accommodation, there is adaptation, and a state of equilibrium is, then, attained. Equilibrium involves, in an essential way, the role of actions (praxis), which constitutes transformation of one state into another. These actions are called ‘operations’. When an organism has attained the ability to perform operations, the organism is better adapted to its environment. Then it can be said to be in a better state of equilibrium with it. When some type of need arises, there is rupture of equilibrium between organism and environment. This state of disequilibrium is motivating the subsequent
attempts to restore equilibrium and to satisfy the need of organism. If a particular cognitive structure $S_i$ has a certain degree of equilibrium, there will be a tendency for $S_i$ to change into $S_j$, such that $S_j$ has a greater degree of equilibrium. It follows that if $S_j$ is not more equilibrated than $S_i$, there will be no change. It will change only because $S_j$ is more equilibrated. Therefore, the motive force behind all development is disequilibrium (inconsistency). A disequilibrated cognitive structure moves one to action in virtue of its intrinsic logical features.$^{63}$

If the equilibrium has been the aim of science, then evolutionary development is the development that occurs not on the world but on the organism and epistemic subject. So nature and development of knowledge depend on an organism’s capacity or disposition. Thus, our knowledge is about the organism that lives in the environment than about the world as it is.$^{64}$

However, this account of theories clearly is instrumentalistic. That is, theories are rules for drawing inferences, and are neither true nor false. They are ways of looking at phenomena which work or do not work, or are or are not fruitful.$^{65}$ There is a claim to ‘reality’ in evolution, but this claim is merely a part of the system of ideas, which is acceptable, for the time being, at any rate, as ‘absolute’ and ‘pleasing to the mind’.$^{66}$

To summarise, for Toulmin, science grows by way of evolutionary process. The standard of rationality of theory choice is neither static nor dynamic. Rather, a standard of rationality too evolves in the process of evolution. As opposed to revolution, the evolutionary process entails continuity and as opposed to accumulation, it entails both loss and gain. However, what is the goal of evolutionary process. Is it truth? Toulmin holds that nothing remains unchangeable in the face of evolution, and the concept of truth too undergoes the process of evolution. Therefore, scientific progress as conceived by Toulmin is not committed to truth.

For Toulmin theories are like an organism, disciplines are species and the intellectual milieu is like the environment. Then whatever evolutionary factors occur with the zoological organism, also occur with a
conceptual organism. Variation and selection which are the essential features of the structure of evolution are also seen in the conceptual variation and selection with the purpose of adaptation with current intellectual environment. Scientific disciplines like organic species are evolving historical entities rather than eternal beings. Since in evolutionary change a species gets increasingly better through its adaptation to the environment, scientific change also entails progress through its adaptation to the intellectual milieu of the times. So this kind of change entails progress.

In every historical epoch humans hold some scientific capacities to deal with nature. However, when the intellectual ideals or explanatory ideals go beyond their capacities, scientists feel problems. It means that problems are human. Therefore, the aim of science is also human, as against some transcendental truth. Now the question arises as to when is a problem solved? Toulmin says that like zoological organisms, scientists seek to obtain an equilibrium between organism and its environment, i.e., with our current capacities and the intellectual ideals. This is a kind of struggle for survival, struggle for getting equilibrium. Therefore, equilibrium, rather than truth, is the aim of science. When an organism has attained the ability to perform operations, the organism is better adapted to its environment. Then it can be said to be in a better state of equilibrium with it.
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Chapter – 8

Critical Evaluation

In the light of the formulations outlined so far, the following conclusion will be defended as the central finding of this thesis:

“Though science makes progress, this progress does not ensure us with the gradual attainment of truth. Such a progress results the increasing ability to solve the problems experienced by us with the world”.

The Traditional View of Science

A comprehensive understanding of scientific progress and the traditional problem of realism entails a recapitulation of the perspective on science. The traditional science was or was deemed and defined to be structured or central on method. It was deemed to be produced, directed and controlled by method. It was a methodocentric enterprise. The scientific method comprised of the four following steps:

1. The initial collection of all relevant facts.
2. The formulation of generalization that abstracts the common elements from the facts collected.
3. The deduction of particular consequences from the generalization, and
4. The testing of the particular consequences.

Traditionally, the scientists would go in for the formulation of a hypothesis with a view to account for the observed facts. From this general hypothesis they would deduce a particular conclusion or an as yet unobserved fact. The particular conclusion would then be tested. If the inferred fact is observed, the hypothesis would be said to have been confirmed. This view of scientific method is called inductivism. Inductive method is so much used in science that more often than not it is deemed to be synonymous with or used interchangeably with scientific method.

It is a hard-headed and straightforward account of science. Scientists are seen as piling up facts, generalizing them into laws, and again piling up more facts step by step in the laboratory with a view to exceedingly widen the
net of their generalizations. If you can infer the laws from the accumulated facts, you can deduce the facts again from the laws, and the content of the laws is nothing but the facts.

This view of science has been widely popular both in and outside academic circles. Following Philip Kitcher we may call this view ‘Legend’. It depicts scientific change as directed by noble goals, which goals have been increasingly successfully realized in the onward march of historical evolution of science. One of these noble goals of science has been the attainment of truth. Science ultimately aims at discovering the truth, the whole truth, and nothing but the truth about the world, or, at least, it is directed at discovering truth of those aspects of nature that impinge most directly upon us. According to ‘Legend’, successive generations of scientists have continuously and constantly filled in more and more parts of the ‘complete true story of the world’. There have been mistakes and false steps here and there but there is an overall trend towards the accumulation of truth, or at the very least, acceleration towards better and better approximation to truth.

This view advocates the supremacy of ‘Method’ in the ongoing march of scientific investigations and achievements. It registers the great debt we owe to the application of the ‘Method’ in the onset and upkeep of revolutionary scientific researches carried out by countless investigators. The ‘Method’ is so almost fool-proof that awakens us to all kinds of actual and possible cross while we engage ourselves in scientific research. The progress of scientific research is unfailingly backed by the method of science. There are objective canons of evaluation of scientific claims. Scientists have been tacitly aware of these canons and have applied them in assessing novel or controversial ideas. Methodologists should articulate the canons for helping to forestall possible misapplications to extend the scope of science into areas where human inquiry typically falters. Thus, science is a clearing up of rationality in the jungle of muddles, prejudices, and superstitions. Its practice is thoroughly informed by reasons both in obtaining truth and in lapsing into error. Scientific theories achieved through such a sacred method are glorified to correspond to the reality. And such correspondence is tested by the crucial experiment. Then scientists bow to whatever new finding exposes their errors.
Scientific method thus generates a logic of justification. That is, it provides the technique for the objective appraisal of the merits of scientific theories. The scientific method also includes a ‘logic of discovery’, which is to say that it provides devices to assist the scientists in the discovery of new theories. And in the noble pursuit of some of these worthy aims, scientists dispassionately and disinterestedly apply their tools and each application of which takes us a step nearer to the truth. The succession of theories constitutes progress and the achievements of earlier theories are retained in later theories. Thus scientific activities are uniformed, unhistorical, unprejudiced, neutral and innocent of human hopes and fears.

The above analysis of science reveals the following aspects of the traditional concept of scientific enterprise:

1. There is a unique method for science,
2. There is pure observation of facts by scientists,
3. Inductive generalization is the process of theory-formation,
4. Scientific knowledge is objective,
5. ‘Crucial experiment’ determines theory-choice,
6. There is justification of a true theory,
7. Theory corresponds to reality,
8. Science make progress,
9. The progress occurs through the accumulation of scientific facts,
10. The goal of the progress is truth,
11. The goal will once be attained.

The Present Understanding

The contemporary philosophy of science has registered or negotiated a paradigm-shift. The epistemology, logic and methodology of scientific discourse have almost taken a U-turn, in view of the radical accounts of science painstakingly assembled by historians of science. Historians of science, concentrating on the actual historical development of science, have exposed the methodologically unwarranted and unwarrantable quantum jumps taken by scientists at different points of space and time across
the evolution of science. The historical narrative of science assembled by various scholars and historians of science does not reveal that science is predominantly a method-controlled or method-guided enterprise. It may not be, methodologically speaking, a free-for-all. However, the legendary view of science is definitely not corroborated by various historical perspectives on scientific discourse. The present understanding of science is definitely more nuanced and sophisticated in comparison to traditional, ahistorical and uniformed view of scientific undertaking. The idea emerged from the analytic discussion of five different philosophers of science, leads us to new understanding of scientific achievements with regard to correspondence to reality, accumulation of facts, rational judgement of theory-choice, etc. Before making a critical judgement about these issues, we need to expose the brief account of them.

**Popper:** According to Karl Popper, a scientific theory can be produced from any source; regardless of myth, metaphysics, fairy tale, etc. only with the proviso that it must be falsifiable by a prescribed experiment. Such a theory is not the product of induction but rather of imaginative conjecture. A theory is not produced by abstraction from observations, because an observation itself can not operate without a theory. There is nothing simply given to us, uninterpreted. Again this conjectural theory cannot be verified, but rather falsified. For him, falsifiability is the sole criterion of empirical science. A conjectural theory due to its falsifiability negotiates various kinds of severe tests, which make the conjectures scientific and objective, even though they are originally a product of human imagination. The theory is empirical not when it is produced through conjecture, but when it is tested by observations. Observations may not produce a theory, but they do subject it to severe tests. For Popper, the test of a theory is always an attempt to show that the theory is mistaken. This is the ‘trial and error’ process of theory test, for through this process scientists trial to sort out error. This is also called method of falsification, for in this method we try to discover a mistake in a theory with a view to falsify or eliminate it. According to this method, every falsifying hypothesis is better than the
falsified one. So, the succession of scientific theories results in obvious progress.

However, every falsification means the elimination of mistakes. In other words, every falsification results in the decrease in falsity-content of our knowledge-claims which logically means the increase in the truth-content. Therefore, every succession of scientific change leads us to better approximation to truth. But this truth cannot be justified by positive evidence, that is to say, shown to be true. This truth is simply a regulative principle, for our scientific activities are conducted in the hope of getting nearer to truth, but actually we just get farther from the falsity rather than approaching truth in every step of theory-change. We are condemned to be agnostic with regard to truth. For Popper, ‘better approximation to truth’ is based on the assertion that one theory corresponds better to the facts than the other. But there is no such a thing as ‘degree of truth’ (better correspondence). So he maintains verisimilitude (truth-likeness) instead of truth in his account of scientific discourse.

Kuhn: According to Kuhn, paradigms are the fundamental scientific frameworks within which scientific research operates. A paradigm is a grand theory under which normal science registers normal growth or development. It includes laws, theories, applications and instrumentations; and also provides model as well as foundation for the ongoing practice of science. Men whose research is based on a shared paradigm are committed to the same rules and standards for scientific practice. A paradigm is a way of looking at the world. It signifies broad quasi metaphysical insights or hunches about how the phenomena in some domain should be explained. Therefore, all activities of normal science are determined by a paradigm. However, at some point of time, there emerges or arises some such a critical situation when no articulation of the paradigm can assimilate certain anomalous phenomena. If the awareness of such anomalies lasts long and consequently penetrates deep, the field-situation is seen to be in crisis. In such a situation scientists tend to loose their commitment to the older paradigm and try to work out some new paradigm which is expected to solve the crisis. Subsequently, the new paradigm is taken up by upcoming scientists with revolutionary fervour or
missionary zeal and a new framework for the resolution of scientific problems becomes the order of the day.

However, such a paradigm-shift is so radical that science after the revolution becomes absolutely different from which was practiced in pre-revolutionary times. Therefore, we can not have common denominator to make any comparison between the successive paradigms. This is paradigmatic incommensurability. In such a situation, the adherents of the new paradigm have to reject the achievements registered by the old paradigm. Therefore, for Kuhn, such a theory-change does not result in any progress. However, the question arises as to on what ground do scientists make their decision about theory-choice? Kuhn holds that the theory-change is not propelled by considerations of progress. The paradigm-shift is basically inspired by psychological reasons. For him, since paradigm is created by human mind which is just a vision about the world, it does not aim at discovering truth. Its merits are considered only as puzzle-solving competence rather than the attainment of truth.

*Lakatos:* According to Lakatos, the fundamental unit of scientific achievement is not an isolated hypothesis, but rather a research programme (RP). The research programme has a heuristics, that is, a powerful problem-solving machinery that digests anomalies and turns them into positive evidence. All RPs have one common characteristic; they all predict novel facts – facts which had been undreamt of or contradicted by the previous programme. A RP that does not discover novel facts, is a degenerating RP. On the other hand, any RP that continues to discover novel facts is a progressive RP. If we have two rival research programmes where one is progressive and other is degenerating, scientists tend to join the progressive one. This is the rationale of scientific revolutions. But this revolution does not occur instantly, because auxiliary hypothesis does explain the anomalies and also at the same time, continues to explain some more facts. A RP does not get rejected just with the emergence of a crucial experiment showing the counter-evidence. Rather every research programme is possessed of some degree of autonomy because of which a RP ignores the counter-evidence.
However, since in theory-succession scientists adhere to a progressive RP, every revolution results into progress. However, this progress does not ensure us with the attainment of or approximity to the truth. For, a RP is constructed with the purpose of discovering new facts, when facts do not necessarily correspond to reality. For Lakatos, what nature shows about the theory is not truth, but consistency. Truth must be consistent, but consistency may not be true.

**Laudan:** According to Laudan, the fundamental scientific unit is scientific research tradition (RT). A research tradition is a grand theory which embodies the models of appraisal and evaluation. This is the primary tool for understanding. RT provides the specific theories and specific theories touch the empirical problems with a view to solve them. If solution of the problem is the understanding of ‘why the problem is the way it is’, it is RT which provides all the components of such an understanding. A RT provides a set of guidelines for the development of specific theories. Parts of those guidelines constitutes an ontology and the models of procedure of research within that tradition.

For Laudan, science is an aim oriented enterprise. This aim is problem-solving. Therefore, problem-solving effectiveness is the rationality of theory-choice. Here progressiveness will determine rationality rather than rationality determining progressiveness. When a research tradition changes rationality of theory choice also changes. This amounts to saying that rational is progressive and progressive is rational. If any tradition is effective in solving the problems, then its rational to accept the theory. However, the progressiveness of science does not ensure us with the attainment of truth. For, science aims only at problem-solving and problem-solving does not entail the truth. The recognition of a problem and its solution are relative to the research tradition (milieu). A solution does not mean the truth.

**Toulmin:** According to Toulmin, scientific rationality lies neither in a single ‘logical system’ nor in a temporal sequence of such systems, but rather in the process of evolution. The process of evolution negotiates history or time and simultaneously ensures continuity in the development of science. Therefore, rationality in the growth of scientific concepts is to be understood
in terms of ‘process’ instead of ‘system’. For Toulmin, scientific theories are not a ‘propositional system’, but a ‘population of ideas’. A propositional system is a complete body of scientific theories constituting a single, coherent logical system, which must be accepted or rejected in its entirety. On the other hand, conceptual population is something in which we can make radical changes piecemeal. There are innovations and selections over periods in populations. Thus, it is an organic process. In organic development, an individual passes through different stages of life; say, from childhood to manhood and so on, but at the same time, the particular individual remains one and the same. In the same way ‘physics’ remains ‘physics’, although in its development there occur many variations and selections in its population of theories. However, such an evolutionary change ensures scientific progress.

For Toulmin, scientific problems are things for whose solution scientific attempts are set out. A problem is a situation where our current capacities fall short of our intellectual ideals. Intellectual ideals are goals which all attempted solutions of the problems try to arrive at. The aim of science is thus reaching the intellectual ideals of the time, through solving our problems. However, scientific problems and intellectual ideals are conceived in current human intellectual capacities. So, scientific progress is human, not real. This kind of progress aims at an equilibrium between human intellectual need and the nature. It has no commitment to the attainment of truth.

**Critical Comparison**

These philosophers of science develop their views of science from an understanding of the past scientific activities. Their understanding of science is based on their perusal of the actual history of science. Their success in developing a view of science depends solely on the successful explanation of historical facts of scientific development. However, it can be reasonably asked as to whether they do succeed in their attempts to explain the historical facts in their entirety. This question raises the issue of relative merits of their views. Therefore, we need to critically compare all these philosophers so as to get a better understanding of their positions regarding scientific progress and the problem of realism.
Popper Versus Kuhn: Karl R. Popper finds that there are some such theories in the history of science which are no less than metaphysical ones. Newton’s theory of gravitation and Einstein’s theory of relativity, for example, are among them. If verifiability is the criterion of meaning, these theories, due to their metaphysical character, become meaningless notwithstanding the fact that these theories have made great contribution to the development of science during the last three hundred years. However, this is totally unconvincing to say that scientific theories like these are meaningless discourse. Popper seeks to establish the meaningfulness of such discourse and also to establish a relation of such metaphysical theories with truth in some sense or the other. For him, it is also unconvincing to maintain that scientific theories do not aim at reality of the world. However, Popper’s attempt at the accommodation of such metaphysical theories into meaningful discourse, made him to face another question as to whether scientific theories are objective or not.

Popper tried to meet all these challenges with his famous theory about science – theory of falsifiability. He declared that any kind of discourse whether metaphysics or fairy tale etc., could be meaningful scientific discourse if and only if they are falsifiable hypotheses. Falsifiability as against the positivistic verifiability is the criterion of meaning or criterion of scientificity. The theory of falsifiability also entails that anybody can criticizes a scientific theory with the help of his own falsifying hypothesis. However, the falsifying hypothesis must, in its turn, also be falsifiable. He also says that through falsification we eliminate falsity-content of a theory, which logically means the increase of its truth-content. So science aims at truth (if not attainment of truth). If there occurs any falsification, the falsified hypothesis is rejected and falsifying one is accepted. Thus, he explains, the phenomenon of scientific change or revolution through his method of falsifiability. In our opinion, he has successfully explained the following things:

a. Hypothetical discourse of science is meaningful.

b. Scientific knowledge is objective.
c. Science aims at truth.

d. The theory-choice is rational.

Thomas Kuhn agrees with Popper and recognizes the fact that scientific theories at their foundations are a part of metaphysical discourse. Kuhn’s paradigm is very akin to Popper’s conjectural hypothesis. Kuhn also agrees with Popper that scientific change occurs through a revolution. Kuhn’s paradigm-shift, and Popper’s falsification both entail revolution. Their recognition of revolution shows that both of them are opposed to cumulative theory of scientific progress.

However, Kuhn as opposed to Popper, discovers sociopsychical elements in theory choice. Kuhn forcefully put forward the thesis that a theory-choice does not depend upon rational criteria. Such a choice is basically dictated by sociopsychical or non-rational and sometimes irrational factors. Therefore, a theory-choice cannot be explained on objective grounds. A scientific paradigm never aims at truth. The paradigms of science that emerge from time to time due to multiple revolutionary factors, are not mutually commensurable. The paradigmatic incommensurability is at the heart of Kuhnian project. Furthermore, the paradigms at their own place and time are neither objective nor rational nor true. Thus, while Popper tries to safeguard the objectivity, rationality and truth of scientific discourse and theory-choice, Kuhn discovers the sociopsychical, human and communitarian roots of scientific enterprise. Both Popper and Kuhn do agree to a certain degree with regard to ‘revolution’, ‘hypotheticality, etc. However, regarding rationality or incommensurability they become so radically opposite that acceptance of one’s view amounts to the denial of other’s. In our opinion, while Popper fails to appreciate the role of sociopsychical elements in theory-choice, Kuhn’s declaration of the entire scientific project being irrational is also not convincing. However, Kuhn makes us aware of the following significant facts:

a. There are sociopsychical elements in science.

b. There is incommensurability problem in science.

c. Science aims at puzzle-solving, not truth.
Lakatos and Popper-Kuhn: Lakatos agrees with Popper that theory choice is rational. Popper maintains that if we choose a theory with 'greater truth-content and lesser falsity-content' in preference to another theory, our theory-choice is rational. Lakatos holds that the theory-choice can be rational. However, his criterion of rationality is different from that of Popper. He maintains that it is rational to choose a theory with 'progressive problem-shift' in preference to a theory with 'degenerating problem-shift'. It is rational, for Popper, to accept falsifying hypothesis to the rejection of falsified hypothesis. It is rational, for Lakatos, to accept a research programme which progressively discovers novel facts, to the rejection of the research programme which is degenerating in discovering the same. However, recognition of rationality by Lakatos goes against Kuhn's position on irrationality of science. Lakatos, as opposed to Kuhn, says that scientists cannot be irrational. However, Lakatos establishes scientific rationality by endorsing 'fact - discovering' as the aim of science. And he says that facts do not necessarily entail the truth of the world. So, he agrees with Popper in recognizing the rationality of scientific theory-choice. However, this admission of the rationality of theory-choice exacts its own cost. Lakatos denies the truth of scientific theoretical discourse. In admitting the rationality of theory-choice, he is akin to Popper and in rejecting the truth of scientific discourse he is in accord with Kuhn. For Lakatos, there is no crucial experiment which falsifies a theory and consequently brings about an instant revolution. Lakatos, while surveying the evolution of scientific discourse, finds that emergence of counter-evidence has not, historically speaking, meant instant rejection of a scientific theory. Scientists can put up an almost dogmatic fight in defense of a cherished theory of their's. However, this historical dimension of theory-change is missed in the philosophical account of science worked out by Popper. Newtonian mechanics was rejected after a century or so, despite facing continuous counter-evidence. This is the relative autonomy of RP. This steady revolution, as opposed to instant revolution, is accepted by Kuhn as well. However, he explains it by recourse to psychological decision of a community of scientists. Lakatos does not see any role of psychological factors in the process of theory-change. For him, theory-change is a rational choice. However, the change may occur slowly and
steadily within the relative autonomy of RP. Both Kuhn and Lakatos see the
historical reality that all scientific achievements are not of the same level. So
Kuhn makes a distinction between extraordinary and normal science, where
paradigm belongs to extraordinary science. Lakatos also speaks of hard core
hypotheses and auxiliary ones, where the former gives heuristic to the latter.
But he, as opposed to Kuhn, does not see both kinds of hypotheses separately,
but rather as a series of theories producing a research programme. In addition
to Popper and Kuhn, Lakatos makes us aware of the following additional
aspects of the history of science.

a. A research programme is a relatively autonomous programme.
b. A scientific revolution is understood retrospectively.
c. Science aims at facts, not truth (Popper) and not puzzle-solving (Kuhn)
d. Theories are not isolated, but programmized into an unit.

_Laudan and Kuhn-Lakatos:_ Like Kuhn, Laudan emphasizes that
there are ideological elements in science. His ‘research tradition’ is thus very
akin to Kuhn’s paradigm. Both paradigm and RT contain the ontology and
methodology which provide the mode of appraisal and guidance for specific
theories within the paradigm or RT. But there are two vital differences
between them. Paradigm aims at puzzle-solving, whereas ‘research tradition’
aims at problem-solving. Puzzle is that which must have a solution under the
shared-paradigm, whereas a problem may not have any solution under the RT.
However, at this point, Laudan, as against Popper, shifts the aim of science
from ‘truth’ to ‘problem-solving’. Both Kuhn and Laudan give up ‘truth’ as
the aim of science. The second difference between Kuhn and Laudan is that,
for Kuhn, there is no rational choice between paradigms, whereas RT changes
through rational decision. For Laudan, problem-solving ability is the rationale
of theory choice. It is rational to adhere to the theory which has greater
problem-solving ability. Popper, Lakatos and Laudan, all hold that scientific
change is rational. Although Lakatos’s RP and Laudan’s RT both maintain the
rationality thesis, yet RT is quite different from RP. Whereas, RP is a series of
both hard-core hypotheses and auxiliary ones. RT is not such a series.
Specific theories are not the constituents of RT. If RT is akin to a paradigm,
specific theories, as opposed to Lakatos, make normal science. However, the characteristic difference of Laudan from all other philosophers is that for him scientific theories are not constructed merely to solve the empirical problems of science. The questions pertaining to a world-view, value-system of science are also problems of science demanding resolution through the construction of a theory. The conceptual problems too are amenable to resolution by recourse to construction of a theory. Therefore, he declares that science is problem-oriented enterprise. When Lakatos says that progressive RP solves all the problems solved by an earlier RP and also solves some additional problems, he asserts a cumulative process of progress. But Laudan says that there are many historical facts indicating that though a latter theory could not solve all the problems solved by the earlier one, the later was accepted to the rejection of the earlier simply on the basis of its greater problem-solving ability. This is explained by Laudan through the concept of 'rate of progress', which Lakatos fails to recognize. Laudan's view about science give us the following additional aspects in the context of history of science:

a. Science is problem-oriented enterprise, problems may be empirical or conceptual.

b. Scientific progress is non-cumulative.

c. Progressive is rational, rational is progressive.

_Toulmin and Rationalists-Irrationalists:_ For Stephen Toulmin, science negotiates an all-inclusive change. Change in science includes not only scientific theories, but also scientific method, standard of rationality, mode of appraisal, concept of truth, etc. Nothing is unchangeable in the course of time. At the same time, he also sees that through all changes there is a continuity over a period of time. To explain these facts, he developed a view of scientific change which we may call evolutionism. For him, rationalists and irrationalists both assume that standard of rationality is static. Thus, both the camps are equally confused. The standard of rationality negotiates change itself. Rationality is not static, but dynamic. The rationalists do argue rationality into a static mode. The irrationalists do aspire to static rationality but cannot find it out. Toulmin holds that reason does not change without
reason. He successfully recognizes what is neither seen by rationalists nor irrationalists. However, Laudan and Toulmin, both share the insight that rationality is immanent to scientific activity. But for Toulmin, as opposed to Lakatos and Kuhn scientific theories do not constitute a ‘propositional system’, but a ‘population of ideas’.

If a discipline is like nature (environment), theories in this discipline are a population of organisms. These organisms exist and develop in accordance with the Darwinian dictum ‘survival of the fittest’. Unlike all other philosophers, Toulmin recognizes satisfaction of human needs as the aim of science and tries to explain equilibrium between man and nature. When Lakatos says about progressive-problem shift and Laudan says about problem-solving ability, they certainly refer to problems encountered by man. They also recognized human needs, but it is Toulmin who laid appropriate emphasis upon this theme. It goes against Popper’s realist view that science aims at truth. For Toulmin, ‘concept of truth’ also undergoes the process of evolution, men accept that notion of truth which fulfils their intellectual needs. For him, neither revolution nor accumulation, but evolution is the pattern of scientific progress. However, his view of science gives the following aspects of science in the context of its historical evolution:

a. Scientific change is all-inclusive.
b. Reason does not change without reason.
c. Rationality is immanent to scientific activity.
d. Theories are populations of ideas.
e. Human needs are the aim of science.

Critical Evaluation of Scientific Progress

Science, as is traditionally understood, is a progressive enterprise. Unlike literary criticism or sociology - science does make progress through all of its activities. Every new theory discovers new facts. Successive generations of scientists have filled in more and more parts of the complete true story of the world. Despite occasional or even frequent mistakes science does inch along towards accumulation of truth. This progress is invariably achieved through the use of ‘scientific method’. All scientific claims have to pass
through the master of objective rules of evaluation provided by scientific method. The scientific practice is thoroughly informed by reason i.e. the contribution of each theory to scientific progress is determined by a crucial experiment. This approach of science to truth is monitored experimentally and methodologically.

However, our present study of the five philosophers of science leads us to doubt the traditional concept of scientific progress. All the five philosophers agree that science makes progress. However, they radically differ as to the pattern of scientific progress. Firstly, our present understanding goes against the concept of cumulative progress. For Popper, Kuhn, Laudan and Toulmin, accumulation cannot be deemed to be the pattern of progress. They all recognize the historical phenomenon of scientific revolution. Revolution, as opposed to accumulation, rejects the past achievements, and thus does not add to the piling of facts. According to Popper’s falsificationist account, an earlier theory is falsified or proved false by an ongoing revolution. Therefore, there is no question of accepting a false theory – scientists simply reject the old and accept the new. In such a revolution there is loss as well as gain. Kuhn too is radically against the concept of accumulation. For him, paradigms being incommensurable, change merely signifies shift of views which does not even mean progress let alone cumulative one. Popper accepts progress but denies accumulation, Kuhn denies both. For Popper, every falsifying hypothesis has greater truth content, though this content is different from earlier theories. In this theory succession the later theory is better than the earlier theory. But Kuhn says that we are not in a position to claim that defeating paradigm is better than the defeated one. Historical study reveals that in course of time even a defeated theory gets victorious and re-established. Heliocentrism is first propounded by Aristaracus, and subsequently defeated by Ptolemy. However, it outcompetes geocentric hypotheses in the astronomical explorations of Copernicus. Such historical facts make Popper’s falsificationist account of science untenable. In view of the same, we cannot claim one theory to be better than another. For Kuhn, there are no logical grounds to claim that the science necessarily makes progress. Celebration of scientific progress is more psychological and
sociological than logical. In such a methodological field-situation we are in a very different position to certify the superiority of theory which is the essence of progress.

For Lakatos, progress is cumulative. He says that every successive research programme solves the problems solved by the earlier one. However it solves some additional problems as well. This view is outright cumulative. But this view is challenged by Laudan. He cites examples from the history of science when later theories have not solved all the problems which were solved by the earlier ones. Even then the later theories were accepted in view of their greater problem-solving ability. Ptolemy’s theory is more effective in navigation than that of Copernicus till date. Toulmin, on the other hand, claims that scientific progress is not revolutionary but rather evolutionary which ensures the continuity of scientific progress. But it does not seem that Toulmin’s continuity tantamounts to accumulation of progress. The essence of accumulation is that the earlier achievements are preserved by the later development, which is not seen in the context of continuity. In evolutionary change, a child grows and passes through different stages of life and develops to the manhood, yet it is the same man despite all changes. It suggests accumulation, but, as a matter of fact, it is continuity. For, the man of 50 years does not preserve the characteristics of his childhood. So, it is the continuity which is conceived to the exclusion of accumulation.

One of the critical and crucial issues being debated in the contemporary philosophy of science is the question of the sense of progress. If scientific problem is not cumulative, how progress makes sense within the content of a revolution or theory-change. If scientific progress is not premised on accumulation of facts, should it signify the enrichment of scientific theory. Then, the question as to how one theory is better or superior to other also seems to be almost irresolvable. The question as to how one theory is better than the other has been variously responded to by our philosophers. For Popper, the successive theory is better because it falsifies the earlier – falsifier is better than falsified. For Laudan, the theory with greater problem-solving ability is better than that with less ability. For Lakatos, the theory with progressive problem-shift is better than that with degenerating problem-shift.
However, Kuhn here again strikes a discordant note. For him, the successive paradigm may become dominant over the earlier one, but dominance does not entail superiority. For him, paradigms are neither dominant nor defeated. It is the genius of a scientist or a group of scientists that either dominates for a certain period of time or is outcompeted at a particular point of time. If a group of imaginative scientists become committed to a defeated paradigm, it may once again bounce back and be a dominating frame of reference. So, the progress does not occur in science as such. It is the community of scientists who become dominant or at times get defeated. Such a progress or defeat is sociological, not logical.

Secondly, as our present understanding goes, scientific progress is not methodical. In other words, progress occurs not just by applying some unique, universal, objective canons which lead scientists step by step towards a better position. According to our present understanding, this method-guided account of science is untenable. Firstly, there are no pure observational facts on which traditional scientific method was deemed to be safely anchored. Feyerabend along with other philosophers of science, points out that experiences arise together with theoretical assumptions. If we can succeed in eliminating the theoretical knowledge of a sensing subject, then we will have a person who is completely disoriented and incapable of carrying out the simplest actions or observations. Two competent observers do not see the same thing while observing the multiple phenomenal features of the universe. Seeing is not only having of a visual experience; it is also the way in which the visual experience is had. If we see some fact we see that as our theory orientates us. All this means that there is no pure observation. If we are to make an initial collection of facts to form a theory, we at the same time are to have theoretical assumptions to get oriented about facts. This position leads us into a vicious circle like “which comes first? – hen or egg?” Contemporaneously the traditional glorified status of scientific method seems to be shaken to its foundation. Facts and theories seem to happen simultaneously. Facts do not precede theory as was traditionally postulated.

Traditional account of science assumed a theory or a hypothesis to be a function of inductive generalization. However, when we turn to
hypotheses involving theoretical statements and terms, our difficulties and problems get all the more accentuated. Theoretical terms have no direct referent in observation. Then how can we possibly arrive at a hypothesis about unseen or unseeable entity? Popper maintains that such a hypothesis is conjectural and produced by our imaginative mind. Kuhn also holds that there are discoveries involving a major reorientation in scientific convention resulting into a new paradigm. A paradigm is also a human creation. In the same token, Lakatos's 'research programme', Laudan's 'research tradition', and Toulmin's 'intellectual milieu', etc. all maintain that such theoretical discoveries or hypothetical formulations are not a function of inductive generalisations but rather creations of human mind.

In traditional scientific methodology, the role of 'crucial experiment' has been deemed decisive, rather all-important. Present philosophers of science argue that the 'centrality' of 'crucial experiment' is not being corroborated by the historical account of scientific evolution. Newtonian mechanics is a great instance in this regard. It was rejected after a long gap of a century despite observation of counter-evidence all along. Lakatos explains this fact by recourse to 'relative autonomy' of a research programme. He denied Popper's methodological falsificationism and propound what he termed as sophisticated falsificatioism – a view which rejects the role of crucial experiments. He says that crucial experiment may indicate the counter-evidence but it can never eventuate the rejection of the theory: A theory gets rejected only when it suffers from degenerating problem-shift (Lakatos), or if there emerges such a theory which has greater problem-solving ability (Laudan). So theory-choice is purposive, not backward-looking or methodical. In other words, purpose not method indicates the progress in the onward march of scientific change.

Furthermore, historical research shows that there is no single rule – however plausible and however firmly grounded in epistemology – that is not violated at some times or the other. According to Feyerabend, such violations are not accidental events; they are not results of insufficient knowledge or of inattention, which might have been avoided. On the contrary, we see that these violations are necessary for progress. The Copernican revolution, the
The rise of modern atonism and quantum theory, the gradual emergence of the
particle theory of light, etc., occurred because some thinkers either decided
not to be bound by certain obvious methodological rules, or because they
unwittingly broke them. What it amounts to is that scientists are not bound to
obey a certain absolute, unchanging set of rules for scientific practice.

Toulmin’s evolutionism about scientific progress also holds the
same thing. For him, scientific change is all-inclusive: method, mode of
appraisal, concept of truth, standard of rationality, scientific theories, etc.
everything changes. For Kuhn, when a paradigm shifts all the rules, methods,
standards, etc., shift according to the paradigm. Laudan’s ‘research tradition’
and Lakatos’s ‘research programme’ hold the same. We continually make
discoveries in science, and there is thus every reason to suppose that
methodological shifts or fluctuations should also inform or characterize the
entire spectrum of scientific programme. In view of the above submissions it
can safely be concluded that science does continuously register progress.
However, this progress is neither cumulative nor methodological. It is always
a function of a creative breakthrough.

**Critical Evaluation of Realism**

The problem of realism is an issue which is logically related to
scientific progress. If science makes progress, the obvious question arises as
to what is the goal of this forward motion. The traditional realistic account of
scientific progress amounts to saying that science, bit by bit, advances
towards truth of the world. Every theory-change results in a step forward
towards this noble goal.

However, before considering the realist claim about scientific
progress, we need to be sufficiently aware of realism itself. According to
scientific realism, the unobservable entities that theoretical terms indicate do
exist. To suppose that a theory is literally true would imply that no further
anomaly could, in principle, arise from any quarter in this regard. But can any
theory really ensure that further anomalies will not arise? Moreover, many
such theories were proved false and rejected by scientists. Realists are very
much aware of these things. So, they modified their position and held that
scientific theories are typically approximately true and theories that are more recent are closer to truth than older theories in the same domain. This view is called convergent realism. To be closer to truth, a successive theory should presume the theoretical relations and referents of earlier theories and also attain some additional measure of truth. But the history of science does not accord with this presumed direction of science. For instance, literally no one criticized the particle theory of light because it did not preserve the theoretical mechanisms of the earlier wave theory. Realists being aware of this also, held that up to a certain time a theory may be regarded as true and after a certain time the same theory may be regarded as false. This view is called tentative realism. The word ‘tentative’ means holding on; both holding on tightly as long as an idea works, and holding on loosely enough to be willing to let it go wherever the idea fails to work. There is another position of realists which is called constructive realism. According to this view, scientists construct theoretical models that tend to be at least partial representations of systems in the real world. The primary relationship between models and the world is not truth or correspondence or isomorphism but similarity. The term ‘constructive’ emphasizes the fact that models are deliberately created, or constructed by scientists.

However, the shifts of realists from scientific realism through convergent realism and tentative realism to constructive realism, indicate that truth as the goal of science is not easily a maintainable claim. The attainment of truth by successive scientific theories seems also to be an arduous project. For example, tentative realism does realize the impact of history and constructive realism does appreciate the role of human manipulation in the crystallization and formulation of scientific theories. In our opinion, these views are very much near to anti-realist position. Given the importance of sociological-psychological elements some philosophers of science view scientific theories as ‘conventions’ or ‘fictions’ leading respectively to conventionalist and fictionalist accounts of scientific discourses. According to conventionalism, basic assumptions of science are convenient definitions or conventions, which can not be validated either by a priori methods or by inductive generalizations from sense-experience. They are products of the free
activity of mind that are imposed by the scientific mind on our scientific schemes. According to fictionalism, scientific theories are fictions. They read like factual accounts, but they are products of the imagination and nothing else. They are like the narrative of a novel. The episodes of a novel can be coherent, and they may be quite convincing as well. They may have a ring of truth, but the fundamental truth about a novel is that the people who figure in it are not real people. The most lucid account of the anti-realist position is made by instrumentalist philosophers of science. They hold that thinking serves human purposes. Thinking is useful, and its utility is its truth. The human will is an instrument for realizing human aims. Theories are neither true nor false but instead more or less effective instruments in the hands of the students of the nature – instruments for predicting natural phenomena or for solving the problems that arise from the interrogation of the Nature. This view is supported by operationalist doctrine that meaning of a scientific statement is its operation or function in the economy of science. There is no meaning of scientific discourse in the semantic sense. They are neither true nor false.

In view of the above considerations with regard to realist-antirealist standpoints about scientific discourse, we need to bring out the positions of Popper, Kuhn, Lakatos, Laudan and Toulmin whose views on scientific progress and realism are embodied in the present thesis. We need to summarize their views on the truth or otherwise of the theories of science.

Popper, the proponent of the method of falsification maintains that scientific theories may be produced from any source whatsoever. It may be metaphysics, myth, fairy tale etc. However, when a theory is formulated, we need to go in for its justification. Our quest for justification of the theory may not yield us its truth. However, its falsification is possible and we can declare a theory to be false with the emergence of a single counter-evidence. The theory may be true but we cannot verify it.

Popper says that we seek highly informative theories by recourse to the method of trial and error. However, higher the information-content of a theory, the lower the probability of its truth. Or we may say the more informative the theory, the farther it is from truth. Then every successive theory which is more informative than its predecessor is exceedingly farther
from truth. So, in scientific progress, in our opinion, we get farther, rather than nearer to truth. Popper himself says that we may mistakenly falsify a true theory and corroborate a false theory. In this methodological endeavour what is our achievement? The net result is that it is impossible to verify the truth of scientific theories.

Realising that truth of scientific proposition is impossible of verification, Popper came up with the idea that scientific theories are characterized by “truth-likeness” or have ‘verisimilitude’ to truth. The greater the “truth-likeness” of a theory, the better it is. Even, at the highest point of scientific progress, the truth in all its glory and majesty will elude us. We will have to remain content with “truth-likeness”. This position is akin to constructive realism which asserts that the relation between models and the world is not truth, but similarity. Models are deliberately created by human mind to form a picture of reality. And we know that such a constructive realism is realism just only due to the fact that scientific theories aim at truth, regardless of its attainment.

Thomas Kuhn, unlike Popper is not at all prepared to accept even such a lenient version of realism. He finds scientific enterprise and progress deeply interlinked to social and psychological dynamics. For him, theories are constructed with the purpose of solving the puzzles. And the solution of the puzzles exclusively depends on the skill of the concerning community of scientists. A scientific paradigm does not aim to describe the reality of the world. After a revolution what we obtain is a new paradigm. Such a new paradigm is not dictated by logical or methodological factors. In the final analysis, the rejection or acceptance of a paradigm is a dictated by the collective but inexplicable wisdom of a scientific community.

Kuhn holds that the transfer of allegiance from one paradigm to another is a conversion-experience that cannot be performed on the basis of a method. Any scientist particularly whose productive career has committed him to the older tradition can put up a life-long resistance to an upcoming paradigm. Like the determination of a religious faith, he can not be accused of violating the standards, because there are no such standards to be violated. In this situation, psychological tricks, prejudices, passion, temperament, faith.
etc. help us to take the decision. Once converted to the new paradigm the competent scientists can explore its possibilities and improve its fortunes drastically. Thereafter, more and more scientists will be inclined to join the new paradigmatic bandwagon. However, this process of paradigm-shift indicates that fortunes of science are sociologically and psychologically oriented and a function of the collective competence of a scientific community. This position may be considered to be conventionalism as opposed to realism.

Lakatos, who rationalizes those elements which Kuhn sees as sociological-psychological, does not agree to Kuhn’s claim that the aim of science is socially or communally determined. For him, the aim of a research programme is to discover novel facts. Discovering novel facts as the goal of science is not social as Kuhn claims. He also asserts that the goal of science is not truth either, as Popper claims. For him, science aims at facts, but facts do not necessarily entail truth. For him, a theory may clash with a factual statement, but it does not necessitate the rejection of the theory. Moreover, a theory which is empirically falsified and actually rejected also, may again became victorious with its ‘progressive problem-shift’ in discovering facts. This realization about discovering facts points at instrumentalism which asserts that scientific theories are instruments for prediction.

Laudan’s view about scientific progress could also be deemed as instrumentalism. For him, scientific theories are constructed with the purpose of ‘problem-solving’ and acceptance of a theory depends on its problem-solving ability. So, theories are instruments for solving the problems that we face in doing science. Laudan emphasizes that solution of the problems does not entail the truth. For, our problems are designed with our worldview and conceptual schemes – the problems are not objective or substantive, but rather experiential. Moreover, a solution is also conceived by recourse to the same way. Both problems and their solutions are oriented by a research tradition which is very akin to Kuhn’s paradigm. For Laudan, research traditions should not be judged in terms of truth or falsity, for they are historical creatures. Since they are created for solving the problems, they have merits or demerits with regard to that purpose. They have no truth-value.
However, the historical aspect of science is considered of highest import by Toulmin. For him, science undergoes on all-inclusive change where neither any particular theory, nor methodology and not even the concept of truth either remain intact. Scientific progress occurs not for the sake of truth, but the concept of truth too evolves through the process of evolution. Again the evolution aims at equilibrium between intellectual ideals and our current intellectual capacity. So, science is involved in a struggle for survival, it is not oriented to the realization of truth. This view about scientific progress again supports instrumentalism, not realism. Theories are not descriptions of the real world, but instruments to help us survive. Equilibrium is a human need, not a characteristic of truth.

The above discussion brings out two opposite positions: realism which asserts that science, at least, aims at truth, and anti-realism which asserts that truth has nothing to do with science; the aim of science being producing better and better instruments for ‘predictions’ of the natural phenomena or ‘solution’ of the upcoming problems. Popper is generally considered to be belonging to the realist camp. The anti-realist group is adorned by Kuhn, Lakatos, Laudan and Toulmin. As already submitted, realists deny the role of psychosocial dynamics in the ongoing march of science, whereas the antirealists deem the entire scientific enterprise to be finally inspired by psychosocial or communitarian factors.

Popper admits that there is no logic of discovery. For him, a discovery is conjectural. It is a recognition of the fact that human creative mind actively participates in the process of discovery. However, he denies such a participation of human mind in the logic of justification. For him, we may not have a logic for discovering a new theory, but once a theory is discovered, we are, of course, able to test it through ‘criticism’. But in our opinion, critical activity too is a creative venture and starts and stops at the command of our attitudes and dispositions. It is a creative and participatory and not a detached and disinterested assessment of philosophical transcendentalist or objectivist. When Popper says that sometimes we may mistakenly falsify true theory and corroborate a false theory, he is unwittingly admitting the role of human mind even in the logic of justification.
The recent developments in the philosophy of science indicate that there is no distinction between the context of discovery and the context of justification. If Popper admits that there is no logic of discovery, and if there is no distinction between the context of discovery and the context of justification, then Popper will have to admit that there is no logic of justification either. In fact, both parts of scientific activity are oriented by human productive mind – neither is free from socio-psychological elements.

Furthermore, the argument can be buttressed by adding the perspectives of biology, genetic epistemology, cognitive psychology, sociology and anthropology. If scientists, like other people, are recognized as biological entities, who have evolved under selection pressures; if scientists, like other people, are cognitive systems with identifiable limitations and deficiencies; and if scientists are embedded in complex networks of social relationships; then chances for scientists being super-human purveyors of objective truth seem decreasingly low.

Finally, Heidegger’s perspective on human condition seems relevant in this regard. According to Heideggers’ analysis of ‘Dasein’ a human being is ‘Being-unto-the-world’ or ‘Being-there’. All human activities or projects are historical, temporal, cultural and linguistic. Man has no trans-historical, transcultural and translinguistic vantage point available for the articulation of his epistemic, hermeneutic, semantic and methodological concerns of themes. He has to be content with historical and cultural meanings and truths. Accordingly, Heidegger reinterprets science too as a human activity.

We think that all these considerations now provide us with more or less sufficient ground to declare that scientific activities are not free from sociological-psychological elements. The matter has become more serious with the discovery of ‘historicity of knowledge’, which holds that human cognition becomes structured by ‘time’. With all this awareness, we can solemnly declare that science is not a truth-seeking enterprise, and not any algorithmic activity either. Science is human activity. Human thinking like other human endowments, has an instrumental role. Therefore, a better scientific theory means a theory with greater ability to solve the problems we face within the domain of scientific research.
Concluding Remarks

The following remarks can be made with regard to scientific progress and the problem of realism:

1. Science makes progress in some sense or the other.
2. Science is not a truth-seeking enterprise.
3. A better theory means greater ability to solve problems.
4. There is no universal method for science.
5. The scientific theories are human products.
6. The scientific research is not free of ideology.
7. The science is not algorithmic activities, but creative ones.
8. There is freedom in scientific research also.
9. There is no justification but competition of theories.

These concluding remarks should not be confused by us in any way. Here we just want to say that scientific discourse as well as philosophy, art, etc. as human products are deemed to fail to construct a true description of the reality. So, when we say that ‘there is no truth’ or ‘there is no reality’, it means just only that there is no truth in scientific discourse (human discourse). We could not construct that description which corresponds to reality. If we understand by these remarks that there is no reality out there at all, it will lead us to the paradox: I think, yet I do not exist. So, there is reality, there is truth; but in so far as science is concerned, though it is not in a position to attain to that reality, despite its being progressive throughout its historical evolution.

Furthermore, there may not be the universal method of science. However, it should not be confused that there is no method necessary for science, and therefore we can do whatever we like to do. Rather when we say that ‘there is no method’, it means only that there is no unique, universal and unchanging method which is capable of turning the dross of the laboratory into the gold of theoretical truth. There are rules for guidance in science, but their role is no more than like the role of grammar in language. Sometimes, the articulation of grammatical rules and their successful application helps us to produce better understanding of language-use. Sometimes, our linguistic
practice brings out a change in grammatical rules. Likewise, methodological articulation helps scientists to get better understanding of science, and new scientific discovery brings out innovation in methodology also.

Furthermore, according to internal realism, the adherents of every framework believe that their own framework is true. Even when they abandon the previous one, they claim that the present one is true. But the recent developments in hermeneutics and deconstruction enlighten us that every linguistic framework is but our interpretation. Hermeneuticists and deconstructionists emphasise that all frameworks are interpretations and no framework can arrogate to itself the right of being a repository of truth. Secondly, all frameworks are devoid of any rational justification whatsoever. Acceptance or appropriation of such a view takes us one step farther from falsificationism. As opposed to verificationism, falsificationism maintains that we can not discover truth, but we can discover falsity. However, deconstructionists maintain that any justification of either truth or falsity is impossible of formulation. There is no transtheoretical or transcultural or translinguistic standard of justification. A falsificationist deems truth to be unjustifiable and falsity to be amenable to justification. Deconstructionists deny the justifiability of both. They hold that we are not in a position to assert either the truth or falsity of the fundamental scientific theories.
Bibliography


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