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Chapter 3

**Post kuhnian philosophers of science**

In this chapter, the author is going to discuss some post Kuhnian thinkers such as Imre Lakatos, Larry Lauden, C. R. Kordig, Mark. A. Stone, John watkin.

**Imre Lakatos:**

In the previous chapter, it has been explored that “meaning variance” occurs in the form of “paradigm shift”. Lakatos was not interested in question of meaning as such and for this reason he did not take up the challenges by the arguments for incommensurability. Rather he was particularly concerned with the question of how he could vindicate his principles of comparison (his methodology).

According to his methodology the great scientific achievements are research programs. The methodology of scientific research programs is to be used in making action guiding decision with
regard to theory choice in contemporary science.\textsuperscript{1} Lakatos says that research program can be evaluated in terms of progressive and degenerating problem shift and scientific revolution consists of one research program superseding another. But a research program is said to be progressive as long as its theoretical growth anticipates its empirical growth, that is, as long as it keeps predicting novel facts with some success. In other words, it can be said that within a research program a theory can only be eliminated by a better theory, by one which has excess empirical content over its predecessors. Though his methodology of research program was criticized by both Feyerabend and Kuhn. According to Kuhn, Lakatos must specify criteria which can be used at the time to distinguish a degenerative from a progressive research program and so on\textsuperscript{2} otherwise he (Lakatos) has told us nothing at all. Lakatos otherwise says that he begins his reworking of Kuhnian paradigm into
research program and sees the rationality of science in his scientific research programs. The relative merits of which can be compared and assessed. He defined research programs in terms of problem shift. Let \( T_1, T_2, T_3, \ldots \) be a series of theories where each subsequent theory results from the semantically reinterpretation of the previous theory in order to accommodate some anomaly, where each theory in the series has as much empirical content as the unrefuted content of its predecessor. The problem shift is said to be progressive if it is both theoretically and empirically progressive, otherwise it is degenerating.\(^3\)

In mature science he says, the series of theories are generated in accordance with research program having “heuristic power”.\(^4\) Such research programs consists of methodological rule for the development of problem shift; these rules comprise a negative heuristic that tells us what path of
research to avoid and positive heuristic tells us what path to pursue.\textsuperscript{5} We can say in other words that criteria of positive heuristic power strongly depends upon how it construct factual novelty. Further Lakatos says that his research program may be characterized by their hard core. The negative heuristic specifies the hard core of the program which is irrefutable by the methodological decision of its proponents while the positive heuristic consists of a articulated set of suggestion or hint regarding the development of research program.\textsuperscript{6}

According to Lakatos we are to compare theories by examining the track record of the scientific research program within which the theories are embedded in the hope that the past record is indicative of the future success rate. To do this we attempt to discover how successful the rival program has been in generating true novel predictions. A preliminary problem is that the explanation of a known fact can be as important in
providing evidence for a theory as the generation of true novel predictions. Lakatos further does argue that his account of science and his methodology of research program is superior to other because the rationale involves is effective to solve the anomalies. The famous dictum is that “philosophy of science without history of science is empty; history of science without philosophy of science is blind.” For Lakatos, the history of science gives philosophy of science its content through providing the test between the rival methodologies.

All methodologies function as historiographical or metaphistorical theories or research program and can be criticized by criticizing the rational historical reconstruction to which they lead.
That is we compare rival methodologies by comparing the different historical accounts (rational reconstruction) to which their use gives rise.

Progress in theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstructing of a growing bulk of value impregnated history as rational. Lakatos argues that his methodology is superior and appealing to other methodologies because it is supposed to be used by the scientist and it must bear relation to what they in fact regard as the good making feature of theories. He further says that it would be wrong to assume that one must stay with a research program until it has exhausted all its heuristic power, that one must not introduce a rival program before everybody agrees that the point of degeneration has probably
been reached. Although one can understand the irritation of a physicist when, in the middle of the progressive phase of a research program, he is confronted by a proliferation of vague metaphysical theories stimulating no empirical progress.\textsuperscript{10} One must never allow a research program to become a Weltanschauung or a sort of scientific rigor setting up itself as an arbiter between explanation and non explanation, as mathematical rigor sets itself up as an arbiter between proof and non proof. Lakatos reacts here and says that unfortunately this is the position, which Kuhn tends to advocate. Indeed what he calls normal science is nothing but a research program that has achieved monopoly. Thus Lakatos establishes that the history of science have been and should be a history of competing research program. He further makes question that, can there be any objective reason to reject a program, that is to eliminate its hard core for constructing protective belts?
The answer lies is that such an objective reason is provided by a rival research program which explains the previous success of its rival and supersedes it by a further display of heuristic power. However the criterion of heuristic power strongly depends on how we construe factual novelty. But the novelty of a factual proposition can frequently be seen only after a long period has elapsed.

All the Lakatos’ position suggests that we must not discard a budding research program simply because it has so far failed to overtake a powerful rival. We should not abandon it, if supposing its rival was not there, it would constitute a progressive problem shift. And we should regard a newly interpreted fact as a new fact.
References

1. Lakatos I. “The methodology of scientific research program” Cambridge Univ. press. Ed. J. Worall and Gregori Curri 1978a PP 89-90
3. Lakatos, I. “Falsification and methodology of scientific research program” Cambridge Univ. press 1970; p-118
4. Ibid. p-175
5. Ibid. p-132
5. Ibid. p-135
7. Lakatos. I. “The methodology of scientific research program” Cambridge Univ. press, 1978a; P-102
8. Ibid. p-122
9. Ibid. p-133
10. This is what must have irritated Newton most in the sceptical proliferation of theories by Cartesians.
Larry Laudan

So far we have seen the view of Lakatos related to the problem of meaning and how he outlined his research program in comparison to Kuhn's paradigm shift, here are the findings of Laudan that how he has emerged with a new account which was termed as Research tradition, a primary tool for understanding and appraising scientific progress.

Before I go into the details of Laudan's programs, I will see first how he looks the development made by both Kuhn and Lakatos.

As we have seen earlier, Kuhn offers a model of scientific progress whose primary element is the "paradigm". Once a paradigm is accepted by scientists, they can proceed with the process of "Paradigm articulation", known as normal science. In periods of normal science, the dominant paradigm will itself be regarded as unalterable and immune from criticism. It remains so until
enough anomalies accumulate, then scientists begin to ask whether the dominant paradigm is really appropriate. Kuhn calls this time a period of crisis. During a crisis, scientists begin to consider seriously alternative paradigms. If one of those prove to be more empirically successful than the former paradigm, a scientific revolution occurs, a new paradigm is enthroned, and another period of normal science ensues. Here Laudan says that despite of all its strengths, Kuhn’s model of scientific revolution suffers from acute conceptual and empirical difficulties. He then outlined the serious flaws in Kuhnian program, the most significant are below.

1. Kuhn never really resolves the crucial question of the relationship between a paradigm and its constituent’s theories. Do theories, once developed, justify the paradigm, or does the paradigm justify them? Laudan says, it is not clear in Kuhn’s case.
2. Kuhn’s paradigms have a rigidity of structure, which precludes them from evolving through the course of time in response to the weakness, and anomalies, which they generate. Moreover, he makes the core assumption of the paradigm; there can be no corrective relationship between the paradigm and the data. Accordingly, it is very difficult to square the inflexibility of Kuhnian paradigms with the historical fact.

3. Kuhn’s paradigm are always implicit, never fully articulated. As a result, it is difficult to understand how he can account for many theoretical controversies, which have occurred in the development of science.

4. Because paradigms are so implicit and can only be identified by pointing to their exemplars (basically an archetypal
application of a mathematical formulation to an experimental problem), it follows that whenever two scientists utilize the same example, they are, for Kuhn committed to the same paradigm. Such an approach ignores the fact that different scientists often utilize the same laws or exemplars. To this extent, analyzing, science in terms of paradigms is unlikely to reveal that it is a strong network of commitments - conceptual, theoretical, instrumental which Kuhn hoped.

In response to Kuhn’s assault on the traditional philosophy of science, Imre Lakatos has developed an alternative view about the progress in science and that is through research program. Laudan says that Lakatos’ model is in much respect an improvement on Kuhn. Lakatos discusses the historical importance of the coexistence of several alternative research
programs at the same time with in the same domain. Kuhn often takes the view that paradigms are incommensurable and thus not open to rational comparison. However Lakatos insists that we can objectively compare the relative progress of competing research programs. Lakatos’ research program, like Kuhn’s paradigms are rigid in their hard core structure. Moreover Laudan says that he is indebted pioneering work of Kuhn and Lakatos but tries to develop the notion of research tradition as an alternative model. According to him, a research tradition provides a set of guidelines for the development of specific theory. Laudan says that every research tradition will be associated with a series of specific theories, each of which is designed to particularize the ontology of the research tradition and to satisfy its methodology. The mechanistic research tradition in seventeenth century optics for example,
includes several of optical theories of Hooke, Rohault, and Huygens. Similarly the phlogiston tradition in eighteenth century chemistry received more than a dozen specific theoretical formulation. So the whole function of a research tradition is to provide us with the crucial tool we need for solving problem both empirical and conceptual.

There are at least two specific modes by which theories and research traditions are related: one is historical and other is conceptual. It is a matter of historical fact tat most of the major theories of science have emerged when the scientist who invented them was working with in one or another specific research tradition. For example Boyle’s theory of gases developed with in the framework of the mechanical understanding of the world. Hertz’s electrical theories were linked in important ways with the Maxwellian research tradition. Thus a specific
theory, abstracted from its historical context may not give unambiguous clues as to the research tradition with which it is associated. It is in the sense, the connection between a theory and a research tradition is as real as any fact of the past. In order to see how important these connections are, we need to look at the ways in with theories and research tradition interacts.

The problem determining role of the research tradition: Even before specific theories are formulated within a tradition, the research tradition will strongly influence the range. So the research traditions have a decisive influence on what can count as the range of possible conceptual problems which the theories in that tradition can generate.

Further Laudan says that the primary function of a research tradition is to establish a general ontology and methodology for tackling all the problems of a given domain or set of domains.\textsuperscript{5}
The research tradition can play a vital heuristic role in the construction of specific scientific theories. Consider the case of Benjamin Franklin and his efforts to articulate a theory of static electricity. Franklin was familiar with certain phenomena (particularly electrification by friction, electroscopes, and the leyden jar). Working within a research tradition which postulated the existence of electrical matter, Franklin needed a theory which could explain how friction electrifies bodies, how electrical bodies could attract and repel, how electricity could be stored in a condenser and why certain bodies were conductors others were insulators. In the early stages of the development of his theory, Franklin came to the view that the positive electrification consisted in the accumulation within bodies of an excess amount of the electrical fluid, while negative electrification was caused by a deficiency of this fluid. These specific
theoretical assumptions are linked together with the ontology of research tradition, an ontology which postulated that electricity was a form of matter and therefore conserved in the same way that ordinary matter was, it became natural to assume that electrical charge must be conserved. This important theoretical insight subsequently confirmed in Franklin experiment and emerged as an inevitable result of Franklin’s thinking about the relation between his theory and its parent research tradition.

Laudan also quotes another example. When Sadi Carnot set out to develop a theory of steam engines, he sought to do so with in the research tradition of caloric doctrine of heat. With in this tradition, heat was conceived as a material, conserved substance capable of moving between the constituent parts of microscopic bodies. Carnot, familiar with the work that could be performed by such simple mechanical system as a water wheel,
tried to conceive of heat flow on analogy with the fall of water, with the temperature gradient between input and output corresponding to the top and bottom heights of the water fall. It is in terms of this analogy that Carnot develops the proof of his theory. Thus it is clear here that, if Carnot had not conceived of heat as a conserved substance capable of flowing from one point to another without loss of its quality, he could not have enunciated his theory. But that way of conceiving heat was natural result of the research tradition with which Carnot worked. In both the cases above, the research tradition functions heuristically to suggest an initial theory for some domain. Research traditions, as we have seen are historical creatures. They are created and articulated with in a particular intellectual milieu. They aid in generation of specific theories. But Laudan says that there is another important way in which research traditions evolve.
These changes involve not the specific theories within the research tradition but a change of some of its most basic core elements. Laudan discusses this type of transformation in some detail since there are philosophers who have denied that research traditions are capable of any significant internal modification. For instance, both Kuhn and Lakatos usually suggest that entity such as research traditions have rigid and unchanging set of doctrines, which identify and define them. Any change in those doctrines produces a different research tradition. Laudan argues that we must reject it for it can create confusion in our effort to get some understanding of the historical processes of science. Laudan says that if one looks at the great research tradition in the history of scientific thought for example Aristotelianism, Cartesianism, Newtonianism, one can see that there is scarcely any interesting set of doctrines which
characterizes any one of these research tradition through out the whole of its history. Certain Aristotelian, at times, abandoned the Aristotelian doctrine that motion in a void is impossible. Certain Cartesians repudiated the Cartesian identification of matter and extension. Certain Newtonian abandoned the Newtonian demand that all matter has inertial mass. So the core assumptions of any given research tradition are continuously undergoing conceptual scrutiny. In such circumstances, it is common for partisans of a research tradition to explore what sort of changes can be made in the ontology of that research tradition to eliminate the anomalies and conceptual problems confronting its constituent theories?

Oftenly scientists find that by introducing one or two modification in the core assumption of the research tradition they can both solve the outstanding anomalies and conceptual
problems. For this research tradition must be carefully evaluated to come to its hope for achieving the goal (problem solving adequacy). Laudan gives two quite different contexts. One is the context of acceptance and another is the context of pursuit. In the former case, scientists often choose to accept one among a group of competing theories and research tradition. There is a whole range of possible answers here. Inductivists will say, choose the theory with the highest degree of confirmation or choose the theory with the highest utility. Falsificationists say, choose the theory with the greatest degree of falsifiability. Others such as Kuhn would say no rational choice could be made. But Laudan replies that choose the theory or research tradition with the highest problem solving adequacy. Thus Laudan establishes that the rationale for accepting or rejecting any theory is fundamentally based on the idea of problem solving. If one research tradition
has solved more important problems than its rivals have, then accepting that tradition is rational precisely to the degree we are aiming to progress.

This way of appraising research tradition according to Laudan has three distinct advantages. (1) It is workable unlike both inductivist and falsificationist model. (2) It simultaneously offers an account of rational acceptance. (3) It comes closer to being widely applicable to the actual history of science.

The context of pursuit; So far as we have seen an adequate account of theory choice, but according to Laudan we are still very far from pursuing a full account of rational appraisal. The research for this is that there are many important situations where scientists evaluate competing theories by criteria, which have nothing to do with the acceptability of the theories, in question. The actual occurrence of such situations
has often been observed. For example when we look
to Copernicanism, the early stages of the
mechanical philosophy, the atomic theory in the
first half of 19th century, the preliminary efforts
at the quantum mechanical approach to molecular
structure. We see the same pattern that scientists
often begin to pursue and to explore a new
research tradition long before its problem solving
success, qualifies it to be accepted over its
older rivals. Since the central aim of science is
to provide the solutions of a maximum number of
empirical problems and anomalies. This view
entails that we should accept at any time the
theories or research traditions, which have shown
themselves to be the most successful problem
solvers.

Suppose we have two competing research
traditions RT and RT' and the momentary adequacy of
RT is much higher than that of RT' but the rate of
progress of RT' is greater than the related value

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of RT. So far as acceptance is concerned, RT is clearly the only acceptable one of the pair but on the other hand $RT^1$ has shown itself to be capable of generating new solutions to problems. This would be appropriate if $RT^1$ is a relatively new research tradition.

Thus Laudan establishes that it is always rational to prove any research tradition which has a higher rate of progress than its rivals. So we see that rational persuitability of a research tradition is determined by the rate of progress it has exhibited. But what should be the guiding principles for rationality? Laudan has provided the following criteria:

1. In the case of Competing scientific research traditions, if one of those traditions is compatible with the most progressive worldview available, and the stir is not, then there are strong grounds for preferring the former.
2. If both (competing scientific research traditions) can be legitimated with reference to the same worldview, then the rational decision between them may be made on entirely scientific grounds.

3. If neither competing scientific research tradition is compatible with a progressive worldview, their proponents should either articulate a new progressive worldview which does justify them or develop a new research tradition which can be made compatible with the most progressive extant worldview.

So, instead of defining progress in terms of rationality, he defines rationality in terms of progress. Laudan’s expression has two main themes - that any adequate model of science must recognize and be able to accommodate scientific change and that rationality and progress are linked with the problem solving effectiveness of theories.
By taking into account of scientific change generated by revolutionary camp, (such as Kuhn, Feyrabend, and other) Laudan argues that their views leads them to conclude that the history of science is nothing but a succession of different world views and that rational change can never be made between such divergent schemes of the universe, because each has its own internal rationale and integrity, no meaning can be attached to the suggestion that one scheme is more or less rational than another.

If there are no conceivable grounds for rational choice between competing research tradition then science will bear unaccountability. This means that tradition, which happens to attract the most influential adherents, will become influential. But before one accept this depressing conclusion that science proceeds in
this way, it is worth examining with some core and the arguments.

The central argument runs like this: Scientific theories implicitly define the terms, which occur within them. Hence, if two theories are different then all the terms within them must have different meanings. Thus when an Einsteinian physicist refers to the “mass” of a particle, he means something different from a Newtonian when the latter refers to the “mass” of a particle. As a result, scientists working in different research traditions cannot communicate with, and cannot understand the statements of their fellow scientists in other tradition. Given this general in comprehensive, science emerging as a new version which employs that theories cannot be compared and rationally evaluated because such comparisons require a common language.

Laudan holds this above argument to be faulty in several respects. It bags a number of
questions about synonymy and translation. But its central flaw, for one purpose, lies in its presumption that rational choice can be made between theories only if those theories can be translated into one another’s language or into a third, “theory-neutral language”.

As Kuhn puts the point, “The comparison of two successive theories demands a language into which at least the empirical consequences of both can be translated without loss or change.

Laudan, on the contrary maintained that even if we accept the view that all observations are theory laden to a degree that makes their contents inseparable from the theory that is used to express them, it is still possible to outline machinery for objective rational comparison between competing scientific theories and research traditions. Laudan mentioned two arguments for such a conclusion.
1. The argument from problem solving: Logical positivists argued that competing theories could be evaluated by comparing their observational consequences.

They usually conceived correspondence rule for the process of translating the competing theories into some purely observational language because observational language was held to be frame of any speculative theoretical basis. It was thought to provide objective grounds for the empirical appraisal of vying theories. As doubts grew about the existence of correspondence rule and about the theory-true observational language, philosophers from revolutionary camp such as Kuhn, Feyrabend and others suggested that theories were incommensurable and not open to objective comparison.

But in comparison of the above two arguments (logical positivist and post positivists) Laudan holds another view and says
that neither correspondence rules nor a theory-
true observation language are necessary for com-
paring the empirical consequences of competing
theories. He says that without correspondence
rules and without a purely observational language
we can talk meaningfully about different theories
being about the same problem, even when the
specific characterization of that problem is
dependent upon many theoretical assumptions. If a
problem can be characterized only within the
language and the framework of a theory, which
purports to solve it, then clearly no competing
theory could be said to solve the same problem.
However, so long as the theoretical assumptions,
necessary to characterize the problem are
different from the theories, which attempt to
solve it, then it is possible to show that the
competing explanatory theories are addressing
themselves to the same problem. Consider a very
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, such as: light moves in a straight line, that certain obstacles can change the direction of a ray of light, that visible light does not continuously fill easy medium etc. Does the existence of these theoretical assumption entails that no two theories can be said to solve the problem of reflection? The answers is clearly, provided that theories which solve the problem are not inconsistent with those relatively low level theoretical assumptions required to state the problem. Laudan says that he does not mean to suggest that all the problems which a theory or research tradition attempts to solve can be characterized independently of the theory which
solves them. The determination of the independence of any specific problem must depend upon the particularities of the case.

He further says that there are far more problems common to competing research traditions than there are problems unique to a single one. These shared problems provide a basis for a rational appraisal of the relative problem solving effectiveness of competing research traditions.

Laudan criticized Kuhn and said that he has been misled by his discovery that some empirical problems are not jointly shared between traditions or paradigms into believing that no problems are identical.

2. The arguments from progress: It was observed that rationality consisted in accepting those research traditions which had the highest problems solving effectiveness. Now, an approximate, determination of the effectiveness of a research tradition can be made within the research
tradition itself, without reference to any other research tradition.

Laudan simply asks whether a research tradition has solved the problems which it set for itself, he further asks whether in the process, it generated any empirical anomalies or conceptual problems. In this way, Laudan says we can come up with a characterization of the progressiveness (or regressiveness) of the research tradition.

He says 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 the research traditions of a given time. It is thus possible at least in principle and perhaps eventually in practice to be able to compare the progressiveness of different research traditions. He says that, even if we could not in principle ever find a way of translating Newtonian Mechanics into relativistic mechanics; if we could never find a way of
comparing the claim of twentieth century particle physics with nineteenth century atomic, then it would still be possible to make an assessment on rational grounds of the relative merits of these research traditions. Thus Laudan established that we can compare theories with respect to their internal consistency or coherence and possible incommensurability of theories and research traditions does not preclude the existence of comparative appraisal of their acceptability.

Reference

Laupan L. “Progress and its problems” Univ. of California, Berkeley, 1977. PP: 74-76

3. Ibid. P: 78

4. Ibid. PP: 85-86

5. Ibid. P: 89

6. Ibid. PP: 90-91

7. Ibid. PP: 97-98, 100

8. Ibid. PP: 108-111

9. Ibid. P: 132

10. Ibid. P: 142

11. Ibid. PP: 142-146
Among the post Kuhnian philosophers, C. R. Kordig also enjoys an important position. Kordig does repudiate the Kuhnian position on meaning variance theory. However he points out that meaning variance theory does not yield desirable consequences. He sketched various undesirable consequences to establish his position of meaning invariance and suggested an alternative account for the comparison of theories through appeal to first level and second level invariance. Thus he holds his position in opposition to widely influential views of Kuhn which maintains that transition from one scientific paradigm to another force a change in the meaning of the terms employed which is radical enough to preclude the possibility of comparison of scientific theories from different traditions. For example according to Kuhn and also by Feyerabend, the meaning of "mass" (among other terms) has radically and
incommensurably changed meaning in the transition from classic to relativistic mechanics.¹

The dependence on velocity and convertibility with energy are built into the relativistic concept of “mass.”

Kordig finds that the philosophical interpretation of such example is implausible so a deeper objection against meaning variance thesis was raised by him which are being explained as under.

The first methodologically implausible consequences of the doctrine of radical meaning variance is that if it were true then no theory could be consistent or contradict with another. (When a new theory T₁ emerges to replace an old one T, the terms involved both theoretical and observational will change in such a way that there will be an elimination of old meaning - this is the view of the proponents of radical meaning variance theory.² Here two different
incommensurable concepts will emerge out by the same term employed in both cases. Hence $T_1$ and $T$ could not contradict each other or mutually be inconsistent.

For further elaboration Kordig quoted the Bohr theory of atoms which assumes that electron revolves about the nucleus of an atom in such a way that their orbital angular momentum is quantized (it is a whole multiple of $\hbar/2\pi$, where $\hbar$ is Plank's constant); it also assumes that energy is radiated or absorbed by the atom only when an electron jumps from one stable orbit to another and that this energy is also quantized. When the Bohr theory claims that angular momentum and radiant energy of electrons cannot have continuous values but must be quantized, it denies the assumption of classical electrodynamics that angular momentum and radiant energy of electrons can have continuous values. I will further explain the classical electrodynamics in comparison to
Bohr’s theory of atom to present the ground reality towards the claim of Kordig. According to classical electrodynamics the angular momentum of moving electron around the nucleus is not quantized (i.e. it is not integral multiple of h/2π). When electron jumps from lower energy level to higher energy level there is loss of energy in the form of radiation which is termed as radiant energy but in this case it will be quantized. It can be presented mathematically, ΔE = hν = E₂ - E₁ where E₁ is the first energy level and E₂ is the second energy level. Here energy is proportional to frequency ‘ν’. So it is continuous. But Bohr modified classical electrodynamics that angular momentum of a moving electron around the nucleus of an atom is not continuous but discontinuous (quantized) when electron jumps from lower energy state to higher energy state there is a loss of energy in the form of radiation and this radiant energy will be quantized because E=nhν where
n=1,2,3,---. This energy will be only in the whole number not in fraction. So we see that the terms such as angular momentum, radiant energy used in Bohr’s theory would be held to have different meanings from those in classical electrodynamics. Indeed they are to express incommensurable concepts, thus they could not contradict one another if the radical meaning variance thesis is true.

The second methodologically unacceptable consequence of the doctrine of radical meaning variance is an extension of the first, which can be stated in the following way. If the doctrine were true then each scientist would be effectively isolated within his own system of meanings. Each of these meaning would be radically different from those of scientists within other traditions or from those of scientists holding other theories. True communication from one such system to another either in agreement or disagreement would be
impossible. Each as Scheffler points out would be “trapped in the web of his meaning”.³

Kuhn maintains that competing paradigms are addressed to radically different problems.⁴ They incorporate radically different standard and even radically different definition of science. They are based on radically different meaning.

But if this is so in what sense could such paradigms be said to be in competition? How could they be either rivals or alternatives? To maintain that they are in competition is to place them within some common framework, which has comparative and evaluative standard applicable for both. It is to consider them as oriented in somewhat different ways towards the same purpose and scientific goals. And it is the invention of alternatives for the purpose of mutual criticism which is central to Feyerabend’s own positive methodology:
You can be a good empiricist only if you are prepared to work with many alternatives theories rather than a single point of view and experience.\textsuperscript{5}

Given the radical meaning variance position, two different theories are radically different in meaning. It is thus hard to see how they could function as alternatives to each other or serve to criticize other, just as sociological theory and quantum theory which are radically different in meaning are neither alternative to each other nor serve to criticize one another. Feyrabend thinks that adopting the radical meaning variance position enables us to come closer to and more fully reach his goal. On the radical meaning variance view, theory displacement in science is held to affect observational as well as theoretical categories and notions. It follows that apparent sharing of observational terms by
theoretical opponents is really a delusion. If we are to understand another’s observational or experimental claim, it is held that we must first accept his theory. Further it is held that if we are to understand another scientist’s language we must share his theory; this means we must share certain deep features of his thought-world, his outlook, expectations and beliefs for it is only in terms of these. It is claimed that his language can be rendered intelligible. True communication between holders of different scientific theories thus becomes impossible. For a scientist of one tradition to significantly converse with an opposing theorist from another tradition on neutral ground becomes impossible. Kordig thus holds the view that radical meaning variance, as Feyrabend thinks is not methodologically desirable.

There is a third methodologically undesirable consequence of the doctrine of radical
meaning variance. If the doctrine were true it would be difficult to see how one could learn a new theory. He could not learn it by having it explained to him using any scientific terms whose meanings he understood before he learned the new theory. Consider the term mass, velocity, and energy, which are used, in relativistic mechanics. The meaning of each of these terms, given the doctrine of radical meaning variance, is theory Laden. As Hanson would say, "The entire conceptual pattern of the game is implicit in each term". If so, then, in order to know what the terms of relativistic mechanics mean, I must know relativistic mechanics or at least its central principles. One of these central principles can be roughly expressed like this. 'Mass' is a function of velocity and is convertible with energy.

But it is hard to know how I could understand what the above expression asserts unless I already to some degree know what the
term's man, velocity, energy mean. While radical variance theorists would hold that to learn what the terms mean I must learn the theory. To learn the theory, however, I must learn its central principles. But to learn the latter, it would seem that I must know what the terms involved mean. Given radical meaning variance, this circularity is vicious. In trying to circumvent, it is useless to appeal to what the terms mean in different theories. What they mean in any two theories is held to be radically different and incommensurable. Therefore Kordig says that one could not use any term whose meanings have 'been understood in order to learn the meaning of the terms in relativistic mechanics. Thus if the radical meaning variance accounts were correct, most scientific looks would therefore end up useless in principle.

So the finding of Kordig is that it is difficult to see how one could in any sense learn
a new theory by adopting the general radical meaning variance position.

There is a fourth methodologically undesirable consequence of the doctrine of radical meaning variance position. If the doctrine were true, no scientific theory could be tested or falsified by any observation reports. Feyrabend and Kuhn would agree that without the help of other theories, no such theory could be falsified.\(^7\)

As Feyrabend puts it:

One most important point of agreement is the emphasis which both of us (Kuhn and Feyrabend) puts upon the need, in the process of the refutation of a theory for at least another theory.\(^8\)

There exists facts that can not be unearthed except with the help of alternatives to the theory to be tested and that become
unavailable as soon as such alternatives are excluded.\footnote{9}

Both the relevance and the refuting character of many decisive facts can be established only with the help of other theories that, although factually adequate, are not in agreement with the view to be tested. Empiricism demands that the empirical contents of whatever knowledge we possess be increased as much as possible.\footnote{10}

This implausible if the doctrine of radical meaning variance is correct, indeed it is inconsistent with the doctrine.

A different theory T\textsubscript{1} could not be used to show that observations exist which do not satisfy T. The concepts of T\textsubscript{1} would be held to be incommensurable with those of T. Thus, satisfied prediction statements expressible in T\textsubscript{1} could not be used as Feyrabend wishes them to be used. If they could then T and T\textsubscript{1} would not be
incommensurable which is contrary to the radical meaning variance position.

There is a fifth methodologically undesirable consequence of the doctrine of radical meaning variance. If it were true then there would be no sense left to the notion of a rational progression of scientific viewpoints from age to age.

We had seen in the previous undesirable consequences related to the doctrine of radical meaning variance theorists that:

1). Two different theories could neither agree nor disagree (contradictory).
2). That each scientist is effectively isolated with his own and unique system of meaning.
3). That no theory can be falsified or tested.
4). That scientists could not learn new and different scientific theories. Thus, Kordig finds that there is no sense to the customary notion of the rational progression of scientific viewpoints from age to age. He holds that when scientists choose one theory over another we could not claim that this choice constituted progress. For example we could not claim that the scientific community's choice of Einstein's theory over Newton's theory and of Kepler's theory over Brahe's theory constituted progress. Kordig examined this consequence and concluded that the problem arises because Kuhn gives no reason consistent with the rest of his positive which could serve as grounds for accepting one paradigm as better or more acceptable than another. Given his general position one could not say, in any ordinary sense, that progress is made when another replaces one paradigm through scientific
revolution. Why? Different paradigms radically disagree as to what are the facts, the problems faced, and the standards which the successfully theory must meet. A paradigm change brings about changes in the standards governing permissible problems, concepts and explanations. Kuhn’s position indeed tends towards the conclusion that the replacement of one paradigm by another is not commutative but is more replacement, mere change. If this is so two different paradigms could not be judged according to their ability to solve the same problems, deal with the same facts or concepts, or meet the same standards, for all of these are radically different for different paradigms. Such a conclusion is, however, inconsistent with the positive part of Kuhn’s methodology. In discussing how paradigm disputes are finally resolved, Kuhn draws attention to “two all important conditions” which a new and successful paradigm will
satisfy. First it will, resolve some outstanding and generally recognized problem that can be met in no other way. And second it will, “preserve a relatively large part of the concrete problem solving ability that has occurred to science through its predecessors”. It will preserve a great deal of the most concrete parts of past achievement. However, neither of these, “all these important conditions” could be met if the rest of the Kuhn’s interpretation were correct. As scheffler has correctly noted:

Such conditions of evaluation contradict the main thesis appealing to the history of science, namely, that paradigm change in science is not generally subject to deliberation and critical assessment.¹³

So the consequence according to Kordig from methodological point of view for the radical
meaning variance position held by Kuhn is undesirable. He has a second sort of objection to the claim that if the radical meaning variance thesis were true then scientific change could not constitute progress.

Toulmin claims, in effect, that there is a special sense of progress and cumulative in which the radical meaning variance position does not entail that scientific change is non-cumulative or non-progressive. After accepting the radical meaning variance thesis as to both observational and theoretical term, he exhibits some sensitivity to the problem —— how do we know which presupposition to adopt? Certainly, explanatory paradigms and ideals of natural order are not true or false in any naive sense. Rather they take us further (or less far) and are theoretically more or less fruitful. Given Toulmin’s radical meaning variance position, it would be doubtful as Shapere has pointed out,
whether there are shared ideals or standards which are invariant with respect to scientific revolutions. Therefore it would be doubtful whether different paradigms could be judged to be "more or less fruitful" in accomplishing common tasks. For the same reason it would be doubtful whether there were common jobs that one paradigm could take us further towards than another. So the question that how theories can be judged against one another and how the replacement of one theory by another can be said to constitute progress or an advance remains unanswered.

The sixth methodologically undesirable consequence of the doctrine of radical meaning variance position is perhaps the worst. The doctrine is demonstrably untenable because of a self-referential problem.

For this, Kordig examines the position that there is no objectivity in science. Consider the claim that the choice between only scientific
theories is a matter of taste (as hold by Feyrabort) where, “neither proof nor error is at issue (Kuhn). Such a restricted claim is nevertheless problematic. It leads to an unjustified dualism. On the one hand we are supposed to hold that, “science is a subjective enterprise whose concepts and domain are theory laden. On the other hand we are supposed to also hold that the philosophy of science is an objective enterprise whose concepts and domains are not theory laden. People who adhere to the earlier claim would to avoid the self referential problem and have to maintain that their own views of scientific change are uninfluenced by the fact that they are Kuhnians that is they would have to hold the latter claim. But what is the difference between these two domains? The answer is that non-theory laden facts are relevant to the philosophy of science but not to science. Kuhn’s and Toulmin’s advocation of a purely descriptive
methodology for evaluating rival scientific theories are the last example here. 18

Therefore the doctrine of radical meaning variance is either demonstrably untenable or leads to neopositivistic aspects.

So Kordig has suggested an alternative account which has distinct virtues and advantages. He has suggested that comparisons of different theories are possible since it is possible for them to be some shared meaning of the terms involve.

But he urged that comparisons of different theories are in fact made through appeal to shared principles and meaning at both a “first” and “second” level. By shared meaning at a first level Kordig means shared extension of terms employed by rival theories and by shared principles and meaning at a second level he means shared regulative principles which scientists require of successful theories and which guide their choice.
among alternative theories. Kordig argued that the “first level” in variance usually occurs in scientific transition.

Let us consider the example of observational or experimental invariance, namely the transition from Galilean physics to Newtonian physics. Nagel thinks that the former science is reduced to the latter, \(^\text{19}\) which Feyrabend denies it. \(^\text{20}\). As Feyrabend, \(^\text{21}\) correctly notes, Galilean physics dealt with the motion of material object (falling stones, penduli, balls on an inclined plane) near the surface of the earth. Its subject matter was a terrestrial object. It is possible to distinguish the subject matter of the Newtonian and Galilean science. The latter was concerned only terrestrial phenomenon and the former dealt also with the celestial phenomenon. Thus the subject matter of two sciences says are not coextensive. But since Newtonian physics also referred to material objects near the surface of
the earth, the subject matter of Galilean physics is a subset of the subject matter of Newtonian physics. The objects referred to by name of the terms employed by $T_1$ (Galilean physics) are also referred to by some of the terms employed by $T_2$ (Newtonian physics). In the transition from $T_1$ to $T_2$ there is observational invariance and thus refers to extensional meaning invariance. As Nagel correctly notes in his discussion of the reduction of $T_1$ to $T_2$, their subject matter are in an obvious sense homogeneous and continuous; for it is the motion of the bodies and determination of such motions that are under investigation in each case. Using Feyrabend own words, we can describe the neutral observational objects in terms which are neutral to $T_1$ and $T_2$. Both $T_1$ and $T_2$ refer to material objects such as falling stones, penduli, balls on inclined planes etc, each of which are near the surface of the earth. None of the terms used in this description are peculiar to only $T_1$
and $T_2$, nor does the description presupposes meaning invariance between $T_1$ and $T_2$. We have assumed only the ordinary English language meaning of these terms – whether or not they have the same meaning as their typographical counterparts, if any, either $T_1$ and $T_2$. Thus in order to establish whether our description is correct and whether there exists observational invariance between $T_1$ and $T_2$, one need not presuppose meaning invariance between $T_1$ and $T_2$ and our description, indeed, is correct as every historian of science would recognize. Therefore, there is observational invariance between $T_1$ and $T_2$. And hence there is also extensional meaning invariance between Galilean and Newtonian physics. Galilean idea of mass, acceleration, force were applicable to all bodies located near the surface of the earth, the Newtonian idea of gravitational force included these bodies within its range and of
course added celestial bodies as well. Margenau notes this point well:

The Galilean ideas of mass and acceleration could be applied to a great variety of bodies, namely all those located near the surface of the earth. However, Newton’s discovery of the law of universal gravitation was more extensible; it included within its range of celestial bodies. Bay seizing upon the idea of a gravitational force, Newton provided a concept of impressive width, thereby significantly advanced the science of mechanics. 23

This overlap between the ontologies of Galilean and Newtonian physics is non-trivial. The class of all terrestrial objects is not a trivial or an insignificant class. And this class compromises
the overlap of the common objects dealt with by both of these physical theories. Terrestrial object is in each theory a correct answer to the question what moves? A term that illustrates non-trivial is meaning invariance in both theories, therefore, inertia. There are other terms also which illustrates non-trivial meaning invariance. In both theories terrestrial objects undergo displacement. Similarly, in each theory terrestrial objects have mass, under go acceleration and undergo velocity. There is, therefore, also some non-trivial meaning invariance with respect to these phrases. There are our two examples of observational invariance for first level.

Now we have to see the findings of Kordig for the second level discourse. He finds Kuhn’s position regarding this as fallacious because Kuhn suggests that sharing of second order standard is impossible. Kuhn feels that acceptance of a
paradigm entails acceptance of governing standards or criteria. One can then use these to justify his acceptance of the paradigm against its rivals. Because of this Kuhn maintains that different paradigm employ different standard at the second level of scientific discourse. He concludes that each paradigm is, in effect, fails justifying and provides science not only with a map but also with some of the directions essential for map making. However the standards used to evaluate paradigms themselves are different in nature and function from the standards, internal to a paradigm. To say there are internal standards involved in a paradigm is just to say that the paradigm may be understood as defining, within some scientific domain, a range of legitimate problems along with approaches to and forms of solution. However, no set of mapping directions employs how it is itself to be evaluated in comparison with alternative sets rather as Scheffler notes, no such set
implies that it is itself superior to its alternatives. Thus it is in this sense, each paradigm is not self-justifying. So kordig suggested the second order standard for such task. These standards might then be used by philosopher of science as a philosophical rationale for the evaluation of a paradigm against one another. In the second order sense each paradigm is not self-justifying contrary to Kuhn, because his argument has not precluded neutrality or objectivity from playing a role in paradigm evaluation. He has not demonstrated that scientific transitions consist only in non-cumulative persuasions and conversions. Kordig holds the view that at a second level standard, there should be some invariance with respect to scientific change. Shared second level standards are needed, and used, in the business of accepting, rejecting, and evaluating rival or competing theories. They serve to regulate the
choices among rival theories. He has briefly described several guiding principles. None is absolutely invariant each may change in time. But some accounts along with some illustrious from the history of science should show that they need not change with change in scientific theories, that changes in them proceed very slowly, and they are in fact usually invariant with respect to scientific transitions. It is through appeal to such shared standards that the shift of allegiance in the scientific community usually occurs. By drawing the work of Margenau, Kordig has briefly discussed the following.

a. Empirical confirmation: A theory is confirmed of its consequences via the rules of correspondence. Further the theory that has sustained many circuits of empirical confirmation is usually promising for scientific acceptance. The range of application of
confirmation theory is designed to include any scientific theory.

b. Logical fertility: Margenau expresses it by saying that hypothesis of scientific theory should obey logical laws.\textsuperscript{28} If, for example, a scientific theory is logically inconsistent then this is ground for its rejection. Scientific theory should be coherent. Logical coherence is one of the regulative aims of science. This requirement can be shared by and used to evaluate different scientific theories. It is a demand, which is usually invariant with respect to changes of scientific theory. Even with respect to quantum mechanics where much valued logic has been considered, logical consistency has not been given up.

c. Extensibility: Scientific theories should be extensible to as large as possible. Other regulative principles being equal, the more extensible is the better theory. On this score,
Newtonian mechanics, which dealt with both terrestrial and celestial objects, is a better scientific theory than Galilean mechanics which dealt only with the former objects.

d. Multiple connections: Scientific theory should be organized and systematic. That is the constructs used in scientific theories should be multiple connected. Scientific hypothesis should be adhoc. Hempel and Grubaum stresses this point well.

e. Simplicity: It is often used as a regulative principle in evaluating rival scientific theories, is beyond doubt. This notion has often been appealed to as a basis for choosing among rival theories. The Copernicus revolution is a case in point. Copernicus, by placing the sun at the center of the planetary universe, was able to reduce the number of epicycle from 84 to 30. This eliminated a large number of unrelated epicycles, which has previously been needed to
explain the same observation. Perhaps because of 
this simplicity Copernican astronomy was more 
systematic and less adhoc than Ptoleemic 
astronomy. And this too is a factor in comparing 
these theories, as Runder aptly puts it, “system 
is no more adornament of science, it is very 
least.” Simplicity is being deliberately 
conducted in a way, which would permit the 
application to more than one theory.

f. Causality: Kordig argues with Margenau that 
causality is a metaphysical requirement. It 
demands that constructs should be so chosen as 
to generate causal laws. Holding the view of 
Margenau, Kordig regarded causality as a 
property of physical law and not as a relation 
between single observations. This regulative 
principle is met by both Galilean and Newtonian 
mechanics and by the scientific theories of both 
Brahe and Kepler. The principle was not 
abandoned in this transition, it continued to be
employed and was in fact fulfill by the laws of these theories. It was, therefore, invariant with respect to these scientific transitions.

Thus Kordig holds that objectivity is an ideal, which is in fact employed in the scientific practice. Science is a systematic public enterprise that can be justified by reference to second level standards, logic, and empirical facts. Part of the purpose of science is to formulate truths about the natural world in a simple, comprehensive systematic and intelligible ways in which nature becomes explainable, predictable and controllable. The success of any particular scientific theory is a measure of how far, and how successfully, it contributes to the realization of the general second level aims of science. If significant first level sharing occurs between T and a competitor T' and if T takes us further in the direction of the above second level aims than T', then T should be accepted and T' rejected.
References


10. Ibid. P-176.
12. Ibid. P-168.
15. Ibid. Pp-57-95.
16. Ibid. P-57.


21. Ibid. P-46.


25. Ibid. P-108.
32. Kordig C R “The Justification of Scientific Change”,
Mark. A. Stone

So far I have discussed the views of Lakatos, Laudan and Kordig. In this section, I shall examine the views of Mark. A. Stone. He holds that Kuhn and Lakatos are against the traditional views about science. Their criticism share two themes; first, that a scientific theory is not tested alone against empirical result and second, that one does not reject a theory without a successor at hand. In Kuhnian notion it is the paradigm of the theory that is either accepted or rejected while in Lakatos’ terminology it is the research program of the theory that is either accepted or rejected with the condition that a successor must be available before the rejection of a paradigm or research program.

He agrees with the view of F. M. Akeroyed who criticizes and holds opinion that there are occasion in the history of science when a research
program is simply rejected and indeed rationally rejected despite the absence of a successor.¹ Mark Stone says that Kuhn is subject to the same criticism with respect to his favorite historical example of Copernican revolution. In his subsequent writings, Kuhn has identified two main sources of paradigm; the disciplinary matrix and the exemplars of a scientific community.² This disciplinary matrix consists of those rules, some explicitly stated and some only implicitly understood that provide methodological guidelines to how research and experiments will be conducted and that will determine what will count, as legitimate problems for investigation by the community as well as criteria for legitimate problem solutions. In addition, the disciplinary matrix includes the metaphysical beliefs shared by the community about what sorts of entities are present in and what sorts of processes are at work in the world. Exemplars constitute a body of concrete examples of
successful problem solutions that provide the community with an understanding, largely implicit of its disciplinary matrix and the theories that are formulated within that disciplinary matrix. So the use of the term paradigm refers to the conjunction of disciplinary matrix-exemplars theory that constitute the discipline of a particular scientific community. Kuhn’s evaluation is based in part on a linguistic thesis and in part on an epistemic thesis. The former asserts that meaning of a paradigm is holistic, one can not understand the belief or a assertion of a scientific community in isolation rather within a whole interrelated network of a paradigm. While the latter asserts that there is no paradigm independent foundation for theory, grounds for believing a theory and grounds for believing that certain empirical results require explanation and are based entirely on criteria internal to the paradigm of that theory. This is the basis on which Kuhn rejection
of prior verificationist / falsificationists approach lie. He summarizes his criticism of verificationism as follows:

When paradigm enters, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm’s defense.¹

Kuhn’s rejection of the principle of falsifiability is more complex. Kuhn recognizes that no single observation that apparently contradicts the prediction made by the theory suffices as grounds for rejecting a theory. One can not neatly separate those observations that constitute problems yet to be solved from those that come to be recognized as genuine anomalies. As Kuhn says:

There are, I think only two alternatives; either no scientific
theory ever confronts a counter instance, or all such theories confront counter instances at all times.\textsuperscript{4}

Kuhn’s argument here provides the basis for his claim that it is paradigm as a whole and not theories in isolation that are accepted or rejected. He further argues that a scientist does not reject one paradigm without simultaneously accepting another. Since Kuhn regards the possession of a paradigm as a necessary condition for practicing science at all, he concludes that, “To reject one paradigm without simultaneously substituting another is to reject the science itself. That act reflects not on the paradigm but on the man. Inevitably he will be seen by his colleague as the carpenter who blames his tool.”\textsuperscript{5}
He further claims that,

Once it has achieved the status of a paradigm, a scientific theory is declared invalid only if alternate candidate is available to takes its place. The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves the comparison of both paradigm with nature and with each other.⁶

But there is an important question here, if a scientist never abandons one theory until a successor is at hand, then where do successor theories come from? If scientists always accept the current paradigm until presented with an alternative, then it is unclear who will do the work to formulate an alternative. To answer this puzzle we must look in more detail at the process
of scientific discovery. The answer will show that at least in some cases, scientists can and must reject one paradigm without ready successor.

At this juncture Mark A. Stone has established his sharp opinion and argued in the following way: He divided scientific discoveries into three types. First, at times revolutionary discoveries occur by accident. It has happened that a scientist will by chance stumble across a phenomenon so striking that he is compelled to adopt a new paradigm in-order to assimilate the new phenomenon. Mark Stone calls this spontaneous discovery. Second, at times revolutionary discoveries occur unnoticed. A scientist makes what he thinks is only a small addendum to some well established theory, and only later comes to realize that the implications of his modification require abandoning his previous paradigm. Mark Stone calls this implicit discovery. Finally, a scientist may perceive a felt need for a new discovery, set about
to find it, and succeed. Mark Stone calls this directed discovery.

The discovery of x-rays is a typical example of spontaneous discovery. While doing routine experiments with cathode rays the physicist Roentgen observed that a barium platino cyanide screen in the laboratory began to glow whenever cathode rays were discharged. Weeks of investigation by Roentgen produced a rudimentary theory of x-rays. At first he refused to believe that cathode ray emissions were in fact responsible. However: further investigation, they required seven hectic weeks during which Roentgen rarely left the laboratory—indicated that the cause of glow came in straight lines from the cathode ray tube, that the radiation cast shadows, could not be deflected by a magnet, and much else beside.  

Thus Roentgen found himself in a situation in which the new phenomenon could only be
understood with the aid of the new paradigm. To accept a new phenomenon was in fact to accept a new paradigm.

The process of implicit discovery accords with Kuhn’s view. Mark Stone finds the example of this process in the origin of quantum theory. In 1900 Plank was studying the phenomenon of black body radiation. A body such as an iron bar, when sufficiently heated goes through a spectrum of changes in color for dull red to bright white. Plank wanted to understand the relationship between the energy absorbed by the body and the radiation emitted by the body. Plank saw a connection between his problems and a problem to which Boltzamann had offered a new solution. Boltzamann was concerned with gases, not solid bodies. His question was: given the known average velocity of the molecules in a gas, what proportions of the molecules are moving at some multiple of that velocity? Boltzmann solution was original because of its application of
probability theory. He imagined a line segment scaled to the total kinetic energy of the molecules in the gas, so that one end point of the segment is zero and the other end point is \( E \), where \( E \) stands for the total kinetic energy of the molecules. He then considered this segment be divided into finitely many smaller segments of equal size. What are the possible distributions of molecules among these cells, given that the sum of the kinetic energy for all molecules cannot exceed \( E \)? Boltzmann was able to show that only certain distributions satisfied these restrictions and using probability theory he was able to show what the likelihood was that any given distribution would be represented in a gas.

Plank made some theoretical assumptions that allowed him to treat the problem of black body radiation like Boltzmann’s problem. Plank theorized that a body was filled with what he called resonators, which resonate radiation only at a
particular frequency, and that the body as a whole would be filled with resonators covering the full operation of frequencies. Dividing the energy line segment into smaller segments as Boltzmann did, Plank then could ask what proportion of resonators fill along each smaller segment and apply probability theory to obtain a solution again just as Boltzmann had.

This much of the story fits exactly the pattern of what Kuhn calls normal science. Certainly Plank saw nothing revolutionary in his theory. Indeed Plank did not immediately see anything significant in the one additional restriction that he had to place on his theory that was not part of Boltzmann’s solution: the unit sizes into which the energy line could be divided were not arbitrary, but were in fact dependent on the number we now know as a Plank’s constant.

In order to understand Plank’s solution one had to accept that there were discrete jumps in
the energy level of an atom. This result was simply unintelligible by the standards of the existing paradigm. Assimilation of Plank’s solution therefore required a new paradigm. The effect of implicit discovery is thus similar to the effect of spontaneous discovery. One either accepted the new problem solution, or thus accepted a new paradigm. There is no rejection of the older paradigm without simultaneous acceptance of the new paradigm.

Mark. A. Stone holds that the significance of spontaneous and implicit discoveries are accidental and when he turned his attention towards directed discovery, he sees that Kuhn has overstated his statement. The historical example that Kuhn discusses most frequently, the Copernican revolution is an instance of this discovery. Copernicus the very famous astronomer judged that the Ptolemaic theory of celestial mechanics is unacceptable and he was firm in his conviction that
Ptolemy’s theory had to be rejected. In his own words:

The mathematicians are unsure of the movements of sun and moon that they cannot even explains or observes the constant length of the seasonal year. Secondly, in determining the motion of these and other five planets, they use neither the same principles and hypothesis nor the same demonstrations of the apparent motions and revolutions.⁸

So Kuhn says:

“For the first time a technically competent astronomer had rejected the time honored scientific tradition for reason internal to his science, and this professional awareness of technical fallacy
inaugurated the Copernican revolution."^9

So Mark Stone sees a clear refutation of Kuhn’s own thesis that scientist never rejects one paradigm until a successor is at hand and yet Copernicus even by Kuhn’s own admission has done precisely. Thus Mark Stone has established that Kuhn has made two mistakes in overlooking the significance of directed discoveries. First he has conflated two different sense of reject. Second he has failed to notice that within his own framework there is a room for an account of falsifiability that makes sense of directed discoveries. On the one hand “reject” can mean to find unacceptable. On the other hand “reject” can mean no longer make use of it. First is called by Mark Stone as epistemic rejection where rejection entails a change in belief. The second is called as pragmatic rejection, where rejection entails a change in action. Thus Mark Stone feels that Kuhn requires an
alternative account of falsifiability and rejection for cases of direct discoveries. So according to him Kuhn's views are threatening to the rationality of science as his claim between the case of Copernicus and Ptolemy has failed because of methodological stricture of Ptolemic paradigm was that planetary motion be accounted for, in terms of circular motion while Copernicus argued that Ptolemic theory had in effect failed to adhere the stricture.

So the view being established here entails that Kuhn's old paradigm if altered requires new one has not a valid ground according to Mark Stone and he further says that if the revolutionary discovery is either spontaneous or implicit then the results are happened by chance and if the discovery is directed then it must be preceded by a rational procedure.
References


4. Ibid. P-80.

5. Ibid. P-79.

6. Ibid. P-77.

7. Ibid. P-57.


11. Ibid. P: 185
John Watkin confronted with Kuhn’s account of Normal science and ultimately disproved that Normal science constitutes the essence of science.

He argued that Normal science could not have the character as Kuhn ascribes to it that it is capable of giving rise to extraordinary or revolutionary science.

Later by taking into account the Kuhn’s comparative evaluations of Normal and extraordinary science on the supposition that history of science does in fact display a Normal science-Extraordinary science cycle, Watkin challenged this supposition in different form. His objection was concerned with the possibility of the emergence of a new paradigm at the end of a period of Normal science and concluded that the new paradigm never could emerge from Normal science as characterized by Kuhn.
For this Watkin recapitulated Kuhnian thesis concerning paradigm change which ultimately leads to meaning change in scientific theories.

1. It is the nature of a paradigm to enjoy a monopoly in its hold on a scientist’s thinking. A scientist cannot entertain a rival paradigm while under the sway of one paradigm. If he has started toying with a rival paradigm, then the old paradigm is already defunct for him. This he called paradigm-monopoly thesis.

2. There is little or no interregnum between the end of the old paradigm’s reign over a scientist’s mind, and the beginning of the new paradigm’s reign. A scientist does not flounder around for any substantial length of time with no paradigm to guide him. He abandons one paradigm only to embrace a new one. Watkin calls this non-interregnum thesis.

3. A new paradigm will be incompatible with the paradigm it supercedes and further Kuhn claims
that it will be incommensurable with the old one.  Watkin calls this thesis as a clash between the old and new paradigm.

4. From the conjunction of the above three theses, it follows that a scientist’s change over from an old paradigm to a new; one must be swift and decisive. Kuhn emphatically endorses this application. He says that paradigm switch is a relatively sudden and unstructured event like the gestalt switch. So Watkin calls this as Gestalt switch thesis.

5. Kuhn’s view allows that it may take quite a time for a paradigm, once invented to gain general acceptance. But, how long may it take the original inventor to put together the rudiments of the new paradigm. He says that we must remember that the new paradigm is immediately powerful enough to induce our scientist to turn against the well articulated and unrefuted that has dominated his scientific thinking. This means
that new paradigm at the outset must be large and definite enough for its striking potentials to be fairly apparent to its inventor. If that is so, the instant-paradigm thesis according to Watkin seems to be barely credible on psychological grounds. He also says that there seems to be a certain internal incoherence in Kuhn’s version of his thesis and here Watkin makes that the paradigm monopoly thesis must go.

REFERENCES


2. Ibid. P: 121.