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"THEORY INCOMMENSURABILITY" AND KUHN'S HISTORY OF SCIENCE

A CRITICAL ANALYSIS

Kuhn's theory of scientific change[1] is founded on the idea that there are minimal defensible grounds for the claim that the history of science is characterized by the cumulative growth of knowledge. According to Kuhn, revolutionary theories in the history of science cannot be perceived as logical and empirical derivations from their predecessors since, quite often, the research methods, theoretical assumptions and the empirical findings of the former are incompatible with the latter. Thus, the analysis of each novel scientific theory must begin with a recognition of the epistemological and ontological independence of that theory's paradigm.

The significance of Kuhn's thesis is that while paradigm independence is generally regarded as an evident feature of theories in nonscientific research areas, this has not been the case for natural science. It is assumed that while the research findings

[1] Reference is made to Kuhn's thesis as expounded in *The Structure of Scientific Revolution*, Chicago, The University of Chicago Press, 1962, 2nd ed.

of most areas of nonscientific research are not justifiably analyzable in terms of separable contexts of discovery and justification, orthodox theoreticians of science have argued that the nature of science is such that this distinction could indeed be made for much of its history. The originality of Kuhn's history of science derives from his denial of this special feature to science.

The purpose of this paper is to evaluate critically Kuhn's thesis theory incommensurability (i.e., novel theories are not epistemologically incompatible with their predecessors) as a feature of the history of science and to propose the argument that the theories that Kuhn claims as part of his thesis are not genuine scientific theories but rather theories best described as not being fully scientific. Kuhn's argument that the incommensurability of such theories with their successors provides proof of an intrinsic nonrational element in the history of science would, therefore, be shown untenable.

Although there has been much evaluation of Kuhn's thesis, most analyses have not fully answered his claims. Some of Kuhn's major critics may be regarded as scientific realists in the sense that they argue that no theory could be viewed as scientific unless it sought to explain truly the phenomenon in question. Specifically, they claim that the referring terms of a genuine scientific theory should refer to actual entities—at least approximately.[2] This proviso is guaranteed by the requirement that novel successor theories should contain their predecessors as special cases. This critique of Kuhn's theory of science is evidently normative and does not answer his thesis, since this thesis is purportedly descriptive of the actual historical path of science. Other theorists, recognizing that Kuhn's thesis supports as irrationalist theory of science, argue that scientific research must be necessarily rational. Laudan, for exemple, writes:

> Having observed, quite correctly, that the Popperian model of rationality will do scant justice to actual science, they [Kuhn and Feyerabend] precipitately conclude that science must have large

[2] See, for example, W.H. Newton Smith, *The Rationality of Science*, London, Routledge and Kegan Paul, 1981, and David Stove, *Popper and After*, Oxford, Pergamon Press, 1982.

irrational elements, without stopping to consider whether some richer and more subtle model of rationality might do the job.[3]

It seems to me, however, that a much more fruitful critique of Kuhn would be to raise questions about his definition of scientific method, and his criteria of demarcation of science from nonscience in order to detect any inconsistencies in his application of such concepts.

I shall proceed as follows in this paper: first a synopsis of Kuhn's views on the idea of theory incommensurability and his position concerning criteria of demarcation, etc. will be formulated. I shall then discuss the idea of scientific method according to its modern and premodern meanings so as to evaluate Kuhn's thesis in the light of the methodology of scientific investigation. Finally, I shall then examine, from the standpoint of the sociology of knowledge, possible reasons as to why Kuhn has been able to generate, without much opposition, a historical program of scientific progress which includes important references to theories which may be best regarded as protoscientific.

I

According to Kuhn, the methodologies of investigation employed by past researchers in their studies of the empirical world should not be viewed as inferior to those of the modern scientist even if the theories established by those premodern theorists have been discarded and shown to be incompatible with their successors. The main point of the Kuhnian argument is that while the history of science does demonstrate the expansion and growth of scientific knowledge, this development is characterized rather by discontinuity and the incommensurability of theories and their successors. This problem is a source of some concern to Kuhn

[3] Larry Laudan, *Progress and Its Problems*, Berkeley, University of California Press, 1978, p. 4. Similar views are expressed in F. John Clendinnen, "The Rationality of Method versus Historical Relativism", *Studies in the History and Philosophy of Science*, vol. 14, No. 1, pp. 23-28, and Harvey Siegel, "What is the Question Concerning the Rationality of Science, "*Philosophy of Science*, 52 (1985), pp. 517-537.

because, perhaps, he senses the need to explain why scientific research, unlike research in other areas, does yield results which are generally regarded as evidence of progress in the accumulation of knowledge about the empirical world. In short, modern scientific research does seem to proceed in a progressive fashion yet Kuhn can demonstrate that there have been qualitative discontinuities between some past theories and their successors. But to adopt this approach is to generate a logical puzzle: if paradigm change entails theory incommensurability and discontinuity, how then could "progress" be measured? Kuhn attempts to answer this puzzle by arguing that "the result of successful creative work is progress" whether in science or not. However, this does not resolve the problem since the term "progress" in scientific discourse refers to progress in the cumulative sense. The scientific community values not merely creative work, but creative work that yields increased knowledge about the empirical world. It is for this reason that it would be erroneous to argue that creative work in astrology, religion or magic yields "progress" in the scientific sense.

Kuhn could resolve this dilemma by arguing that those theories which have been shown to be incommensurable with their relevant successors are not genuine scientific theories. But this approach would have denied him the grounds for making his own particular claims about scientific revolutions and the independence of paradigms. Consider the following:

> The more carefully they [historians] study, say, Aristotelian dynamics, phlogistic chemistry, or caloric dynamics, the more certain they feel that those once current views of nature were, as a whole, neither less scientific nor more the product of human idiosyncracy than those current today. If these out-of-date beliefs are to be called myths, then myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge. If, on the other hand, they are to be called sciences, then science has included bodies of belief quite incompatible with the ones we hold today. Given these alternatives, the historian must choose the latter. Out-of-date theories are not in principle unscientific because they have been discarded. That choice, however, makes it difficult to see scientific development as a process of accretion.[4]

[4] Thomas Kuhn, *op. cit.*, pp. 2-3.

https://doi.org/10.1177/039219218803614303 Published online by Cambridge University Press

It should be noted immediately that Kuhn's research efforts are to be regarded primarily as those of an historian of science, which explains his minor concerns for questions concerning the methodology of science. Yet Kuhn's text deals implicitly with the issue of scientific methodology given the claims he makes about *how* science has progressed throughout history. An orthodox historian of science would have tended rather to state the facts about man's attempts to understand the empirical world. But even the orthodox historian of science must theorize according to a set of implicit criteria of demarcation. He would normally attribute the beginnings of science to the theorists of classical Greece while recognizing the foundational but prescientific work of the pre-Hellenic natural philosophers. Kuhn, as historian of science, accepts this historical model but as a methodologist he must contend with the thesis that science has progressed cumulatively through time and is founded on rational principles of analysis and investigation. Yet, as his thesis demonstrates, if one accepts the historical boundaries of the orthodox history of science model, some account must be given of the theoretical discontinuities evidenced between some theories and their successors.

In order to justify the historical range of this scientific tradition, despite the discontinuities displayed within it, one approach would be to show that the theories which constitute this tradition demonstrate certain epistemological features which distinguish them from other modes of knowledge. Kuhn takes this position but in so doing is forced to make claims which are clearly inconsistent. Consider, in this connection, the following statements made by the early Kuhn: "The mutability of its fundamental concepts is not an argument for rejecting science. Each new scientific theory preserves a hard core of the knowledge provided by its predecessor and adds to it. Science progresses by replacing old theories with new."[5] Yet Kuhn writes later:

> But though the achievements of Copernicus and Newton are

[5] Thomas Kuhn, *The Copernican Revolution*, New York, Random House, 1957, p. 3.

> permanent, the concepts that made those achievements possible are not. Only the list of explicable phenomena grows; there is no similar cumulative process for the explanations themselves. As science progresses, its concepts are repeatedly destroyed and replaced, and today Newtonian concepts seem no exception. Aristotelianism before it, Newtonianism at last evolved—this time within physics—problems and research techniques which could not be reconciled with the world view that produced them.[6]

But this is the fundamental problem: if "each new theory preserves a hard core of knowledge provided by its predecessor and adds to it," what then is the basis for the major Kuhnian claim that the history of science is characterized not only by epistemological shifts in modes of theory construction and confirmation but also by the radical ontological claims of the new theories. At the same time Kuhn must retain the idea that each scientific tradition dating from the Greeks preserves something that serves as a general demarcation criterion to set it apart from other epistemological traditions. This something is the above-mentioned "hard core of knowledge."

Although Kuhn's thesis represents a critical work in the history of science, its impact thereon has been minimal. I believe that the reason for this is that historiographers of science are more concerned to state what they perceive as the facts of the history of the scientific enterprise without particular concerns for interpretive or epistemological considerations. Kuhn's approach as formulated in *The Structure of Scientific Revolutions* (henceforth, *SSR*) is not only a study in the history of science, but also one of the methodologies of scientific change. It is for this reason that SSR has attracted more attention from ethologists of science.

II

Much of Kuhn's thesis is founded on the notion of the paradigm independence of novel theories and their predecessors. Kuhn's critics, of course, argue that an endorsement of this methodology leads to epistemological relativism and the de-emphasizing of

[6] *Ibid.*, p. 265.

rational analysis as a key criterion in scientific research. Evidently, the problem rests on an examination of the idea of scientific methodology. Although Kuhn does not discuss, to any great extent, criteria for an adequate methodology of scientific investigation, his interpretation of the progress of scientific research in history does implicitly rest on a set of methodological criteria. Kuhn, for example, states that some research enterprises should not yet be regarded as sciences since they are still in preparadigmatic stages of development.[7] According to Kuhn, the preparadigmatic stage of development of a research discipline is characterized by ongoing discussions on what could constitute an appropriate methodology. These discussions invariably lead to a multiplicity of methodologies being proposed and ultimate disagreements on what research results constitute progress.

But in the case of genuine scientific research, as Kuhn argues, one witnesses consensus on a single research paradigm among its members and a commitment to the solving of puzzles or problems from within that framework. One could contrast this with reference to the point made above about the preparadigmatic stages of a research discipline, that is, that research areas in which competing schools persist and constantly question the foundations of each other are not usually regarded as scientific. (*SSR*, pp.162-163) But note that during periods of crisis for a given paradigm, alternative paradigms develop and in this respect the field of scientific research closely resembles research in nonscientific areas. And it is these periods of paradigm crisis that afford Kuhn the theoretical basis for his theory of paradigm independence and the questioning of the idea of cumulative progress in the history of science.[8]

I should want to argue, however, that the existence of a shared paradigm and the commitment to solve puzzles is a necessary though not a sufficient condition for establishing a scientific research program. After all, dogmatic schools of thought whether in religion or ideological areas express methodological features

[7] Thomas Kuhn, *The Structure of Scientific Revolutions*, p. 160.

[8] See Ian Barbour, "Paradigms in Science and Religion," in *Paradigms and Revolutions*, ed. by Gary Gutting, Notre Dame, Indiana University Press, 1980, pp. 223-245 for a thesis demonstrating similarities between adherence to a theological religious creed and a school of scientific thought.

similar to those of Kuhn's paradigm. There is a consensus on methodology (dissenters are usually silenced) and the function of theories is merely to solve or answer problems or questions as they arise. In fact, Kuhn's loose definition of a scientific theory opens up the possibility that obviously nonscientific fields such as astrology, religion, may be regarded as scientific. But it is this loose definition that best affords Kuhn the grounds for making his specific claims about scientific revolutions and the incommensurability of theories and their successors.

It is indeed the case that the premodern researchers to whom Kuhn refers as scientific theorizers, i.e., Aristotle, Ptolemy, Lamark, and others, share methodological research criteria with individuals who engage in activities not normally considered scientific, such as magic and astrology. After all, astrologers and magicians are concerned to know the world as objectively as possible, and to use this knowledge to control it. Research in the history of science affords much evidence that no sharp ontological distinction could be drawn between, say, the astrological and astronomical beliefs of premodern researchers, so too for those who did rudimentary research in chemistry and alchemy. It seems that if Kuhn could argue that the false "scientific" theories of Aristotle, Ptolemy, Lamarck *et al.*, are scientific in the sense that Newtonian theory is scientific, then one could make a similar argument in favor of theories of magic, religion, astrology, etc.

As pointed out above, Kuhn's rationale for his criterion of demarcation between science and nonscience is not defensible on methodological grounds; his thesis that the Ptolemaic and Aristotelian theories about the world were scientific is based rather on sociological habit. But we must recognize that the issue concerning the incommensurability of successor theories is essentially one of epistemology and methodology. It must be resolved on those grounds.

A solution to the problem may be found by recognizing that the idea of a properly scientific or objective methodology for the investigation of phenomena in the world has been subject to diachronic modifications. Yet the motivation has always been the same regardless of the historical period: researchers have always been concerned to understand the world of experience, i.e., to say what phenomena really are (apart from subjective experiences) in

https://doi.org/10.1177/039219218803614303 Published online by Cambridge University Press

terms of their constituent structure, their future behavior and their ultimate significance. The astrologer and the physicist share similar intellectual interests, but they differ fundamentally in terms of methodology for it is methodology that determines a researcher's ontology and the epistemology invoked to defend it.

We can reiterate the above by stating that the idea of scientific knowledge developed in human history out of those activities which deliberately sought to understand (some would say interpret, explain, etc.) the world of phenomena. This activity seems to be universal as is exemplified in any explanatory (magical, astrological) or strictly scientific theory. But that which sharply distinguishes the scientific enterprise from other modes of investigation is the emphasis placed on the idea of evidence. In fact, the idea of "evidence", is, perhaps, the most important methodological criterion in scientific analysis. Of course, scientific investigation, though grounded in the empirical world, would have been a purely trivial pursuit were it limited to the mere recording of immediate observations. For example, there is no need to construct a scientific theory to know that fire burns.

Interpretation and understanding requires causal analysis, which, in turn, invokes the notion of evidence.

Let us now posit the epistemologically basic statement that "S is a scientific theory if S offers empirically confirmable and causally justifiable evidence for the set of phenomena P that it purports to explain." Thus, if some theory S purports to explain P, then manipulable and empirically causal chains should be shown to exist between S and P. It is the manipulable character of the causal chains (expressed as variables) that determines the falsifiability and predictive accuracy of the theory. It is on account of these requirements that the whole enterprise of modern science rests on the necessary availability of artificial and closed laboratory conditions. The theorist should then be able to demonstrate within the confines of the artificial laboratory that the theory repeatedly yields its expected predictions and that these predictions are explainable according to *empirically confirmable* causal phenomena. Given the requirement of repeatability for purposes of confirmation the quantitative expression of variables is necessary.

In those cases where an explanation of some phenomenon P is not fully certifiable by means of empirical evidence, then the theory may be accepted on partially instrumentalist grounds provided that laboratory manipulation deems it highly likely that the empirical structures of the theory's causal posits exist as defined. Furthermore, the acceptance of an instrumentalist theory gains in acceptability the greater its empirical content and predictive strength *vis-à-vis* alternative competing theories. This is indeed the case for highly theoretical vanguard theories in particle physics.

In the above discussion, I have argued that a genuine scientific theory must be capable of subjection to analysis under laboratory conditions. The variables of the theory must be capable of definition in precise and measurable terms. This requirement necessarily helps in the theory's confirmation and potential acceptability. These provisos signify, of course, that research programs in astronomy, archaeology, etc. are not sciences in the strict sense of the term but rather applied sciences. Our strict sciences are none other than physics, chemistry, molecular biology and other laboratory bound, simulation prone research programs.

The aim of this discussion is to demonstrate that Kuhn's thesis of the revolutionary path of science is flawed unless it can demonstrate adequate criteria of demarcation between science and nonscience. Kuhn's criteria of demarcation, which could be paraphrased that "S is a science if S solves puzzles and S's methodology is paradigm-bound," is problematic as we have demonstrated. The continued improvement in technology and instrumentation in human history has also led to the qualitative refinements in the methodology of science. This refining process has been gradual as is evidenced by its historical products, namely, theories of magical explanation, metaphysical and unsubstantiated theories, protoscientific theories and, finally, genuine scientific theories.

Note that whatever the limitations of instrumentation and theorizing imposed historically on researchers of the world of experience, the intent was always to understand and control that world. Yet intention to understand the empirical world is a necessity for the formulation of a scientific theory but it is not sufficient. A genuine scientific theory must also provide acceptable evidence for its claims.

There is, of course, the immediate epistemological problem of an adequate definition of "acceptable evidence." I believe that a viable solution to this problem is to be found according to the way in which knowledge has been accumulated scientifically. Consensus concerning knowledge claims about the world is usually established according to some form of instrumentation. If some theory T^o purports to explain E, then T^o would tend to be more favored consensually than any other theory T^1, if T^o can be shown to "control" E instrumentally. Evidence for this is had by the rapidity with which pharmaceutical or medicinal cures for illnesses have been adopted in societies where nonscientific cures were long practiced. Yet again, instrumentalism was the basis for the acceptability of those theories which were eventually displaced by genuine scientific theories. Such theories were generally accepted only because there was no other instrumentally more effective rival theory. A theory's evident predictive failures would be usually explained away by appeal to *ad hoc* assumptions. Thus, one could argue that the problem of "adequate evidence" is solved if it can be shown that the theory in question consistently yields what it purports to yield. As stated above, those theories which do not yield what they purport to yield may be accepted instrumentally but only at the price of continuing *ad hoc* modifications. Such theories could not possibly withstand a stringent test of falsifiability—one of the major criteria of a genuine scientific theory.

In this connection, those theories which Kuhn regards as scientific and which serve as the basis for his thesis concerning scientific change should not be so considered. Ready examples are the Ptolemaic, Aristotelian, and the phlogiston theories of the premodern period. In the case of the Ptolemaic theory it is useful to note that its claims about the structure of the universe were essentially hypothetical and not subject to controlled empirical test. It is indeed true that this theory was founded on observation but its fundamental assumptions were incorrect and its explanations ultimately proved to be vacuous. Thus, I cannot perceive any genuine structural differences between the Ptolemaic theory and pseudosciences such as astrology and phrenology. Some theorists would argue correctly that the Copernican theory offered no clear instrumentalist advantage over the Ptolemaic theory, and

https://doi.org/10.1177/039219218803614303 Published online by Cambridge University Press

that the basis, therefore, for its initial acceptance derived from extrascientific considerations.[9] I want to argue, however, that the reason for the survival of the Copernican hypothesis is that it was eventually modified and empirically confirmed by the quantitative research of Tycho Brahe and Johannes Kepler. In fact, I would prefer to regard the Copernican hypothesis as a qualitative assumption which formed the foundation of a later empirically confirmable scientific theory.

Similar methodological criticisms could be made against Aristotelian dynamics. There is no evidence that Aristotle carried out experiments in the sense required of modern research, nor is there evidence that the fundamental assumptions of his theories were empirically confirmable. To emphasize the notion that science progresses through developmental stages, consider the now discarded phlogiston theory. This theory, though eventually rejected in favor of the more modern Lavoisierian chemistry, may be viewed as being superior in terms of research methodology when compared to the much earlier theories of Aristotle or Ptolemy. But such a theory did not yet possess the features of a full-fledged scientific theory. One can also discuss the scientific tenor of the caloric theory in the same vein. Rumford's research demonstrated that there was no basis for assuming the existence of some causal substance which was not only invisible, but also imponderable. It was also shown that the so-called caloric fluid could not be created by friction—thereby further discrediting this theory.

A closer diachronic study of the history of science prompts us to argue for a stage theory of the progress of scientific theorizing. It seems rather that the path of the evolution of the scientific enterprise is more one of evolutionary change than otherwise. There is historical evidence for the following developmental stages: (1) a strict pragmatic empiricism concerning phenomena immediately accessible to the senses coexisting with theories which appeal to empirically unrelated and unconfirmable causal entities as explanations; (2) pragmatic empiricism is maintained but there is a tempering of the disposition to explain phenomena by appeal to causally nonconfirmable metaphysical entities. It is further

[9] See Kuhn, *Structure of Scientific Revolutions*, p. 154 and Paul Feyerabend, "How to Defend Society against Science," *Scientific Revolutions*, ed. by Ian Hacking, New York, Oxford University Press 1981, p. 165.

https://doi.org/10.1177/039219218803614303 Published online by Cambridge University Press

assumed that nature, though lively, demonstrates a remarkable consistency in terms of its behavior. In this context, measurement, classification, etc. are increasingly viewed as highly useful for the proper interpretation of nature. The natural philosophies of Ptolemy and Aristotle are examples of theories founded on these considerations; (3) improvements in technology and investigative methodology lead to the gradual transition from natural philosophy to protoscientific theories, i.e., theories which display characteristics not only of modern scientific investigation but also those of the period of natural philosophy and speculative analysis. Ready examples are the phlogiston theory, the caloric theory and the ether theory; (4) more recently, one has witnessed the qualitative change in methodology and analysis—i.e., controlled predictability, falsifiability, quantitative modes of expression for the theory's assumptions, guarded instrumentalism, but fuller confidence granted to realistic laboratory insulated theories, etc.—that characterizes the genuine scientific theory. This qualitative change effectively marked the end of the period of natural philosophy and the beginning of the dominance of natural science. One might also note the correlative development in the area of the investigation of human behavior: "moral philosophy" evolved into "moral science" which, in turn, gave way to the "social science" of our contemporary period.

It is for this reason that while there is an evident qualitative transition between stages (1) and (2), che crucial transition is between stages (3) and (4). It is this latter transition that establishes the proper criterion of demarcation between science and nonscience. It should not be surprising, therefore, that those genuinely scientific theories of stage (4) should be incommensurable with those of stages (1), (2), and (3), according to the evolutionary model proposed in this paper. This model is justifiable since it may be applied to the development of the major branches of natural science, i.e., physics, chemistry, and biology. In the case of biology—granted that we have already discussed cases of physics and chemistry—the historical evidence demonstrates a development from mere taxonomic classification to the qualitatively different models of contemporary biochemical genetics. The research methodology demanded of the former was that of a nontheoretical empiricism supported by inductive generalization while that of the latter is that of the controlled quantitatively based laboratory experiment concomitant with the necessary requirements of predictability, falsifiability, etc.

It is for this reason that the research of Aristotle and, say, Lamark with regard to biological phenomena cannot be properly regarded as scientific while that of their contemporary homologue in the field of biochemistry can. It would appear, therefore, that Kuhn's thesis of theory incommensurability *vis-à-vis* successor revolutionary theories cannot be supported and that the idea of epistemological relativity is not applicable to the enterprise of modern scientific research. Rather, each novel and successful scientific theory can be shown to be logically consistent with its predecessor as the former explains the latter as a special case, i.e., with the appropriate structural adjustments.

The epistemological dilemma as to how to reconcile Kuhn's claims about *gestalt* shifts and revolutionary change is now resolvable by limiting the idea of science only to those well-confirmed theories that established the cumulative nature and continuity of scientific knowledge. In fact, it appears that one of the problems entailed by Kuhn's thesis and not fully answerable by him is the question of whether progress in science may be theoretically defensible. For if the chief criterion of demarcation between some scientific theory T and nonscientific theories is that T possesses its own paradigm and solves puzzles according to the requirements of its theorists, then what rational defences are there against the possibility of some previous theory T^o already replaced by T^1 eventually dislodging the latter at some later date? For if the Ptolemaic and Copernican theories are incommensurable, then what arguments could one bring against those theorists who at some future date choose to support the Ptolemaic paradigm or even the flat-earth theory.

In order to avoid this possibility, Kuhn's thesis needs, according to Wolfgang Stegmuller,

> the *introduction of an adequate concept of scientific progress for the case of theory dislodgement.* Only in this way, it appears to me, can we avoid the Scylla of teleological metaphysics and the Charybdis of relativism.[10]

[10] Wolfgang Stegmuller, "Occidental Theory Change", in *Paradigms and*

Stegmuller's concerns here are important and justified but his solution to the problem can aptly be described as epistemological agnosticism. He claims that if two incompatible theories T^1 and T^2 succeed some theory T^o to which both are reducible or "approximately imbeddable," then "we have here a juncture at which *ultimate*, not provisional, *value judgments* must decide which route to take, or whether both such paths should be pursued." (*Ibid.*, p. 89) But this approach would certainly yield the epistemological impasse that he and others have attempted to overcome. Yet the empirical historical evidence belies this theoretical possibility. As Stegmuller himself put it:

> For even if progress branching [i.e., the above discussion concerning T^o, T^1, and T^2] has occurred, it was presumably quite rare. But why? I know of no general answer... The *real* problem here is to explain *why such branching is much more rare than one would expect.* I have only the vague idea that an adequate answer will involve peculiarities of human nature as well as internal and external factors.[11]

The answer to the question in the above discussion is that the aim of scientific investigation is to formulate theories with greater empirical content and increasingly pragmatic yield, fortified by demonstrable and repeatable sensory evidence. It is this self-definition of science that provides for the cumulative growth of its yield and safeguards it against the "branching" possibilities suggested by Stegmuller. It should be recognized that the historical development of science was characterized principally by improvements and refinements in methodology. These improvements were determined by the increasing efficiency guaranteed by the technology of the available experimental design.

In this context, it is useful to point out that the "method of hypothesis" (the hypothetico-deductive method), though espoused initially by eminent researchers such as Huygens, Descartes, and Hooke, was later replaced by the inductivist approach formulated by Bacon and exemplified by the Newtonian methodology. It is

Revolutions, ed. Gary Gutting, Notre Dame, University of Notre Dame Press, 1980, p. 87.

[11] *Ibid.*, p. 89.

https://doi.org/10.1177/039219218803614303 Published online by Cambridge University Press

quite interesting though that the hypothetico-deductive method is very much the accepted model of contemporary methodology, fortified as it is by its absorption of inductivism as a means of formulating predictive and explanatory laws. I am inclined to believe that one reason why inductivism was preferred to the "method of hypothesis" during the age of Newton is that the science of mechanics during that era dealt especially with macroscopic entities which were subject to direct observation under appropriate control conditions. However, as researchers sought to explain the behavior of nature at the microscopic level aided by an improved technology, the preferred methodology was increasingly the hypothetico-deductive method. Note though that the formulation of theories in the periods of pre-science and protoscience was determined not only by strict observation minimally aided by experimental technology, but also by speculative assumptions often founded on magic. Consider the following observations of the historian of science, E.J. Dijksterhuis:

> In discussing the science of Antiquity, astrology and alchemy had to be taken into account; so also in the present case, though now magic because of its close association with physical experiment, will have to be considered as well.
>
> The facts are rather that men who are now held in honour as precursors or pioneers of science—Albertus Magnus and Roger Bacon are two illustrious examples—were also up to a point adepts in the seductive art of magic, and it is better to leave it an open question whether the magic they practiced was white or black, Christian or diabolic.[12]

It is evident, therefore, that the emergence of genuine scientific theorizing entailed not only the cumulative growth of knowledge and theory commensurability but also methodologies of research qualitatively different from those employed by researchers in the periods of prescience and protoscience. The key theories to which Kuhn appeals to support his thesis were, no doubt, formulated according to research principles qualitatively different from those

[12] E.J. Dijksterhuis, *The Mechanization of the World Picture*, Oxford, Clarendon Press, 1961, pp.154-159.

that are acceptable for contemporary research. In fact, these important methodological transitions from pre-science through protoscience to modern science were witnessed by important methodological debates among the natural philosophers. Consider the research methodologies expounded in Newton's *Principia* and Descartes' *Discourse on Method.*

One does not get the impression that Kuhn has fully explored the methodological significance of these works in terms of the establishing of criteria of demarcation between science and nonscience. The eventual impact of these novel methodological treatises was to demonstrate that the aims of scientific research were no longer concerned with questions examining, for example, the nature of "being" or "purpose" but only with the structure and behavior of phenomena.

III

In the above discussion, I have attempted to show that some of the theories to which Kuhn appeals to prove his thesis of theory incommensurability cannot be viewed as scientific. But can a case be made for Kuhn's thesis with regard to those theories which were clearly scientific but were later superseded in favor of some other theory that proved to be more successful? I suggest an answer to this question in terms of the main thesis of this paper.

It must be recognized first that in the case of highly successful theories such as Newtonian mechanics, it is not the case that the research paradigm of this theory has been replaced by that of the Einsteinian and quantum mechanical paradigms. A student of modern physics could easily and consistently embrace these three theories simultaneously. It is generally argued, and correctly so, that classical mechanics can be consistently accomodated by the relativistic and quantum mechanical systems. The problem with classical mechanics *vis-à-vis* its successors is essentially one of range: its predictive scope is highly reliable for macroscopic bodies and finite distances and velocities. The notions of the reducibility and imbeddedness of one theory with regard to its cognate are relevant here.

The point made is further supported by the obvious example of the way in which students of chemistry are able to appreciate the elementary definitions of "acid," "base," (classical, Bronsted-Lowry and Lewis) all consistent with each other but justifying their differences in terms of the increased information produced by a more efficient technology. The discussion suggests that mature scientific research sets store by realism and values it more than instrumentalism which, to most researchers, serves essentially as a heuristic device until stronger claims could be made.

In the case of those theories which were clearly short-lived and eventually replaced by theories with which they were not consistent, I argue that no scientific theory is formulated, fully developed, and complete at the time of its conception. It develops rather according to the positing of various hypotheses on the part of research theorists, many of which would prove unacceptable, while others would prove to be explanatorily and predictively incomplete.

Ready cases in point are Bohr's theory of the atom which, though eventually shown incomplete, did serve a vital instrumental function in the development of the theory of atomic structure. The fact that Bohr's theory of the atom though falsified should not be viewed as "false", in the way in which Ptolemy's geocentric theory is false, is evidenced by the improvement of it by theorists such as Schrodinger and Heisenberg. Consider too the preliminary attempts at formulating the structure of the benzene molecule by Kekulé which, though problematic on account of the discrepancies in the lengths of its carbon bonds, served as the proper template on which the later confirmed benzene molecule structure was founded. The point is that we should distinguish between false prescientific theories and the initial developmental hypotheses of genuine scientific theories. This distinction relies greatly on our notions of methodology as our discussion demonstrates. Kuhn, apparently, makes no epistemological distinction between both kinds of theory.

Other theorists have criticized Kuhn's thesis of theory incommensurability, but only on the tautological grounds that science *must* be realistic,[13] thereby logically precluding the notion of equally valid incommensurable theories, or that progress in

[13] See W.H. Newton Smith, *The Rationality of Science*, Boston, Routledge and Kegan Paul, 1981, pp. 148-182.

science would not have been possible were theories immutably incommensurate.[14] One would have preferred an examination of the historical examples Kuhn himself presents as proof of his thesis. A possible explanation for the approaches taken by Newton Smith, Kordig *et al.* is that much of the criticism of Kuhn has been undertaken by philosophers of science, not historians of science. The former would, of course, be more interested in a discussion of Kuhn's thesis from the standpoint of contemporary methodology of science.

Yet what is interesting is that the methodologists in question do not pay much attention to the fact that contemporary methodology of science is the historical product of a process that is several centuries old: "science" (or rather natural philosophy) in the sense of Aristotle is not "science" in the sense of the contemporary researcher.

Contemporary methodological guidelines are more stringent in terms of the acceptability of epistemological and ontological claims than hitherto. If contemporary science rejects epistemological relativism and embraces realism as a methodological principle, then this is due to the more efficient modes of analysis and experimentation developed in a period of mature scientific research. I want to argue that if scientific researchers were burdened with the problem of incommensurable theories then cumulative progress would not have been possible. It seems to me that the best way to view the special feature of the scientific enterprise is to view it rather as a phenomenon which has evolved historically to achieve maturity finally. It would certainly be inadequate to explain a mature science and its accompanying methodology merely by formalist analysis. A fuller understanding of science requires not only studies of its logic but also of its diachronic path in history.

It is evident that what is argued for in this paper is some form of scientific realism. In the recent literature this approach to the appraisal of the scientific enterprise has been supported by

[14] Carl R. Kordig, *The Justification of Scientific Change*, Dordrecht, D. Reidel Publishing Company, 1971.

theorists such as Putnam[15] and Boyd.[16] The realist thesis is founded on the normative claim that scientific research should aim at establishing theories that increasingly correspond with the structure of reality. This thesis also makes the purportedly empirical claim that the observational and theoretical terms of a mature science refer, and that successive theories in a mature science contain earlier relevant theories as limiting cases.

This theory of convergent realism has been critically evaluated by some theorists on the ground that there are historical cases of successful theories whose purportedly referring terms were later shown to be nonreferring. As Laudan puts it: "Now, what the history of science offers us is a plethora of theories that were both successful and (so far as we can judge) nonreferential with respect to many of their central explanatory concepts."[17] But the examples which Laudan provides to support his claim such as the phlogistic theory, the caloric theory of heat, theories of spontaneous generation are not genuine scientific theories but rather proto-scientific theories. Laudan ought to recognize that the success of a purportedly scientific theory in terms of its popularity is not sufficient to render it scientific.

The problem with the realist's argument in connection with his possible responses to Laudan is not that his methodology is erroneous but rather that he is not sufficiently clear in establishing criteria demarcation between science and nonscience. Those scientific theories which the contemporary realist would describe as immature should, I believe, be described as protoscientific. An immature scientific theory, if "immature", is interpreted literally, is a scientific theory nevertheless while protoscientific theories have not yet developed sufficiently to be described as scientific theories. I admit that the distinctions made here are somewhat subtle but they are unavoidable when analyzing phenomena that undergo distinct developmental changes.

Laudan's response to the realist's query as to how there could be

[15] See Hilary Putnam, *Meaning and the Moral Sciences*, London, Routledge and Kegan Paul, 1978.

[16] Richard Boyd, "Realism, Underdetermination, and a Causal Theory of Evidence," *Nous*, Vol. 7, pp. 1-12.

[17] Larry Laudan, *Science and Values*, Berkeley, University of California Press, 1984, p. 121.

a rational explanation of the success of science except by appeal to some theory of realism and its implications concerning successor theories, is that

> Parts of science, including many immature sciences, have been successful for a very long time; indeed, many of the theories I allude to above were empirically successful by any criterions of which I can conceive (including fertility, intuitively high confirmation, successful prediction, etc.).[18]

But, in response to Laudan, the same argument could be made for theories normally not regarded as scientific. Surely astrologers, spiritualists, and even psychiatrists believe that their theories are fertile in terms of predictive and explanatory content. It is clear though that the idea of "science" *qua* science must be maintained in the midst of such epistemological confusion. The definition of science requires proper criteria of demarcation which would emphasize controlled experimentation and provide evidence of logical continuity between theories and their successors.

Laudan is clearly in error when he argues that one could make

> ...the *prima facie* plausible claim that there is no necessary connection between increasing the accuracy of one's deep-structural characterizations of nature and improvement at the level of phenomenological explanations, predictions, and manipulations. It seems entirely conceivable intuitively that the theoretical mechanisms of a new theory, T^n, might be closer to the mark than those of a rival T^o, and yet T^o might be more accurate at the level of testable predictions.[19]

If such a state of affairs were the case, then it would be contemporary, for how then could one rationally explain "progress" in the scientific enterprise? If Laudan's claim were valid, what epistemological incentive would there be to offer alternative theories to, say, the flat earth theory, or to reject religious or magical theories in favor of genuine scientific theories? Evidently, there is no third alternative between Kuhnian relativism

[18] *Ibid.*
[19] *Ibid.*, p. 122.

and some variant of epistemological relativism, as Laudan would argue. Yet the problem with the orthodox realist is that he is committed to a sociology of knowledge which hinders him from making the claim that those theories generally taken to be scientific are not really scientific according to proven methodological criteria. I shall explore this idea in the final section of this paper.

IV

In the above discussion, we have raised questions about Kuhn's thesis that the path of science in history is characterized by revolutionary change in which hitherto accepted paradigms are overthrown and replaced by others with which they are theoretically incompatible. Note too that Kuhn's thesis is not unique, it is shared to some extent—either in attenuated or exaggerated form—by Feyerabend, Lakatos, and Laudan. The interesting question is what is the basis on which the Kuhnian thesis is maintained, given that even his most severe critics are unable to respond adequately to his examples of revolutionary change except to make the normative claim that science, if it is to retain any credibility, cannot sanction a methodology of epistemological relativism. Recall from the above discussion that central to a resolution of this issue is the establishing of adequate criteria of demarcation between science and nonscience. Despite his thesis of epistemological relativism, even Kuhn himself finds it necessary to establish a criteria of demarcation. These were shown to be inadequate.

The problem derives from the purely sociologically normative thesis that genuine scientific research began with the Greeks. Kuhn writes, for example, that "only the civilizations that descend from Hellenic Greece have possessed more than the most rudimentary science." (*SSR*, p. 16) On this basis, the obviously speculative and natural philosophical theories of Ptolemy, Aristotle *et al.* are thereby labeled as genuinely scientific. The thesis for which I argue, however, is that the speculative and empirical research of the Greeks cannot be viewed as being qualitatively different from that of their precursors, contemporaries and immediate successors. (More specifically, I want to argue that the empirical and speculative research of the natural philosophers of classical Egypt and China was, for the most part, more impressive than that of the Greeks. The same may be said for the quality of research in ancient Babylon and the Islamic world. It was the sum of these research efforts that led to the emergence of science as a maturing phenomenon in the seventeenth and eighteenth centuries. It should be noted too that this transition period which heralded the maturation of science would not have been possible without the use of geometry and algebra, both of non-Greek origin. The Egyptians are responsible for the former while the origins of the latter are in the Islamic world.

Let us recognize that the thesis argued for in this paper is not altogether novel. Historians of science, such as George Sarton, E.J. Dijksterhuis, Alexandre Koyré, and Gaston Bachelard, have argued, in the tradition of Comte, that scientific knowledge progressed in cumulative fashion and that scientific research, as we know it, did not develop its special features until the nineteenth century. In the case of Bachelard, for example, there is a clear distinction between science and nonscience evidenced by the progressive nature of the scientific enterprise. This justifies grounds for epistemological optimism.[20] Yet Kuhn, Feyerabend, and Foucault have tended to undermine the notion that science progresses cumulatively. One witnesses here epistemological agnosticism with an accompanying pessimism. It is this *attitude* toward scientific research that prompts sociological analysis.

In the area of actual scientific research, this epistemological agnosticism is somewhat puzzling since the quantity of reliable knowledge generated by scientific research in the latter part of the twentieth century is indeed remarkable. For example, the sciences of genetics and molecular biology are practically burgeoning with new findings, explanations, and vigorous research efforts. One author, who refers to the theories of Kuhn, Popper, Feyerabend as irrational, believes that the popularity of the nonprogressive view of science derives from the breakdown of the classical Newtonian model in the early twentieth century.[21] This is an odd explanation

[20] Gaston Bachelard, *La Formation de l'esprit scientifique—Une contribution à une psychanalyse de la connaissance objective* Paris, Vrin, 1972.

[21] David Stove, *Popper and After—Four Modern Irrationalists*, New York, Pergamon Press, 1982.

since the practitioners of science themselves have not abandoned their enterprise; on the contrary, research continues unabatedly, and the message of science and the fruits of its efforts are continuously sought after.

A plausible explanation for the epistemological estrangement between Kuhn's theory of scientific change and that of those theorists who espouse the view that the growth of scientific knowledge is cumulative is that the traditional formalist models of scientific methodology are both atemporal and normative in structure while the history of science has not generally been written according to any strict methodology. This is the essence of the conflict between the ideas of the context of justification and the context of discovery. A resolution of this dichotomy requires that the idea of the context of discovery be integrable with the idea of a criterion of demarcation.

V

The thesis defended in this paper seeks to restore the idea that there is no genuine incompatibility between the idea of scientific research as an enterprise that has as its goal realistic theories about the empirical world and the idea that scientific knowledge was the final culmination of efforts begun at the inception of human history but developed according to the principle of trial and error until the formulation of genuine scientific theories. The preparatory period was that of natural philosophy as practiced by almost all researchers until it developed fully into scientific research having jettisoned whatever magical and metaphysical content it contained.

In the above discussion, we took issue with Kuhn's thesis of theory incommensurability as an integral part of scientific change. It was argued that if the idea of what constitutes a science was modified to conform to contemporary methodology of science, then Kuhn's thesis would falter: the examples that Kuhn offers as proof of theory incommensurability could not, on those criteria, be regarded as genuine scientific theories. Kuhn's error, it seemed, derived from the erroneous sociological assumption that genuine science began with the Greeks. I argued on the contrary that if the natural philosophy of the Greeks were genuinely scientific then so too would be the natural philosophy of their precursors and contemporaries. I argued that science is distinguished necessarily from nonscience in that its knowledge claims were cumulative. True science, therefore, is a product of the modern era.

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