INCOMMENSURABILITY REVISITED

By

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To

My Great Motherland, China

My Beloved Mother, Shuzhen Zhang

My Wonderful Wife, Panying Lin

ABSTRACT

In the dissertation, I propose to consider the vicissitudes of Thomas Kuhn's historical approach to science with an eye to clarifying his controversial notion of incommensurability. The emphasis of the thesis is in the main on the epistemological significance of Kuhn's insight of the incommensurable rather than on philosophy and history of science.

Although the notion of incommensurability, one of the most significant results of Kuhn's *The Structure of Scientific Revolutions*, has been much criticized in recent Anglo-American philosophy, I argue that it incorporates insights that are still worth reviving and defending. Moreover, since these insights derived from Kuhn's syntheses of the ideas of thinkers such as L. Fleck, A. Koyré, L. Wittgenstein, N. Hanson, M. Polanyi, H. Gadamer and P. Feyerabend, my defence of Kuhn's concept of incommensurability provides a review of some profound issues in

recent Anglo-American philosophy. Also in this connection, I assume that the "Kuhnian Revolution" in philosophy of science in the 1960s did not occur by chance in the avenue of contemporary American philosophy. Rather, the appearance, the reception and the subsequent criticism of *The Structure of Scientific Revolutions*, including Kuhn's later conversion to the "analytical tradition", must be interpreted in their respective philosophical contexts.

To be precise, I maintain that the debate between the Kuhnian school and its opponents should be examined historically in the light of the deeper and broader cultural and philosophical issues involved, issues that even Kuhn seems not to have fully appreciated. Through investigating the debate, I argue that the critiques of Kuhn's original ideas, including his own later objections to the ideas, presuppose as well as impose the analytical standard of legitimatization that underlies present-day Anglo-American ways of reasoning. It is this standard, however, that presupposes the very requirement whose feasibility Kuhn's notions of paradigms and incommensurability put into question.

More specifically, I defend Kuhn's historical approach to philosophy of science in 1962 against those criticisms which are guided merely by ahistorical and linguistic requirements of analyzability, and criticize Kuhn's acceptance of the precepts of the analytical tradition since 1969. I maintain that his conversion is neither necessary nor successful.

On the other hand, with reference to Kuhn's changing views since 1962, I suggest that it might be time to reconsider the virtues of Kuhn's forerunners such as J. Dewey, E. Husserl, L. Fleck, L. Wittgenstein and M. Polanyi, whose approaches are nowadays usually ignored by mainstream philosophers of science. My aim is thus not to develop a new view but to reinforce some "out-of-date" insights of these thinkers and to clarify and strengthen Kuhn's 1962 stance. In this, the thesis is contrived to throw light on the current epistemological issues concerning those of language and meaning in understanding and communication.

Central to this research is the view that there are phenomena of incommensurability in the process of communication and understanding and that no account of rationality is valid unless it recognizes the development of reason in history. In light of this, I hold, the analytical approach should always allow a historically-oriented vision in order to detect the real picture of our everyday thinking. To understand different rationalities in history and culture, philosophy must go to the history of sciences and to the real process of our everyday thinking.

Thus I take Kuhn to have been right when he argued that there exist fundamentally incommensurable theory breaks in history such as between Aristotle's physics and Galileo's mechanics. Based on this conviction, I defend the phenomenon of incommensurability, although admitting that there is always the need and possibility of information exchange and transformations from one tradition to the other. Moreover, I contend that the life of the notion of incommensurability is rooted in the norms of rationality and various practices of sciences in history, norms which are constituted in the process of applying our thoughts to particular problems as well as in cultural exchange. This means that the conception of incommensurability cannot be simply resolved by the requirement of the philosophical analyzability or 'translatability', in spite of the fact that the conception has been theoretically abandoned and transformed into linguistic commitments even by its inventor himself.

Methodologically, I approach the vicissitudes of Thomas Kuhn's notion of incommensurability in two ways: in Part I by historical case studies of Kuhn's changing views, and in Part II by philosophical reflections on those consequences of the historical case studies.

In Part I, I expose Kuhn's discovery of the incommensurable phenomenon, his changing views since 1962, and the debate between him and his opponents. These changes are divided into several stages to give a clear picture of the process of Kuhn's establishment of the notion of incommensurability and his retreat since *The Structure of Scientific Revolutions* appeared, as well as the unsettled philosophical debate about the notion.

In Part II, which is principally critical, I analyze Kuhn's changing views and criticisms of his opponents step by step. With recourse to the historical case studies in Part I, I consider the problems of those shifts and criticisms, and simultaneously provide my alternative interpretations of communication breakdowns, all with an eye to strengthening Kuhn's early approach to incommensurability.

The upshot of my argument is that incommensurability is not a rare event. Rather, the phenomenon reflects the fact that there are rationality breaks in history and the limitation of human rationality at certain time and location. Thus it is plausible to hold that incommensurable changes in the conception of reason occur, i.e. that the standard of rationality in history and culture is not set up once for all and we should not expect to find a universal standard in communication and understanding capable of guaranteeing information exchange regardless of the nature of the discourse, history and culture.

In my conclusion, I raise some deeper worries and make some modest suggestions regarding contemporary Anglo-American philosophy, in particular, contemporary Anglo-American epistemology.

This method of approaching the thesis reflects my view that to understand a philosophical notion, one has first to be aware of its history and the culture in which it develops, and also, tackles it with recourse to one's living experience. In short, philosophers have to examine and accept the cogency of everyday thinking in history.

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Finally, I thank my son, Xia Li, who has patiently waited my promise of our family trip to Disney World while knowing how much the whole family suffers due to father's insistence on finishing his Ph.D dissertation.

LIST OF ABBREVIATIONS

CCC	Commensurability, Comparability, Communicability
CGK	*Criticism and the Growth of Knowledge*
CR	*The Copernican Revolution*
ET	*Essential Tension*
FR	*Farewell to Reason*
PR	*Paradigms and Revolutions*
PS	Postscript -- 1969
RMC	Reflections on My Critics
PSA	*PSA 1982*, Volume 2
SSR	*The Structure of Scientific Revolutions*
SSRE	*The Structure of Scientific Revolutions* (Enlarged Edition)
SST	*The Structure of Scientific Theories*
STP	Second Thoughts on Paradigms
TM	*Truth and Method*

1

Introduction

Thomas Samuel Kuhn is generally regarded as one of the most influential philosophers and historians of science of the twentieth century. Yet, general agreement about the meaning of his work, *The Structure of Scientific Revolutions*,[1] has by no means been reached. Kuhn's appearance in the philosophical arena has called forth not only influential but also controversial interpretations. Surprisingly, even he has later found himself wondering about the

[1] Thomas S. Kuhn, *The Structure of Scientific Revolutions*, the University of Chicago Press, 1962; second edition, enlarged, 1970.

real intent of his earlier approach, in particular, about his notion of paradigm and its related concept of incommensurability on which both the influence and the controversy have mostly centred. While many intellectuals outside the philosophical arena have become interested in transplanting the Kuhnian perspective on science into various academic areas, Kuhn and his philosophical opponents reproach one another with having not "understood" the philosophical meanings of these two notions. This was certainly a remarkable fate for a philosopher of science who had many years earlier become convinced of the existence of "incommensurability" in the history of science and who cherished the hope of putting an end to the ahistorical ambition of philosophy of science.

There have been many attempts to approach Kuhn's philosophy on the basis of existing philosophical traditions. In particular, affinities between his hermeneutics of science and certain themes in European philosophies have been noted while close relations between Kuhn and Anglo-Saxon thinkers such as M. Polanyi, N. R. Hanson, Willard V. Quine, Benjamin L. Whorf etc. have also been acknowledged.[2] Still, at present Kuhn's approach is generally interpreted within Anglo-American philosophy in anti-rationalist

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[2] The philosophical background of *The Structure of Scientific Revolutions* is profoundly sketched in Daniel G. Cedarbaum's essay, "Paradigms", which traces Kuhn's philosophical heritage from Georg C. Lichtenberg, Ludwig Wittgenstein, and Ludwik Fleck etc. (see Cedarbaum's essay in *Studies in the History and Philosophy of Science*, Vol. 14, No. 3, 1983, pp.173-213). As a matter of fact, Kuhn himself admits that his synthesis of philosophy of science significantly benefited from reading Continental thinkers such as Alexandre Koyré, Emile Meyerson, Hélène Metzger, Anneliese Maier, Arthur O. Lovejoy, Jean Piaget, and Gestalt psychologists, etc. (See his "Preface" to *The Structure of Scientific Revolutions*.)

tone against which Kuhn himself has protested repeatedly. He complains that his philosophy of science had been subjected to many misunderstandings because it had not been properly understood. For many analytical philosophers, however, the historic debate on Kuhn's thought has been triumphantly settled since as early as in 1969 when the Illinois Symposium on *The Structure of Scientific Theories* was held.[3]

A complicating factor is that Kuhn's thinking has never stood still. No concept in the contemporary history of philosophy of science has altered so dramatically as Kuhn's notions of paradigm and incommensurability. In fact he has been substantially altering the meaning of the notion of paradigm while drastically reducing the revolutionary content of the other notion of incommensurability to cover a very limited sort of 'local' circumstance. Thus, concerning the line of 'development', there has been considerable confusion about Kuhn's intentions. The problem is not only that his use of 'paradigm' appears extremely ramshackle and omnipresent -- once even being analyzed into twenty-one categories[4] -- it is also that Kuhn has surreptitiously retreated from his original use of this notion to a concept that even he himself does not quite know how to explain. Indeed, the question "What is Kuhn's approach to science?", which has been asked many times and answered in various

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[3] See F. Suppe's 'Conclusion' to the second edition of his edited book, *The Structure of Scientific Theories*, University of Illinois Press, 1977, pp.729-730.

[4] Margaret Masterman, "The Nature of a Paradigm", in *Criticism and the Growth of Knowledge*, (ed.) by Lakatos and Musgrave, Cambridge University Press, 1970, pp.59-89.

ways, is still unsettled. To understand the influence, controversy as well as modifications of Kuhn's key notions, case studies of their development are valuable. These case studies should highlight some indispensable issues of the recent philosophical trends and the problems of contemporary analytical philosophy, especially those of contemporary American philosophy of science and epistemology. Given that those issues and problems have not been accorded enough attention either by Kuhn's adherents or by his opponents, I believe that a reconstruction of Kuhn's notion of incommensurability is by no means an 'out-of-date' topic in contemporary epistemology and philosophy of science.[5]

This study is written in the conviction that only a thorough analysis of the vicissitudes of Kuhn' historical approach to science can clarify his controversial notions of paradigm and, especially, incommensurability. Also, only through such an analysis will the real problems of Anglo-American philosophy of science and epistemology today emerge.

In particular, I plan to demonstrate that Kuhn's perspective of incommensurability offers points of contact for various epistemological interpretations of our everyday thinking. Thus I presume that Kuhn's approach must be seen first and foremost as an epistemological achievement, one that ought to be understood on the cognitive basis of its own development and problematic. I attempt to show that although various charges of Kuhn's notions can detect

[5] Cf. SST, p.5 and pp.633-634. I should note that among analytical philosophers Kuhn's view of "local incommensurability" is still widely studied.

flaws of his changing views, they have only tried to arrest and isolate his thought at a particular stage of its development. In addition, they do not realize that Kuhn's vision in his youth was actually far ahead of that of his contemporaries with regard to diagnosing difficulties in the epistemology of science and in philosophy in general.[6]

Although there appear to be two Kuhns, the young one and the later one after 1962, both are concerned with a single theme, even if this involves a regressive development. In other words, there is a definite major epistemological theme in Kuhn's philosophy of science that he never consistently unfolded, and consequently the later phase of his thinking cannot be regarded as satisfied resolution of all his previous experience while approaching the history of science. In this regard, I shall point out that it was Kuhn's personal enlightenment in 1947 that gave him the philosophical insights with which he was to be concerned throughout his academic career. And also, the enlightenment drove him to more radical alternatives and consistency in order to sustain the unity of his various views.

In this, throughout my analysis I shall detail the history of Kuhn's conceptual changes in which Kuhn's texts are accepted as the only authority. I hope to avoid anachronistic interpretations of Kuhn's understanding of science. As I see it, the well-known

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[6] As a matter of fact, Kuhn's first book, *The Copernican Revolution* (Harvard University Press, 1957), has more insights than SSR and research of the book is still needed into the significance of Kuhn's "Copernican Revolution" in 1947.

Kuhnian question "Similar with respect to what?" is itself in need of a historically justified interpretation to fully understand the unity of his approach. The procedure I have chosen then is to make it possible to discern Kuhn's early work as a matrix vis-à-vis the later development, and the later work as an unfortunate response to the problematic raised by his earlier insights of incommensurability.

For this reason, I take it essential to conduct the extensive and sometimes detailed case investigation into the particular theme of incommensurability. Because the hermeneutic precept that understanding belongs to the being of that which is understood holds for Kuhn's philosophers of science as well.[7] That is, understanding Kuhn is never subjective process toward a given 'rational object', but toward its effective history -- the history of its influence and encounters. Only when we have shared this vision of understanding Kuhn will we be able to grasp the key to the difficult problems with which Kuhn's approach presents us, e.g., the meaning of his historicism about science and the problematic notions of incommensurability and paradigm, etc. What is more, the debate over rationalism and irrationalism caused by the appearance of Kuhn's work, a still portentous philosophical topic, can only be well understood by case studies specifically focused on the basis and development of the inner importance of a particular phase, Kuhn's 'enlightenment' in 1947.

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[7] Cf. Hans-Georg Gadamer, *Truth and Method*, (trans.), The Seabury Press, 1975, p. xix.

Consequently, I will follow Kuhn's career as far as his transition to linguistic philosophy of science in 1982, which must be regarded as the most confusing point of his entire philosophical development. I am well aware that this point was at the same time an opening for perceiving Kuhn as a whole again, while he himself held that it was a true departure from his commitment to paradigms, scientific revolutions, normal science, and so on. In methodological respects, however, Kuhn's thinking in 1982 reached a moment of departure from his early hermeneutic approach to the history of science, when he still hoped to retain the concept of incommensurability at the expense of reducing its philosophical consequences.

I agree that even the left-over notion, the 'local incommensurability', remains of immense importance. But I also believe that Kuhn's 'enlightenment' in 1947 and its product, the perspective of incommensurability, are the "promised land", to use Husserl's metaphor. This idea, which Kuhn first contributed to the contemporary epistemology, has been unfortunately lost by both his adherents and opponents, and is badly in need of further cultivation in terms of epistemology.

To be more precise, in my discussion of the theme, many important subjects from Kuhn's earlier approach will be examined only in their reference to the deliberation of incommensurability. For I think that once the key notion of incommensurability is clarified, much of the confusion over Kuhn's other themes can be understood in an entirely different way. That is, I shall argue that it is the young Kuhn who had the most to contribute to contemporary epistemology and that the debate on Kuhn's philosophical approach should be resumed in its historical context. So in the course of my exposition, I will repeatedly defend the earlier Kuhn against the later Kuhn and his critics and interpreters.

On the other hand, I sympathize that Kuhn's complaints about superficial interpretations of his work were not unjust. For I regard that it ought to be pointed out that his opponents can never write adequate critiques until they have fully understood what they are criticizing, and have grasped the nature of Kuhn's 'enlightenment' in 1947. In a nutshell, I shall argue that the critiques of Kuhn's original ideas, including his own later objections to the ideas, presuppose as well as impose the analytical standard of legitimatization that underlies present-day Anglo-American ways of reasoning. This is a problem if only because this. For the feasibility of this standard has been put into question by Kuhn's own notions of paradigm and incommensurability.

My defense of the young Kuhn should not be taken to suggest that I share his philosophical intentions in every respect. It is my conviction that the debate between Kuhn's adherents and his opponents should be examined historically in connection with deeper and broader cultural and philosophical issues, issues that even Kuhn seems not to have fully appreciated. In other words, I consider that Kuhn himself has not been aware that his 1947 discovery of incommensurability reveals profound epistemological issues already in the western philosophical tradition, issues which have been abandoned by contemporary western philosophy. Thus for me, it is more the way that Kuhn discovered the incommensurability, his experience of 'enlightenment' in 1947, than the implication of the notion that prompts me to defend the young Kuhn's approach to philosophy of science. For Kuhn's experience of 'enlightenment' is so intrinsically illuminating that philosophy can no longer be an enterprise alien to the real way of our everyday thinking. In light of this, I take a pragmatic understanding of human thinking as my primary attitude challenging the contemporary epistemology.

In particular, I shall be defending the young Kuhn's historical approach to history and philosophy of science against the criticism from the analytical tradition which is guided only by ahistorical and linguistic requirement of analyzability. I shall further contend that Kuhn's conversion to the precepts of the analytical tradition since 1969 is neither necessary nor successful. Moreover, I suggest, with recourse to my analysis of Kuhn's changing views since 1962, that it is time to reconsider the virtues of some of Kuhn's forerunners such as E. Husserl, L. Fleck, A. Koyré, L. Wittgenstein, P. Bridgman, J. Dewey, and M. Polanyi etc. whose approaches are often ignored by mainstream philosophers. In this, I hope that my research can throw light on the current issues of epistemology, especially those involving language and meaning in the process of understanding and communication. To be more precise, by addressing the relevance of illumination or 'enlightenment' to our experience of knowing, I anticipate to contribute to reorienting epistemology as a philosophical branch of everyday thinking.

Central to my research is the view that there are phenomena of incommensurability in the process of understanding and communication. I hold that the analytical approach should always give way to a more historically-oriented vision in order to inspect the real picture of our everyday thinking. For example, as the early Kuhn once argued, there exist fundamentally 'incommensurable' theories such as Aristotle's physics and Galileo's mechanics, Copernican and Ptolemaic astronomy, Einstein's theory and Newton's *Principa*, and so on, to say nothing, I might add, of the relation between Chinese traditional medical theories and Western medical science, or between Oriental ideas of knowledge and Anglo-American theories of epistemology. This is the ground that I contend that the young Kuhn's notions of incommensurability and paradigm are rooted in the research of norms of rationality and different experience, i.e. in actual historical practice rather than in the requirement of philosophical analyzability. So, in spite of the fact that the notion of incommensurability has been theoretically abandoned or transformed into linguistic commitment even by its inventor himself, I regard the phenomena of incommensurability to be constituted in the very process of applying our thoughts to understand particular problems in history as well as those in cultural exchanges and communications.

Methodologically, I approach the vicissitudes of Kuhn's philosophy of science partly by case analyses of his changing views, and partly by reflecting on the results of the case analyses. In Part One, which is mainly historical case studies, I expose the stages of Kuhn's changing views so as to give a clear picture of his establishment of the notion of incommensurability and his retreat and conversion since *The Structure of Scientific Revolutions* appeared, as well as the unsettled debate about the notion. In Part Two, which is mainly critical, I reflect on Kuhn's changing views and criticisms of his opponents. With recourse to the case analyses in Part One, I reconsider the nature of the debate about Kuhn's SSR while providing my alternative of incommensurability to strengthen Kuhn's approach in the 1960s.

In my conclusion, drawing on both the historical case studies and my reflection on Kuhn's shifts and the debate about *The Structure of Scientific Revolutions*, I raise some deeper worries and make some modest suggestions regarding the problems of contemporary Anglo-American philosophy in hope of resurrecting the debate over Kuhn's approach to history and philosophy of science.

My final assessment will be that the young Kuhn's view of incommensurability incorporates insights that are still worth reviving and defending; the appearance, the reception, and the subsequent criticism of SSR, including Kuhn's later conversion to the analytical tradition, can be well interpreted in terms of their philosophical and cultural incommensurability; thus both epistemology and theory of knowledge should always answer questions of the incommensurable raised by scientists and common people in the course of their everyday endeavours.

PART I

On Kuhn's Changing Views

2

Kuhn's Epistemological Crisis and the Discovery of Incommensurability

> ***The fulfilment of what exists potentially, in so far as it exists potentially, is motion*** --- namely, of what is alterable ***qua*** alterable, ***alteration***: of what can be increased and its opposite what can be decreased (there is no common name), *increase* and *decrease*: of what can come to be and can pass away, *coming to be* and *passing away*: of what can be carried along, *locomotion*.

It was this bizarre definition of 'motion' that perplexed Kuhn when he was reading Aristotle's *Physics* in 1947.[8]

However, Kuhn's reading of Aristotle on motion and mechanics gave rise to a fortunate epistemological crisis in his then vision of science, one that led him to an epistemological "revolution", in particular, to his discovery of incommensurability.

It all happened, as Kuhn recalled in 1977, in the summer of 1947 when he was a Harvard graduate student of theoretical physics within sight of the end of his dissertation, and when the president of Harvard, Dr. James Bryant Conant, asked him to interrupt his Ph.D study for a time in order to assist Conant with teaching *Natural Science 4*, an experimental college course treating history of science for non-scientists at the level of freshmen or sophomores.[9]

For Conant, Kuhn's involvement in teaching *Natural Science 4* was an important part of his ambitious plan of Harvard curriculum innovation in the 1940s.[10] By and large, as the president of

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[8] Cf. Vol.II of *The Works of Aristotle*, *Physics*, Book III.i 10., (under the editorship of) W. D. Ross, Oxford: The Clarendon Press, 1930.

[9] To know more of the importance of the experimental college course, see Chapter 27 of James B. Conant's memoirs, *My Several Lives*, Happer & Row, Publishers, 1970, pp.363-373.

[10] Curious why Kuhn was asked to interrupt his study and eventually become a professor of history of science, I encountered a pool of new bibliography which Kuhn, surprisingly, has seldom mentioned. My several days in the library of tracing the reason for Kuhn's transference were as exciting as the discovery that his autobiographical remarks are not always reliable. This adventure between books makes me understand more of his "Copernican Revolution" in 1947 than he himself has informed to the public.

Harvard, Conant had been worried about two things during the early 1940s. One was the fact that the Second World War had actually put Harvard at "war", that it was "rapidly turning Harvard into an institution concerned only with winning the war".[11] The other was the mood of "intellectual anarchy" which, Conant considered, had put Harvard in need of reform, something that had found little favour among Cambridge academic circles for a long time. But to get out of this "chaos" at Harvard, in the first place, Conant was aiming at the resolution of the curriculum reform. Foremost in Conant's reform agenda was the mission of the liberal arts curriculum. Conant's philosophy was already succinctly presented in his Tercentenary Oration as early as 1936:

> The older educational discipline, whether we like it or not, was disrupted before any of us were born. It was based on the study of the classics and mathematics; it provided a common background which steadied the thinking of all educated men. We cannot bring back this system if we would, but we must find its modern equivalent. Like our ancestors we must study the past, for "he who is ignorant of what occurred before he was born is always a child." In my opinion it is primarily the past development of our modern era which we must study and study most exhaustively and critically. We must examine the immediate origins of our political, economic and cultural life and then work backward. We must not, however, spread the inquiry over so wide a range that the average man will obtain only a superficial knowledge. It does not seem to me to be a step in the right direction to dip our children first in one barrel of tinted whitewash and then in another. The equivalent of the old classical discipline is not to be found in a bowing acquaintance with universal history and general science, and an exposure to scattered examples of art and literature. Our present educational practice, which insists on the thorough study of at least one discipline,

[11] James B. Conant, *My Several Lives*, Happer & Row, Publishers, 1970, p.364.

is certainly sound.[12]

This was Conant's primitive motivation, studying histories, that he was strove for realization of the programme of General Education in a Free Society. In the process of carrying out the programme, for which Conant found few supporters either in 1936 or thereafter, *Natural Science 4* played a key role after 1944.[13] Thus, it was crucial for Conant to invite three young men, F. G. Watson, L. K. Nash and T. S. Kuhn, to recruit a strong teaching group devoted to fulfilling his aim of cultivating future American citizens an appreciation of the advancement of knowledge in modern times while combating the prevailing conservativism of Cambridge.[14]

As part of *Natural Science 4*, Kuhn undertook the role of lecturing on the origins of seventeenth century mechanics. Fortunately, it was this teaching opportunity that prepared two of Kuhn's own "Copernican Revolutions": one is the Kuhnian 'Copernican Revolution' in the summer of 1947 which I shall detail in following sections; the other his 1957 book, *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought*, which to a very large extend heralded the work that made him famous, *The Structure of Scientific Revolutions*.

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[12] Ibid, p.656.

[13] Cf. ibid, pp.372-373.

[14] Of the three, only Kuhn was a doctoral candidate. The two volumes of *Harvard Case Histories in Experimental Science* (edited by James Bryant Conant, Harvard, 1948 and 1957) were compiled for the course of *Natural Science 4*.

The seventeenth century was the cradle of modern western science which produced such great scientists as Harvey, Kepler, Descartes and, of course Galileo and Newton. So, when preparing to lecture on the origins of seventeenth century mechanics, Kuhn started first to trace out how much about mechanics has been known by the predecessors of Galileo and Newton. His inquiries soon led him to the seventeenth century's discussion of motion in Aristotle's *Physics* and works descended from it. What was in his mind when reading those works were questions so as: "How much about mechanics was known within the Aristotelian tradition, and how much was left for seventeenth-century scientists to discover?" (ET, p.xi) Against his training in Newtonian mechanics, Kuhn surprisingly uncovered that Aristotle's explanation of projectile motion as well as his definition of motion, which is cited at the beginning of the chapter, had little in common with modern mechanics founded by Galileo.[15] Rather the reverse, Aristotle's impetus theory of motion seemed to Kuhn to be one of the weakest components in the body of his physics:

> Even at the apparently descriptive level, the Aristotelians had known little of mechanics; much of what they had had to say about it was simply wrong. No such tradition could have provided a foundation for the work

[15] Of all Galileo's contributions to mechanical conceptions, the most fundamental was that the continuous application of a force produces either an increment or decrement of velocity *at every moment*. The conception of acceleration as a constantly changing velocity *accompanying the application of force* was in contradiction to the Aristotelian principle that terrestrial bodies tend *their own nature* to come to rest at a level which is natural for them. (Cf. C. Singer, *A Short History of Scientific Ideas To 1900*, Oxford University Press, 1959, p.250).

> of Galileo and his contemporary's. They necessarily rejected it and began the study of mechanics over again. (ET. p.xi)

However, this unexpected impression was not yet Kuhn's discovery. For this sort of generalization had been shared by most earlier historians of science.[16] There is no doubt that Kuhn's first encounter with the history of science not only painfully undermined some of his "basic conceptions about the nature of science" (SSR, p.v) and scientific training, but also radically challenged his long-standing avocational interest in philosophy of science.

We do not exactly know what Kuhn's basic conceptions of the nature of science and of the philosophy of science were when he was a Harvard graduate student in the 1940s. Nevertheless, while he was studying at Harvard, both physicists and philosophers at the university were reflecting upon the success of Einstein's theory of relativity and trying to understand it in terms of logical empiricism. In the department of physics at Harvard, P. W. Bridgman had not only been teaching science, but also promoting his

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[16] For example, H. S. Williams wrote in 1904 that Galileo's mechanical discoveries "were the natural output of his own creative genius" and the "onslaught upon the old Aristotelian ideas" (cf. *The Beginnings of Modern Science*, The Goodhue Company, 1912, p.93). See also W. C. Dampier's *A History of Science*, Cambridge University Press, 1929-1961, p.132; B. J. Harvey-Gibson's *Two Thousand Years of Science*, A & C Black, 1931, p.43; C. E. K. Mees' *The Path of Science*, John Willey & Sons, 1946, pp.88-89; and A. R. Hall's *The Scientific Revolution*, The Beacon Press, 1954, pp.75-77.

philosophy of science, operationism.[17] At the same time, since the fall of 1939, Philip Frank,[18] who had come to the Harvard physics department, had also been involved in Conant's experiment in the General Education Program, teaching philosophy of science, and never let the interaction between science and epistemology stay out the centre of his students' attention.[19]

Also, under the general leadership of Frank and Bridgman, a "shop club" called "Unity of Science" was regularly held for many years in the Department of Physics.[20] As Gerald Holton describes the atmosphere in those meetings, "the logical empiricists among all these, contrary to the doctrinaire days of the 1920s and 1930s, laid as much stress as did all the others on fundamental,

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[17] Although he never talked with Bridgman about the latter's philosophical position, Kuhn recalled in a letter to me, he took an advanced thermodynamics course taught by Bridgman and ultimately read his book (see Appendix 3).

[18] Kuhn remembers: "With Philip Frank I did talk from time to time, and I also read some of his work. Probably he contributed more than anyone else to my somewhat erroneous impression of the positivist movement" (see Appendix 3).

[19] Cf. Philip Frank's *Preface* to his book, *Modern Science and Its Philosophy: History, Philosophy and Sociology of Science*, Harvard University Press, Cambridge, 1949, and also his introduction to this book, pp. 50-52.

[20] As Gerald Holton recalls that professors who often showed up at those meetings were such figures as H. Aiken, K. Deutsch, R. Jakobson, G. Kepes, P. LeCorbeiller, W. V. O. Quine, G. de Santittana, H. Shapley, B. F. Skinner, S. S. Stevens, L. Tisza, R. Von Mises, N. Wiener, and some occasional visitors such as E. Nagel and H. Margenau (cf. Gerald Holton, *Thematic Origins of Scientific Thought*, Harvard University Press, 1973, p.417).

interdisciplinary discussions".[21]

Manifestly, the "shop club" meetings of "Unity of Science", and the philosophical atmosphere at Harvard still fostered the spirit of logical empiricism as well as of pragmatism, the spirit which only deals with the statics rather than the dynamics of science.[14] It is why, although Conant had foreseen "the development of a national culture based on a study of the past"[15] and thereby had launched his reform to reconstruct the Harvard image to accord with the scientific spirit of the times, Kuhn had not escaped from the shade of operationism and logical empiricism to see science in terms of its historicity, as a flowing river rather than a frozen iceberg.[16] This was presumably one reason why Kuhn was profoundly shocked when attempting to interpret the historical relations

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[21] Ibid. p.41. That was the kind of scientific training of basic conceptions of the nature and philosophy of science in which Kuhn would have been immersed before 1947.

[14] As a matter of fact, Frank admits that only after 1947 had he stressed the point that science does give support to metaphysical interpretations. He recalls, "after that time, as a result of contact with my students and fellow teachers, I became more and more interested in the question of the actual meaning of the metaphysical interpretations of science-idealistic, materialistic, relativistic, and others" (cf. his book *Modern Science and Its Philosophy*, p.51).

[15] James B. Conant, *My Several Lives, Harper & Row,* 1970, p.656.

[16] For, unfortunately, Conant was one of those who concurred Sarton's view of accumulative scientific knowledge: "There can be no doubt, of course, that knowledge has been accumulated, classified, and digested to serve practical ends ever since the dawn of civilization" (*Science and Common Sense*, p.40). So, Conant speaks "conceptual schemes" in the history of science, actually it was quite a fashion term at that time, but he thought it in terms of different understanding of scientific development.

between seventeenth century physics and Aristotle's physics. Under the influence of logical empiricism, Kuhn at the beginning of his teaching *Natural Science 4* had proceeded as if there had been no physics in Hellenism at all or else one fully consistent with its modern development.

The second reason, the most substantial one, that puzzled Kuhn was due to his philosophical interest immersed at Harvard, especially the logical empiricist insistence on the "rule" of science that a theory should be logically coherent. Regarding this rule as an essential standard of science led Kuhn to assess the coherence and consistency of not only Aristotle's physics but also the entire body of his system:

> When dealing with subjects other than physics, Aristotle had been an acute and naturalistic observer. In such fields as biology or political behaviour, his interpretations of phenomena had often been, in addition, both penetrating and deep. How could his characteristic talents have failed him so when applied to motion? How could he have said about it so many apparently absurd things? And, above all, why had his views been taken so seriously for so long a time by so many of his successors? The more I read, the more puzzled I became. Aristotle could, of course, have been wrong - I had no doubt that he was - but was it conceivable that his errors had been so blatant? (ET, p.xi)

It is from these beginnings that Kuhn's 1947 epistemological crisis, which seriously questioned the current interpretation of the history of science, derives. In particular, his initial puzzlement was over whether mechanics has a history going back to Aristotelian times, or began only when science became "science", which then never changes at all. Connected with this confusion, his second puzzle was over whether Aristotle's physics can be categorized as science. If so, then what is the common standard by which to define the nature of science? If not, how can we explain the fact that Aristotle set up the foundation for several modern sciences such as logic and biology. In other words, Kuhn was bewildered by what had gone on in the history of science. Apparently, the logical empiricist generalizations about science has been put in question when they are applied to its history.

Historians of science in Europe had already noticed that science has a history quite different from the one imagined by logical empiricists.[17] But, for Kuhn in 1947, logical empiricists had been right in holding that scientific theory itself should be coherent. In terms of this, Kuhn's epistemological crisis appears rather complicated: from the side of the history of science, Kuhn doubted the legitimacy of the then mainstream of philosophy; from the other side, i.e. from his training as a physicist as well as a logical empiricist, he questioned the consistency of Aristotle's system from the point of view he acquired from his positivist training. All in all, Kuhn wondered whether Aristotle's system itself had ever had a coherent structure or not according to the

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[17] A few historians of science in Europe had discovered that science has a totally different story of its history. For example, C. Singer already clearly claimed that the 'Galileo Revolution' "was more than an addition to knowledge. It was more even than an alteration in the conception of the structure of the universe. It was rather a change in mood as to the kind of knowledge that was to be sought. If partook of the nature of a philosophical crisis. ... it must be recognized, (the implication of the change) are incommensurable" (cf. *Short History of Scientific Ideas to 1900*, 1959, Oxford University Press, p.249).

rules of logical empiricism.

Surprisingly, it was the second quest that made Kuhn eventually dethrone the image of the logical empiricism itself. That is, while struggling with and overcoming the crisis, with a help of mental enlightenment, Kuhn saw a new way of looking at the matter of Aristotle, the one that led him to his own Copernican Revolution. That was his discovery of the notion of (historical) incommensurability, the conceptual barricade existing between the past and the present. This enlightenment, like the one in which Mendeleeff found the connection between the atomic weights of the elements and their physical properties in his dreams, dramatically resulted in Kuhn's connection of Aristotle's scattered rudiments in his entire system. Kuhn in 1977 recalls the historic discovery as follows:

> One memorable (and very hot) summer day those perplexities suddenly vanished. I all at once perceived the connected rudiments of an alternate way of reading the text with which I had been struggling. For the first time I gave due weight to the fact that Aristotle's subject was change-of-quality in general, including both fall of a stone and the growth of a child to adulthood. In his physics, the subject that was to become mechanics was at best a still-not-quite-isolable special case. (ET, p.xi)

As a result of the 'gestalt switch', as Kuhn later would talk of the sort of thing that occurred, he found that all the "absurd things" in Aristotle's framework suddenly faded away and a new picture of the system emerged with some key ontologically primary and indestructible elements as the principle of Aristotelian system. They are the permanent ingredients of Aristotle's conception of the universe: they are in nature ***qualities***. For Aristotle, Kuhn suddenly realized, not material bodies, but rather the qualities that constitute an individual material body or substance when the qualities are imposed on some portion of omnipresent neutral matter. With this perception, Kuhn recognized that position as one element of the qualities in Aristotle physics is changing, but a body that was changing its position remained the same body. "In a universe where qualities were primary, motion was necessarily a change-of-state rather than a state."(ET, p.xii)

Ten years later, Kuhn depicted the Aristotelian system as follows:

> Part of the authorities of Aristotle's writings derives from the brilliance of his own original ideas, and part derives from their immense range and logical coherence, which are as impressive today as ever. But the primary source of Aristotle's authority lies, I believe, in a third aspect of his thought, one which it is more difficult for the modern mind to recapture. Aristotle was able to express in an abstract and consistent manner many spontaneous perceptions of the universe that had existed for centuries before he given them a logical verbal rationale. In many cases these are just the perceptions that, since the seventeenth century, elementary scientific education has increasingly banished from the adult Western mind. (CR, p.96)

Philosophically, the by-product of Kuhn's "first scientific revolution" is by all means a new way of "recapturing out-of-date ways of reading out-of-date texts"(ET, p.xiii) in contemporary philosophy of science.

Later, in 1977, Kuhn also came to see that his discovery and its development is in accordance with the line of Continental hermeneutics which made him interpret history of science differently. It was the tradition of hermeneutics that, Kuhn realizes later, reveals that our reading out-of-date texts could not only rely upon our creation "by additions to knowledge or by the mere piecemeal correction of mistakes"(ET, p.xiii) without finding our own conceptual scheme of the text. According to Kuhn, it is inappropriate for us to try to justify or rationalize out-of-date texts in a way so as to add or correct them in terms of any contemporary interpretation of science. Rather the opposite, Kuhn argued that the text of science itself had to be justified in its own cultural as well as historical context, or as Kuhn put in CR, in "the combination of science and (its) intellectual history."(CR, p.viii)

Although Kuhn never refers to his discovery in terms of the idea of "incommensurability", he points out that the only way to get rid of "strained metaphors" and "apparent absurdities", which are important characteristics of incommensurable phenomenon that arise when reading an obsolete text, is first to think like men in the past. Experience of this kind, he suggests, is required to undergo "a real mental transposition"(CR, p.95), so as to come to see the plausibility and coherence of the text.

Several years later, Kuhn employed his experience of enlightenment in 1947 as a teaching maxim to students learning history of science:

> When reading the works of an important thinker, look first for the apparent absurdities in the text and ask yourself how a sensible person could have written them. When you find an answer, I continue, when those passages make sense, then you may find that more central passages,

> ones you previously thought you understood, have changed their meaning. (ET, p.xii)

No doubt that here exists his commitment to registering the phenomenon of historical incommensurability as means ushering his students to see the truth: the way of reading text in present need not fit into the way in past.

Almost ten years after his enlightenment, Kuhn embarked on a term ***incompatibility*** to interpret what he had illuminated about the phenomena of conceptual change in his first book, *The Copernican Revolution*.[18] Although, in this less well-known and even less frequently read book, Kuhn's notion of incommensurability was still in the process of evolution, he had already claimed that there existed two kinds of 'incompatible' phenomena, one between different conceptual schemes, one between old conceptual scheme and new observations. What is more, he considered, the latter, "the incompatibility of theory and observation" or "logical incompatibility", is the ultimate source of every revolution in the science.[19] Kuhn defined the former in terms of history:

> The history of science is cluttered with the relics of conceptual schemes that were once fervently believed and that have since been replaced by incompatible theories. (CR, p.39)

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[18] As a matter of fact, the term "incompatibility" is the embryo of Kuhn's notion of incommensurability. Once with this comprehension in mind, his first book will be made out more sense than it appears.

[19] Cf. CR, pp.75-77.

And he held that to see the logical incompatibility was much easier than to discover the historical one:

> ... though scientists undoubtedly do abandon a conceptual scheme when it seems in irreconcilable conflict with observation, the emphasis on logical incompatibility disguises an essential problem. What is it that transforms an apparently temporary discrepancy into an inescapable conflict? How can a conceptual scheme that one generation admiringly describes as subtle, flexible, and complex become for a later generation merely obscure, ambiguous, and cumbersome? Why do scientists hold to theories despite discrepancies, and, having held to them, why do they give them up? These are problems in the anatomy of scientific belief.(CR, p.75)

This commitment to the "anatomy" of scientific beliefs in CR eventually paved the way to his concept of incommensurability in SSR.[20]

Kuhn first used the term, "incommensurability", in 1962 in *The Structure of Scientific Revolutions*.[21] On page 4 of that book, he

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[20] Kuhn's commitment to "the anatomy of scientific belief" in CR reminds us of Crane Brinton's *The Anatomy of Revolution*, that influenced most young historians in the postwar years. The resemblance is especially striking when Brinton says, "Actually the scientist cannot work without a conceptual scheme; and through the relation between facts and conceptual scheme is not by any means clear, it is at least clear that a conceptual scheme involves something besides facts, involves, indeed, a working mind" (*The Anatomy of Revolution*, Prentice-Hall, Inc., 1938, p.8).

[21] Twenty years later, in 1982, Kuhn recalled that 'incommensurability' was the term "Paul Feyerabend and I first used in print a term we had borrowed from mathematics to describe the relationship between successive scientific theories"(*PSA 1982*, Vol. 2, p.669). He believes that Feyerabend's and his resort to 'incommensurability' was independent, and he has an uncertain memory of Feyerabend's finding the term in a draft manuscript of SSR and telling Kuhn he too had been using it. Also cf. Feyerabend's 1962 paper, "Explanation, Reduction, and Empiricism" in *Scientific Explanation, Space, and Time* (*Minnesota Studies in*

distinguishes the concept of incommensurability from that of incompatibility:

> ...the early developmental stages of most sciences have been characterized by continual competition between a number of distinct views of nature, each partially derived from, and all roughly compatible with, the dictates of scientific observation and method. What differentiated these various schools was not one or another failure of method - they were all "scientific" - but what we shall come to call their incommensurable ways of seeing the world and of practising science in it.

With the notion of incompatibility, Kuhn attempts to draw attention to the relation between two different scientific theories when logically comparing them, especially between a new and an old one. First, Kuhn think that when two scientific theories cannot be logically compared, it is because the new one is of "a logically higher type."(SSR, p.98) In other words, they are logically incompatible not because the new theory is in conflict with its predecessors, but because they are not at the same logical level. In light of this, Kuhn proposes historicity into the business of logical comparison accordingly. Second, Kuhn believes that the notion of incompatibility is established not only in the logical sense, but also in that,

> though logical inclusiveness remains a permissible view of the relation between successive scientific theories, it is a historical implausibility.(SSR, p.98)

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the Philosophy of Science, Vol. III), edited by H. Feigl and G. Maxwell, University of Minnesota Press, 1962, pp.56-59, 74-76, 81. Kuhn recalls in a draft he sent to me in October, 1991, that what motivated them most was closely related to those views already discussed by N. R. Hanson in his *Patterns of Discovery*.

Kuhn thus argues, for example, that Einstein's theory can be accepted only with the recognition that Newton's was wrong. For Kuhn, it is useful to have Quine's holist perspective always in mind when comparing theories. That is, it is not piecemeal comparison, but that the successful new theory must somewhere permit predictions which are different from those derived from its predecessor. Therefore, differences in predictions will not occur, Kuhn assumes, if the two are logically compatible. It is in this sense that Kuhn distinguishes his notion of incompatibility from the newly employed concept of incommensurability:

> The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.(SSR, p.103)

Incommensurability as the inner reason of incompatibility, therefore, becomes one of the important philosophical results to arise from Kuhn's 1947 discovery of how to read Aristotle. The notion not only discloses the nature of reading an out-of-date text, but also, fifteen years later, reappears in his argument against empiricist reductionism. That is, from his stance of incommensurability, Kuhn reveals that it is implausible for reductionist to reduce Galileo's system of mechanics to Aristotle's system of physics. For the same reason, Kuhn declares in 1962 that Einstein's theory cannot be simply reduced into Newton's framework. Challenging the reductionist view that something "is just" (or "is really") something else, Kuhn armed with the notion of incommensurability aims at defeating the prevailing reductionist belief in unlimited extensibility of a single theory even beyond any historical and scientific limits.

Because of incommensurability, Kuhn argues, two traditions just appear different without further illustration since they never articulate the same "world view". With his commitment to defeating reductionism, Kuhn declares that the unreducible theory is often richer than its predecessors, such as the case with respect of Einstein's theory and Newton's. So, because of the difference between successive traditions, Kuhn envisages, it is not only unnecessary but also impossible to reduce one to other.

What is the notion of incommensurability? Kuhn in SSR clearly defines the notion in three aspects with reference to his notions of "paradigms" and "revolution".

The first aspect is the incommensurability of standards. Since the standards or the definitions of science between two traditions are not the same, Kuhn argues, proponents of competing paradigms will often disagree about the list of problems that any candidate theory must resolve. One of his examples is that, in the nineteenth century, Lavoisier's chemical theory inhibited chemists from asking why the metals were so much alike, a question the phlogistic chemistry had both asked and answered. And this transition, one from phlogistic chemistry to Lavoisier's chemistry of elements, "meant a loss not only of a permissible question but of an achieved solution."(SSR, p.147) Given that in the twentieth century some questions inhibited by Lavoisier's chemistry entered science again, Kuhn claims that the existence of different standards of scientific problems and answers implies incommensurability.

The second aspect is of the reconstruction of the old conceptual and manipulative vocabulary or apparatus, and of partial communication between two traditions as well. Kuhn deems that since old terms, concepts, and experiments which were employed by the old paradigm fall into new relationships within the new paradigm, it cannot be illustrated or understood in the traditional way any more. The inevitable result will be a misunderstanding between two competing schools if they insist on employing their own terms, concepts, and experiments to interpret the other's. For example, a desire by mathematicians, physicists, or philosophers to develop a Euclidean version of Einstein's theory just could not be fulfilled for what had previously been meant by space was necessarily flat, homogeneous, isotropic, and unaffected by the presence of matter. "To make the transition to Einstein's universe, the whole conceptual web whose strands are space, time, matter, force, and so on, had to be shifted and laid down again on nature whole."(SSR, p.148) So, given incommensurability, "communication across the revolutionary divide is inevitably partial."(SSR, p.148) Those who grasp a notion in different paradigms cannot fully discover what they really agree or disagree about. For example, Kuhn points out that people called Copernicus mad just because he proclaimed that the earth moved. And they were not aware that Copernicus totally changed the meaning of both 'earth' and 'motion', which, of course, made communication breakdown. However, communication can be conducted, Kuhn considers, but it will never be in a commensurable way that the reductionist has envisaged.

The third, the most fundamental aspect of incommensurability is that "the proponents of competing paradigms practice their trades in different worlds."(SSR, p.149) Deeper than the other two, this feature of incommensurability provides the reason "why a law that cannot even be demonstrated to one group of scientists may occasionally seem intuitively obvious to another."(SSR, p.149) Of course this does not mean that scientists in different communities can see anything they please. Objectively, what they look at in nature has not changed, but they see different things which are in different relations even when they look from the same point in the same direction.[22] Their world views simply differ after a scientific revolution in which the previous view of the world has been perceptually as well as conceptually changed to in a certain way, as a man wearing different lenses, because interpretation of data is presupposed not by piecemeal linkage with particular or random experience, but, on the contrary, by a paradigm, understood as a "world view".

To overcome incommensurability and to hope communicating fully, therefore, must involve a conversion, a paradigm shift. For Kuhn believes that a transition between the incommensurable ways cannot be made a step at a time. That is, it must occur all at once, like a gestalt-switch, or not at all. As Kuhn discovered the coherence of Aristotle's system in the summer of 1947, he learned,

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[22] Here, we can see the influence of Hanson on Kuhn's notion of incommensurability.

to some extent, to think like an Aristotelian physicist.

From the summer of 1947 to 1962, however, Kuhn's concept of incommensurability sprang out of his epistemological revolution and entangled with his other synthesis of a paradigm in such a degree that without fully understanding the notion of incommensurability, there can be no plausible interpretation of paradigms. From his incommensurable experience of understanding Aristotle's physics, Kuhn not only discovered the phenomenon of incommensurability, he also built up its twin concept of a paradigm. So, without clarifying Kuhn's synthesis of a paradigm, we shall be unable to fully comprehend Kuhn's Copernican Revolution, the discovery of incommensurability. In the next chapter, I shall investigate the development of Kuhn's other key notion, that of a paradigm, so as to disentangle the nature of his revolution and his later conversion.

3

From Conceptual Schemes To Paradigms

When the perplexities caused by reading Aristotle's *Physics* suddenly vanished on that very hot summer day in 1947, Kuhn experienced "a real mental transposition",[23] a mental enlightenment concerning the existence of incommensurability. It was through this mental transposition that Kuhn not only discerned the logical

[23] Later, in 1957, Kuhn depicted his 1947 adventure in this: "but Aristotle's day is not our day, and a real mental transposition is therefore necessary in approaching his writings, particularly those dealing with physics and cosmology."(CR, p.94)

coherence of Aristotle's system, but also realized that there exists more than one conceptual scheme of in the history of science. In this chapter I plan, with recourse to his discovery of incommensurability, to further sketch the outline of the evolution of Kuhn's notion of a paradigm against the historical background he had experienced.

Even though in the summer of 1947 Kuhn experienced a mental transposition, or enlightenment, his epistemological discovery was not what he later recalled in his autobiographical *Preface* to *The Essential Tension.*[24] As a matter of fact, what Kuhn really discovered in the summer of 1947 was merely a new way of reading an ancient text, although this presaged Kuhn's later approach to incommensurability. In 1947, while assisting Conant in teaching *Natural Science 4*, Kuhn was also busy finishing his physics dissertation on a new technique for measuring the cohesive energy of certain types of metals. Only after he was granted Ph.D degree in physics in 1949 and, especially, with the help of a Junior Fellow of the Society of Fellows of Harvard University between 1948 and 1951, did Kuhn have opportunity to pursue in depth some of the ideas which he had enlightened on the very hot summer day in 1947. It was only during this period as a Junior Fellow that Kuhn made the transition, practically as well as theoretically, from

[24] This illustrates that a person's autobiography is not always reliable in tracing his or her academic deeds. For people quite often, on purpose or not, rewrite personal history in much the same way as Kuhn criticizes the flaws in scientific textbooks. This is also true, sometimes, in the case of studying Kuhn himself.

physicist to historian and philosopher of science.

In practice, Kuhn started in 1949 teaching one of the Science General Education courses at Harvard College, a series of lectures on history of science. Due to the fact that students taken the General Education course did not intend to continue studying of science, Kuhn tried, quite successfully, to combine technical materials of science with intellectual-historical materials of science. Apparently, the combination of technical and intellectual-historical materials was influenced by certain European thinkers such as A. Koyré:

> In particular I continued to study the writings of Alexandra Koyré and first encountered those of Emile Meyerson, Hélène Metzger, and Anneliese Maier. ... this group has shown what it was like to think scientifically in a period when the canons of scientific thought were very different from those current today.(SSR, pp.vii-viii)

This is the period of shaping his conception of what the history of scientific ideas might be. Particularly, Kuhn spent much of his time exploring fields without apparent relation to history of science. Through this exploration, Kuhn also encountered prominent figures of twentieth century such as Jean Piaget, Arthur O. Lovejoy, Gestalt psychologists, B. L. Whorf, and of course, W. V. O. Quine.[25] Another fortunate result from his random exploration was Kuhn's encounter with Ludwik Fleck's almost unknown monograph, *Entstehung and Entwicklung einer wissenschaftlichen Tatsache*, an essay, as Kuhn later acknowledged, "that anticipates many of my own

[25] Kuhn had been personally acquainted with Quine since 1948.

ideas."(SSR, p.ix)

But even after he was invited to deliver a series of eight public lectures on "The Quest for Physical Theory" during March of 1951 at Lowell Institute in Boston, Kuhn remained unconvinced that he was equipped with either enough knowledge in history of science or enough philosophical ideas to proceed to publication. As Kuhn recalled, "for a period that I expected to be short but that lasted seven years, I set my more philosophical interests aside and worked straight forwardly at history."(ET, p.xvi)

The upshot of that straight-forward work on history was Kuhn's first book, *The Copernican Revolution* (1957). In the *Preface* of this book, Kuhn first employed the term "paradigm":

> Since students in this General Education course do not intend to continue the study of science, the technical facts and theories that they learn function principally as paradigms rather than as intrinsically useful bits of information.(CR, p.ix)

However, Kuhn's use of the concept of "paradigms" in November, 1956 has nothing more special than the normal dictionary definition. That is, in CR, "paradigm" has not yet assumed the central place that it was later to assume.

Yet, the perspective of a paradigm in the sense of SSR already penetrated the entire work of CR as the idea of a "conceptual scheme". For example, Kuhn regards the Copernican Revolution as a transformation of "conceptual changes"[26], one which presupposes that at certain times in history "the concepts of many different

[26] Cf. CR, p.vii.

fields are woven into a single fabric of thought."(CR, p.vii) Scientific conceptual scheme is only one strand of that fabric of thought and thereby should be sociologically tackled by "the combination of science and intellectual history."(CR, p.viii) Under this interpretation of scientific history, Kuhn maintained in 1956 that conceptual schemes are time-honoured products of the human imagination, each "a theory, deriving from observations but simultaneously transcending them."(CR, p.35)

Kuhn's 1956 perspective of "conceptual scheme" has several advantages for the enterprise of science. Among them, logical as well as psychological functions are basic characteristics of a conceptual scheme.

The most striking characteristic of conceptual scheme, logical function, Kuhn otherwise calls "conceptual economy",[27] provides scientists with memory efficiency. For given a mass of scattered observations in a long list of bare facts, "it is difficult to retain the whole list in memory simultaneously."(CR. p.37) Only can logic provide a compact pattern for a vast quantity of important observational materials. Once the logical model is systematically presented, Kuhn points out:

> The model replaces the list, because, as we have already seen, the observations can be derived from the model. Frequently, they need not even be derived. A man who observes the heavens with the two-sphere universe firmly fixed in his mind will find that the conceptual scheme discloses a pattern among otherwise unrelated observations, that a list of the observations becomes a coherent whole for the first time, and that the

[27] Cf. CR, p.36.

> individual items on the list are therefore more easily remembered. Without these ordered summaries which its theories provide, science would be unable to accumulate such immense stores of detailed information about nature. (CR, p.37)

In other words, logic economically provides a conceptual scheme with coherence or consistency which, in a certain period of history, can be easily committed to scientists memory once for all.

Secondly, the "conceptual scheme" has another implicit striking characteristic, the psychological function of representing the scientists' attitude towards science, especially their belief in the "truth" of a conceptual scheme. In other words, a conceptual scheme has a priority requirement, Kuhn believes, that psychologically the scheme is thought to be more than a convenient device for summarizing what is already known. A conceptual scheme provides no explanation, Kuhn holds, unless it is believed to be true:

> A scientist's willingness to use a conceptual scheme in explanations is an index of his commitment to the scheme, a token of his belief that his model is the only valid one. Such commitment or belief is always rash, because economy and cosmological satisfaction cannot guarantee truth, whatever truth may mean. (CR, p.39)

Furthermore, Kuhn takes these two striking functions of a conceptual scheme to lie at opposite ends of a spectrum between which there also lie many other functions. Thus the "two-sphere universe" model, a conceptual scheme, Kuhn point out, wholly depends upon the two-end spectrum model: one end is the logical structure of a theory, and the other its psychological ability to evoke belief.

Besides these two main functions, three others in the spectrum need to be introduced. First is the "predicting function" that allows scientists not to be limited to what is already known. That is, scientists with a conceptual scheme "will expect nature to show the additional, but as yet unobserved, properties that the conceptual scheme predicts."(CR, p.39) Kuhn insists that as soon as a conceptual scheme is set up,

> (It) will transcend the known, becoming first and foremost a powerful tool for predicting and exploring the unknown. It will affect the future of science as well as its past.(CR, p.39)

Following Quine's lead of holism,[28] Kuhn assumed that the logical structure of a conceptual scheme is not just a analytical reductionist system. For the scheme also synthetically predicts new knowledge which tells the scientist about the behaviour of the nature.

A second closely related function is that conceptual schemes have a "heuristic function". Since a conceptual scheme is not initially developed to account for the phenomena not covered by observational materials, producing new knowledge means a guidance

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[28] Many readers of SSR get the strong sense that they are reading a work that was composed as response to Duhem. But, this resemblance of holistic approach in Kuhn's work, I think, is largely thanks to the influence of Quine rather than directly from Duhem. Kuhn mentions in quite few occasions that not only he had chances to talk to Quine during his fellowship at Harvard, but also Quine gave some feedback to his draft of first several chapters of SSR in 1958-59 when they both were at the Centre for Advanced Studies in the Behavioral Sciences at Stanford.

of discovering a new way to extend the scheme:

> the guidance provided by conceptual scheme is rarely so direct and unequivocal as that illustrated above (Cf.examples illustrated under the predicting function the author). Typically a conceptual scheme provides hints for the organization of research rather than explicit directives, and the pursuit of these hints usually requires extension or modification of the conceptual scheme that provided them.(CR, p.40)

Thus a conceptual scheme has a framework within which scientists can cultivate the body of knowledge to an unexpected scale.

The third function, that a conceptual scheme has the characteristic of incompatibility was already mentioned in Chapter 2. Employing this incompatible feature of conceptual schemes, Kuhn depicted the history of science in light of conceptual change:

> The history of science is cluttered with the relics of conceptual schemes that were once fervently believed and that have since been replaced by incompatible theories. There is no way of proving that a conceptual scheme is final.(CR, p.39)

Now we have no difficulty recognizing elements of SSR in *The Copernican Revolution*; the notions of paradigm and incommensurability appear in embryo. It is only a matter of time before they will emerge in their mature form.

From 1958 to 1959, not very long after he finished CR, Kuhn was granted a Fellowship at the Centre for Advanced Study in the Behavioral Science at Stanford where he intended to write a draft on scientific revolutions. But while he had no difficulty to produce a chapter on revolutionary change, "attempts to prepare a companion chapter on the normal interlude between revolutions gave (him) great trouble."(ET, p.xviii) It was only after communicating with social and natural scientists at the Centre that he came to realize that practioners of natural science, unlike their colleagues in the area of social science, possess firmer or more permanent answers to such questions about the nature of legitimate scientific problems and methods. That is, Kuhn conceived that the community of natural scientists is different from the one of social scientists in that the former obtains a relatively longer and firmer consensus than the latter does. Furthermore, this recognition of the difference between two communities led Kuhn to conceive, early in 1959, that the consensus of a natural scientific community stands due to a more profound characteristic of scientific enterprise:

> If scientists were not taught definitions, they were taught standard ways to solve selected problems in which terms like "force" or "compound" figured. If they accepted a sufficient set of these standard examples, they could model their own subsequent research on them without needing to agree about which set of characteristic of these examples made them standard, justified their acceptance. That procedure seemed very close to the one by which students of language learn to conjugate verbs and to decline nouns and adjectives. ... The usual English word for the standard examples employed in language teaching is "paradigms".(ET, p.xix)[29]

This description of his usage of "paradigms" is only close to

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[29] Actually, Kuhn wrote two stories about his capture of the notion of paradigms. One is in the *Preface* to SSR in 1962(cf. SSR, p.viii); the other in the *Preface* to ET in 1977(cf. ET, pp.xviii-xx). Although both provide the procedure to the birth of the perspective of "paradigms", they deeply differ in their details.

his current stand in "*The Essential Tension: Tradition and Innovation in Scientific Research*", delivered in 1959 at The Third University of Utah Research Conference on the Identification of Scientific Talent. It is in this paper that Kuhn first uses the notion of a paradigm positively in terms of "examples" in scientific textbooks:

> Last, but most important of all, is the characteristic technique of textbook presentation. Except in their occasional introductions, science textbooks do not describe the sorts of problems that the professional may be asked to solve and the variety of techniques available for their solution. Rather, these books exhibit concrete problem solutions that the profession has come to accept as paradigms. (ET, p.229)

And he continues with strong echoes of Bridgman and Wittgenstein:

> [these textbooks] then ask the student, either with a pencil and paper or in the laboratory, to solve for himself problems very closely related in both method and substance to those through which the textbook or the accompanying lecture has led him. Nothing could be better calculated to produce "mental sets" or *Einstellungen*. (ET, p.229)

In his 1962 book, SSR, with recourse to operationalism as well as Wittgensteinian belief Kuhn further anchored all his furniture of conceptual change on the nature of scientific education, i.e., a textbook-leaning enterprise.[30] He insists that only by the sort

[30] In the sense that both Bridgman and Wittgenstein stress the importance of what one does rather than of what one thinks, I envisage that the relationship between the two philosophers might be established in terms of practical or pragmatic philosophy. Perhaps the only difference is that Bridgman mainly discusses Operationalism in the field of physics whereas Wittgenstein approaches the issue in a much larger area in epistemology.

of rigid scientific education in exclusive paradigms, a dogmatic initiation, natural sciences began to make rapid and systematic progress. In particular, Kuhn holds that the course of learning paradigms makes scientific community eventually reach an agreement, without which in certain scientific fields there would be no patterns to foster the rapid consequential scientific advance. Kuhn thinks that this is the fact that recent centuries have accustomed us: the unique paradigm feature of natural sciences teaching explains not only that science is a consensus or convergent-bound enterprise, but also that the "normal research" with traditional techniques and beliefs, i.e. the consensus-bound scientific endeavour, can be abandoned and replaced by new ones through scientific revolutions. Scientific revolutions, in this, "are but one of two complementary aspects of scientific advance." (ET, p.227)

So it is not very hard to see that as soon as he nails down the logical starting point of his approach, i.e. paradigms as scientific examples, Kuhn's interpretation of conceptual change, scientific revolutions, is set into a coherent logical structure. A settled consensus of a scientific community is acquired from scientific education, one that is characteristic of paradigms learning. The settled consensus then pins down normal research as a highly convergent and collective activity. And the break-up of the "essential tension" implicit in scientific tradition or normal research heralds the advent of scientific revolutions -- all these start with a belief that "textbook paradigm teaching" is the only nature of scientific enterprise. With this understanding of science, Kuhn regards the spectrum of his notion of conceptual scheme, the paradigm, as one that has dynamic structure of scientific history and scientific belief at its two ends. But, how does this spectrum function in its development? Dogma, answered Kuhn in a 1961 paper, "The Function of Dogma in Scientific Research".[31]

In this essay, Kuhn makes a strong claim that one has to regard "dogma" as a virtue of science to recognize that no mature science could exist without it. Normal research is in nature dogmatic because scientific education is dogmatically initiated. That is, students of science learning paradigms are neither invited nor equipped to evaluate the "pre-established problem-solving tradition" *per se*. Furthermore, Kuhn remarks that in fact the whole scientific community sociologically behaves the same way:

> Scientific education inculcates what the scientific community had previously with difficulty gained -- a deep commitment to a particular way of viewing the world and of practising science in it.[32]

In other words, it is "exclusiveness of paradigms" that determines a developmental pattern for the normal research of science. But apparently here Kuhn has moved away from his original

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[31] This paper was delivered to the symposium under the title *The Structure of Scientific Change* held at Oxford on the 9-15, July 1961 when Kuhn had almost finished his forthcoming monograph, *The Structure of Scientific Revolutions*. Later in 1963, the paper appeared in *Scientific Change*, (ed.) by A. C. Crombie, New York: Basic Books, 1963, pp.347-369.

[32] A. C. Crombie (ed.), *Scientific Change*, New York: Basic Books, 1963, p.349.

sense of paradigms in the 1959 paper. For Kuhn has largely inflated the status of a paradigm from a "teaching example" to a "universally received scientific achievements". That is, he has come to regard Aristotle's *Physics*, Franklin's *Electricity*, Lavoisier's *Chemistry*, and Lyell's *Geology*, etc. as paradigms "for a time implicitly to define the legitimate problems and methods of a research field for succeeding generations of practioners."[33] And the exclusiveness of a paradigm is regarded as dogma of a scientific community to inform its members, not just students of science any more, the questions that may legitimately be asked about nature and of the techniques in their search for answers to those questions.

Much of the effort of a scientific community, therefore, is further directed to articulating a paradigm under the view of its exclusiveness. Apparently, the dogma or paradigm is expanding towards the degree of "conceptual scheme" in CR. That is, although students of science still learn the trade under the guidance of the dogma, Kuhn considers that this is only one principal advantage of the scientific dogmatism. For scientists also need something in normal case to tell them where to look and what to look for. This dogmatic "something", a paradigm, appears to be something that, on the one hand, students of science has to learn, and that, on the other, the whole scientific community has to obey. To this extent, the construct of a paradigm in the 1961 paper is very close to the definition of conceptual scheme in CR and that of paradigms in SSR.

[33] Ibid., p.352.

However, the first exposure of his tentative notion of a paradigm to the international symposium was by no means successful, Michael Polanyi's wholeheartedly positive commentary aside.[34] Towards Kuhn's thrust of the notion of dogma, which sets science in a rigid framework, a debate occurred among his critics at the Symposium of which S. E. Toulmin stands only in favour of logical dogmas and Polanyi radically in favour of the belief as essential element of Kuhn's paradigm:

> There are of course no such rules. We have some useful maxims to guide us, but the choice of the maxim to be applied and the discretion left open in applying it still leaves responsibility for his own conclusion to the individual work engaged in research.[35]

Having realized the problem of introducing the term "dogma", in the discussion section of the Symposium Kuhn expressed himself willing to surrender the word of "dogma" in favour of commitment to a notion called ***paradigm***. Also, Kuhn welcomed Polanyi's radical and helpful suggestions:

> More important, I entirely agree both that there are no rules for distinguishing an essential anomaly from mere failure and also that some anomalies are recognized for many years without "inducing any scientist to reconsider the current theories with which they conflict". I was concerned not to find a methodological rule for individual scientists (e.g. Mr. Popper's principles of falsification) but rather to characterize the state of the scientific community within which a new theory is

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[34] A. C. Crombie (ed.), *Scientific Change*, New York: Basic Books, 1963, pp.375-380.

[35] Ibid., p.380.

invented and accepted.[36]

There can be no doubt that after the Symposium, Kuhn substantially changed his notion of a paradigm in his forthcoming monograph, SSR.[37] For, when it appeared in 1962, Kuhn significantly addresses his approach on the opposition to the view that science is a rule-governed dogmatic enterprise which is a theme that he hardly touched before. He now proclaimed in SSR that "normal science is a highly determined activity, but it need not be entirely determined by rules."(SSR, p.42) For the first time, apparently with the help of Polanyi, Kuhn nails down his approach to the line of an alternative view of history and philosophy of science to defy the mainstream Popperian image of science.

In Chapter V of SSR, "The Priority of Paradigms", Kuhn makes four points about rules *vis-à-vis* paradigms in order to further strengthen his perspective of a paradigm. In contrast with the function of rules, Kuhn claims:

(1) paradigms, not rules, govern the normal science;

(2) the ability to do successful research can be understood without recourse to rules because it is rooted in the nature

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[36] Ibid., p.392.

[37] However, Kuhn does not concede that there is difference between the paper and SSR. "The ideas developed in this paper have been abstracted", he says, "in a drastically condensed form, from the first third of my forthcoming monograph, *The Structure of Scientific Revolutions*, which will be published during 1962 by the University of Chicago Press. Some of them were also partially developed in an earlier essay, 'The essential tension: tradition and innovation in scientific research'..." (see note 1 in "The Function of Dogma in Scientific Research", *Scientific Change*, A. C. Crombie (ed.), New York: Basic Books, 1963, p.347).

of scientific education which only involve acquiring paradigms tacitly obtained by applications of a theory;

(3) rules become important only whenever paradigms or models are felt to be insecure;

(4) substituting paradigms for rules should make the diversity of scientific fields and specialties easier to understand;

Of these four points, the second one is central since Kuhn regards the first, third, and fourth objections as only corollaries of it.

Kuhn's first point is that the intellectualist picture of science as a rule-governed enterprise is ultimately unfaithful in that there is no such thing in the area of normal science. On the one hand, he holds, "the existence of a paradigm need not even imply that any full set of rules exists."(SSR, p.44)[38] On the other hand, although rules, considered as 'isolatable elements', may be abstracted from paradigms and deployed in scientific research, he remarks, "the search for a body of rules competent to constitute a given normal research tradition becomes a source of continual and deep frustration."(SSR, p.44) The reason for frustration is in that scientists can agree in their *identification* of a paradigm without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* of it.[39] In other words, "normal science can be

[38] He refers this kind of scientific inquiry to something like Polanyi's "tacit knowledge" that is only acquired through practice and that cannot be articulated explicitly. This trait of science will be discussed later in connection with the fourth objection.

[39] In this understanding Kuhn indicates: "scientists can agree that a Newton, Lavoisier, Maxwell, or Einstein has produced apparently permanent solution to a group of outstanding problems and still disagree, sometimes without being aware of it, about the

determined in part by the direct inspection of paradigms."(SSR, p.44) By the phrase 'in part', Kuhn merely implies that the direct inspection of paradigms "is often aided by but does not depend upon the formulation of rules and assumptions."(SSR, p.44)

For Kuhn, however, the problem of the 'direct inspection of paradigms' can be easily cleared up with recourse to Wittgenstein's concept of 'family resemblance'. Because, Kuhn envisages that scientific activities are constituted by a network of overlapping and crisscrossing resemblances, which can be recognized by scientific community as its established achievement, paradigms.

In any event, Kuhn's aim is to show clearly that "paradigms could determine normal science without the intervention of discoverable rules"(SSR, p.46), that the scientific endeavour "may not imply even the existence of an underlying body of rules."(SSR, p.46) Therefore, his first objection, no matter how vague it may be, is to erase the supposed positivist image of rules in normal science. In terms of this, Kuhn not only denies the over exaggerated role of rules in normal science, but also insists that it can simply survive without any rules. For paradigms can so diversifying and flexibly function, Kuhn depicts, that they dictate science under a new perspective of a paradigm-governed enterprise.

As for his third point, here Kuhn is suggesting that rules can become somewhat so important in normal science that they can replace the function of paradigms and guide research by direct

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particular abstract characteristics that make those solutions permanent."(SSR, p.44) modelling as well as through abstracted rules. Under another condition, that is, when scientists disagree about whether the fundamental problems of their field have been solved, Kuhn also holds, the search for rules gains a function that it does not normally possess. Nevertheless, it seems that under certain conditions rules function to rescue the paradigm-governed enterprise. That is, Kuhn grants rules the following legitimate stance:

> Normal science can proceed without rules only as long as the relevant scientific community accepts without question the particular problem-solution already achieved. Rules should therefore become important and the characteristic unconcern about them should vanish whenever paradigms or models are felt to be insecure. (SSR, p.47)[40]

His fourth point, the possibility that there can be small revolutions as well as large ones, Kuhn deploys paradigms over all kinds of fields of normal science in order to grant paradigms a

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[40] Anyway, I do not think that Kuhn's idea of rules in his third point is clear enough to carry the real implication of what he tries to say. In any case, it gives readers an impression that, as Kuhn admits, SSR is the result of a rush job. As a consequence, the whole issue of Kuhn's third point lies in whether his picture of scientific history is still in the stages he depicts, i.e. from pre-science to normal science; then from overcoming crisis through scientific revolution to a new normal science. Namely, it must be untangled from whether there is but one process in the stage of normal science in which paradigms are prior to anything in a scientific community. Or there are two processes, one for paradigms and one for their representation by rules, within the normal science. Or simply there is another one, rules stage, which is somewhere between normal science and crisis and for a variety of reasons Kuhn did not bring it out. Of course this issue is already beyond the topic of the current thesis.

status prior to that of "shared rules".[41] In this, his point is neither that paradigms can function without the help of rules (as in his first objection) nor that rules can act without the priority of paradigms while they are felt to be insecure (as in his third objection). Instead, Kuhn assigns paradigms a more concrete and local role: "substituting paradigms for rules should make the diversity of scientific field and specialities easier to understand."(SSR, p.49)

Why can the 'substituting paradigms for rules' be a means to easily understand the diversity of scientific field and specialities? Kuhn provides three reasons:

(1) "there can be small revolutions as well as large ones, (that) some revolutions affect only the members of a professional subspecialty, and (that) for such groups even the discovery of a new and unexpected phenomenon may be revolutionary"(SSR, p.49);

(2) "explicit rules, when they exist, are usually common to a very broad scientific group, but paradigms need not be"(SSR, p.49);

(3) there are two kinds of paradigms. For example, "though quantum mechanics (or Newtonian dynamics,or electromagnetic theory) is a paradigm for many scientific groups, it is not

[41] It is bizarre that in his fourth point Kuhn regards rules as something shared with a very broad scientific group while taking paradigms as, totally opposite to his first point, ones only for the members of a particular professional subspecialty (cf. SSR, pp.49-50).

the same paradigm for them all."(SSR, p.50)[42]

Whereas his second point, the most essential reason for granting paradigms a status prior to rules, Kuhn reveals is rooted in the nature of *scientific education*.[43] Kuhn claims that

> scientists ... never learn concepts, laws, and theories in the abstract and by themselves. Instead, these intellectual tools are from the start encountered in pedagogically prior unit that displays them with and through their applications.(SSR, p.46)

So the process of application is simultaneously the process of learning a theory which includes practising problem-solving both with a pencil and paper and with instruments in the laboratory.[44] In terms of this, Kuhn's concept of paradigm was still in consistent with the one he defined in his 1959 paper, "The Essential Tension". But as a result of other points, the notion of paradigm has shifted into entirely different level, one that is

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[42] This becomes a very interesting as well as confusing issue between small and large revolutions, and between small and large paradigms, or different levels of paradigms, which Kuhn raises here and never comes back to. I think that Kuhn actually blurs the distinction between rules and paradigms to such an extend that no body can get hold of what he means by the terms 'rules' and 'paradigm'. This attests to my judgement that, because of either the need for drastic condensation from the publisher or the 1961 Oxford Symposium, SSR seemed finished in a rush, especially the relationship between rules and paradigms.

[43] This objection to "rules" actually echoes a view of the later Wittgenstein who emphasizes the importance of learning from educational examples.

[44] In terms of this process of learning by finger exercise, I tell Kuhn's kinship with Bridgman's operationalism, although Kuhn himself seldom mentions Bridgman's influence on the formation of his thought when he was in the Department of Physics at Harvard. However, please see Appendix 3.

more close to metaphysical establishment of sciences or something else.

In this, a Kuhnian paradigm construed as 'historically and pedagogically prior unit' is supposed not only to display intellectual tools with and through their applications, but also to further direct research even without the help of applications. Under this interpretation, the prior unit in science, a paradigm, functions by 'direct inspection', according to Kuhn's first point. However, also under this understanding, in the sense of that the prior unit ought to mold laymen HOW to practice in science, unfortunately, it somewhat functions as rules.

With strong echo of Bridgman, Wittgenstein, and Polanyi, Kuhn thus set his notion of a paradigm not only to be useful for explaining history of science, but also to challenge the mainstream philosophy of science. But, as we have discussed, the "prior unit", his notion of paradigm, has its own problems when Kuhn emphasizes that the ability to do successful research is rooted in the nature of ***scientific education***, i.e., learning "intellectual tools" with and through their applications. For this account attributes a paradigm to two contradictory functions, concrete examples as well as "the prior unit", which are in nature incommensurable. And this incommensurable crisis in Kuhn's commitment to explaining the notion of paradigm in turn affects his endeavour to make the notion of incommensurability understood. (We shall detail this unfortunate chain reaction in Part Two.)

It is, therefore, no surprise that immediately after the appearance of SSR, negative criticism to the book was mainly from the arena of philosophy and of philosophy of science. In particular, they all questioned the inner consistency of the notion of paradigm.

4

Paradigms, Exemplars, or Disciplinary Matrices

--- The Transformation Years after SSR (1)

America seems to have always been seeking the new during the years after the Second World War, especially in the 1960s. Under the Kennedy's theme of "Let's get America moving again", the decade of the 1960s became an era of unusual unrest, tension, and change.[45] And so it was in the arena of philosophy and history of science in 1960s. In this chapter, I shall highlight the

[45] Cf. Charles M. Dollar (ed.), *American Changing Times*, John Wiley & Sons, Inc., 1982, pp.900-917.

consequences of Kuhn's revolt and subtle changes in his views after SSR, with special emphasis on his London paper, "Logic of Discovery or Psychology of Research?".[46]

The history of Western ideas also changed significantly in the 1960s as Stephen Toulmin observed:

> It is already clear that the 1960s were one of those remarkable decades which historians of ideas will in due course have to scrutinise with microscopic care. In a dozen different fields, it was a watershed. Intellectual, artistic and social movements which dominated their respective fields throughout the first half of the 20th century finally petered out or ran into the sand, to be replaced by other streams running in quite different, and often quite opposite directions.[47]

That is, for some 50 years before the 1960s, the western mind of sciences had been dominated by static, structural, ahistorical, abstract -- in many cases mathematical -- patterns.[48] In particular, with the influence of German mathematician, Gottlob Frege, B. Russell launched a widespread philosophical revolt around the very first years of the century. It was a movement in favour of more abstract Platonic essence, "logic", which was considered to be

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[46] T. Kuhn, "Logic of Discovery or Psychology of Research?", in *Criticism and the Growth of Knowledge*, (ed.) by I. Lakatos and A. Musgrave, Cambridge University Press, 1970, pp.1-23.

[47] S. Toulmin, "Aspects of Science -- Rediscovering History", in *Encounter*, 36, January 1971, p.53.

[48] Although in the 1930s E. Husserl embarked on the crisis of European sciences in his transcendental phenomenology and G. Bachelard, A. Koyré, etc. posed questions to the ahistorical 'scientific rationality', the main trend of western philosophy of science was in the direction, as Toulmin concludes, till the 1960s.

intrinsically significant for, at least, scientific thinking. To be more precise, in terms of Frege, this movement professes that any attempt to treat the *formal* concerns of logic and mathematics in considerations from history and cognitive psychology was unacceptable in that it not only blurs the representation of logic, making them "laws of thought", but also "makes everything subjective".[49] It is an illusion to suppose, Frege claims, that either historical research into the origins of logic or psychological experiments on child developments could teach us anything about the intellectual foundations of the principle of non-contradiction. On the contrary, Frege holds, philosopher's real business is the duty to strip away the historical and psychological accretions and to reveal concepts in their pure logic forms.

It is with this Platonist ambition that Russell launched this *Logical Reformation of Philosophy,* beginning with the purification of logic and conceptual analysis. The essential effort of the "Reformation" is to call for the intellectual and even artistic fields, i.e. from logic and mathematics to the human sciences and the fine arts, to redefine their conceptions in static, structural, ahistorical, non-representational, and wherever possible mathematical terms. Thanks to the contributions of philosophers of the Vienna Circle, Frege's philosophical ambition of Logical Reformation, the same project as Russell's, spread across intellectual and artistic life in Europe before the World War II.

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[49] G. Frege, *The Foundations of Arithmetic*, (trans.) by J. L. Austin, New York: Harper Torchbooks, 1960, p.xviiif.

Also thanks to the World War II, however, the Vienna Circle's "Philosophical Reformation" was in a large scale transplanted to the "New World" and played a dominant part not only in the philosophy of science but also in the methodology of the social sciences during and after the war. All the way across the major departments of philosophy in U. S. A., concern about logical models and analysis replaced the traditional role of American pragmatism and thereafter dominated professional philosophy of science until well into the 1950s. That is, a theoretical preoccupation with "systems" and "structures" kept intellectual focus on *synchronic* issues to such an extent that philosophers hesitated to tackle the *diachronic* problems of historical, social, and cultural changes.

Yet, concerns of ***the rationality of conceptual change*** were increasingly reaching to the centre of comparable depth in the mind of the younger generation of Anglo-American philosophers of science. That is, these intellectuals began to have increasing doubts about the positivist slogan, "There is no logic of discovery". How, they asked, are new scientific ideas, the result of *conceptual change*, justified after a scientific conceptual change which is already beyond the current conceptual yoke of rationality?

In the 1950s, the Logical Reformation of empiricist positivism had already been challenged by questions about *conceptual change*, questions which finally edged the positivist analysis of *logical structure* aside and further undermined the adequacy of the positivist image. From within professional philosophy, Quine mounted a decisive attack on the empiricist positivism in "Two Dogmas of Empiricism".[50] However, Quine's account of *conceptual change* did not come to be recognized as being of relevance to the philosophical understanding of scientific procedures. For Quine,

> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric which impinges on experience only along the edges. ...the total field is so underdetermined by its boundary conditions, experience.[51]

In other words, what Quine espouses is a more thorough form of pragmatism in which "each man is given a scientific heritage plus a continuing barrage of sensory stimulation; and the considerations which guide him in wrapping his scientific heritage to fit his continuing sensory prompting are, where rational, pragmatic."[52] But, what is "a scientific heritage"? Whatever it may be, one thing is clear that Quine did not refer to the heritage to general history and psychology of scientific discovery.

Almost at the same time, however, a young British philosopher of science, Stephen Toulmin, who was extensively influenced by L. Wittgenstein, W. H. Watson, H. T. Paton and G. Ryle, published a

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[50] W. V. Quine, "Two Dogmas of Empiricism", in *From a Logical Point of View*, Harvard University Press, 1953, pp.20-46. This article was first read, with omissions, to the Eastern Division of APA in December, 1950 at Toronto, and appeared in the *Philosophical Review* in January, 1951.

[51] Ibid., p.42.

[52] Ibid., p.46.

book, *The Philosophy of Science*.[53] In his essay, Toulmin attacked the English and American philosophy of science especially on its views of logical positivism. It is not that things that are said in logical positivism are untrue or fallacious, Toulmin proclaims, but rather that they are irrelevant: the questions which are so impeccably discussed by the positivist have no bearing on physics. For example, he points out, the actual methods of argument physical scientists employ are only rarely examined and one must not, as French philosophers of science, Poincaré for instance, have recognized, take too much standard views about the methods for granted. The trouble with English and American philosophers of science, Toulmin argues, is that they tend "to set off on their work assuming that we are all familiar with the things that scientists say and do, and can therefore get on to the really interesting philosophical points that follow."[54] More than what Quine's pragmatism had required, Toulmin suggests, along the line of Wittgenstein, philosophers must pay close attention to the actual practice of scientists.

However, it was Norwood Russell Hanson, a pioneer of the logic of discovery in 1960s, who brought the psychology and history of scientific discovery back to the frontier of English and American philosophy of science. He in particular focuses on issues of, as Wittgenstein urges, how scientists practically discover laws,

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[53] S. Toulmin, *The Philosophy of Science*, Hutchinson & Co. Ltd., London, 1953.

[54] Ibid., p.10.

theories, and hypotheses. The positivists confine their attention only to the finished research product, Hanson points out, and their "analysis leaves undiscussed the reasoning which often points to the first tentative proposals of laws."[55] In particular with recourse to the *Gestalt* psychology, Hanson claims that

> physical science is not just a systematic exposure of the senses to the world; it is also a way of thinking about the world, a way of forming conceptions. The paradigm observer is not the man who sees and reports what all normal observers see and report, but the man who sees in familiar objects what no one else has seen before.[56]

Furthermore for Hanson the scientist is equipped with a logic of discovery involving ***retroductive reasoning***: theories "are built up 'in reserves'" and "they constitute a 'conceptual Gestalt'."[57] Nevertheless, Hanson draws more attention of his conception of philosophy of science to that "profitable philosophical discussion of any science depends on a thorough familiarity with its history and its present state."[58] Whereas the unfamiliarity between scientific discoveries, or one between 'its history and its present state', i.e. conceptual changes, Hanson seems not yet to touch, although he had never hesitated to refer to events in the history of physics.

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[55] N. R. Hanson, *Patterns of Discovery*, The Cambridge University Press, 1958, p.71.

[56] Ibid., p.30. Also, we can notice that Hanson had employed the term 'paradigm' in his work four years before Kuhn used it.

[57] Ibid., p.90.

[58] Ibid., p.3.

The issue of "unfamiliarity" in history of science, in the guise of incommensurability, was first raised by Kuhn in CR in 1957 and first drawn world-wide attention through both Kuhn and Feyerabend in the 1960s in their effort to throw more light on the debate of *conceptual change.* Of the two, I consider Kuhn's account of the unfamiliarity in scientific history more penetrating in that it synthesizes almost all the insights of *conceptual change* by earlier philosophers into an account of the incommensurable structures of scientific revolutions.[59] And since then American philosophy of science has undergone a historic change represented by Kuhn who introduced both *diachronic* and psychological issues into the field.

It is in this sense that the psychological as well as diachronic issues dealt within Kuhn's books, CR and SSR, are not only legitimate but inescapable in the history of contemporary philosophy of science. For, against Frege's *formalist reformation of philosophy*, Kuhn's challenge reveals that the problem of *conceptual change* became inescapable in the process of formalizing human discourse. Because, the moment when philosophers recognize the rationality of any natural science in the past, as Kuhn depicts in CR, in fact involves something more than logical requirement of coherence of propositions to figure out the system of science. That

[59] As I have noted in the last two chapters, the nature of Kuhn's work is a synthesis. As he himself concedes in 1953: "although much of the historical documentation for the monograph has been drawn from my own studies of the literature and has not previously been published, the project is primarily a work of synthesis." (Cf. Kuhn's 'Plans for Research', his application for the Guggenheim Memorial Fellowship, 1953, p.3.)

is, the moment they grasp the scientific rationality of the system needs to be accounted for in the context of its history as well as in the dimension of psychology.

It is the philosophical problem of *conceptual change* in the history of Western ideas that therefore makes Kuhn's work so penetrating and pertinent to the development of contemporary philosophy of science. For only by understanding the historical problems of Western ideas in the first half of the 20th century, can one see why in the century no work of philosophy of science was so widely read and comprehensively discussed as SSR. It simply aroused a great interest to see possible changes in philosophy, history, sociology and related areas on the *diachronic* and psychological issues which have been, unfortunately, treated in a mathematical, static and sychronical way.

Thus, it is no wonder that as soon as SSR appeared, Kuhn's approach to conceptual change was warmly welcomed in various academic worlds of ideas other than philosophy of science. Historically, as Marie B. Hall commented in April, 1963:

> As a provocative discussion of the progress of science, this book (SSR) can be warmly recommended. Many readers will respond whole-heartedly to a work that deals with problems raised by the history of science without presupposing much knowledge either of that history or of science itself. Historians who teach courses that embody brief discussion of the history of science at various periods should find this book useful and stimulating.[60]

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[60] Marie Baos Hall: "Review of the Structure of Scientific Revolutions", *American Historical Review*, Vol.68, No.3, April, 1963, p.701.

Sociologically, as Bernard Barber predicted in 1963:

> His (Kuhn's) essay has obvious and important relevance to the social sciences. Any sociologist, even without much knowledge of the physical sciences, can read this book with his "third ear" tuned for its meanings for the development of sociology.[61]

Indeed as the October 25th issue of *The Times Literary Supplement* in 1963 recommended:

> There is revolution in the ideas behind this book... The paradigm analysis is well supported by some beautifully simple psychological analogies, but those who argue that science advances either from special to more sophisticated interpretation of what is observed, are always hard on Dr. Kuhn's heels. ... it has the mark of a good book that is honest and penetrating as well as provocative, one often stops reading to think. It holds a new mirror up to scientists and, by reflection from a new angle, something of value to those who watch them.[62]

Whereas in the arena of philosophy, as the *Times Literary Supplement* had implied, English speaking philosophers of science on the both sides of the Atlantic Ocean seriously and carefully watched the progress of this "heretic event".

Particularly in Britain, one important figure of philosophy of science in London, the world well-known philosopher of science, Sir Karl Popper, had strong objection to the *Times Literacy Supplement*'s commentary to SSR. In a seminar that he was conducting specifically on Kuhn's book in the same year at London School of

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[61] Bernard Barber: "Review of the Structure of Scientific Revolutions", *American Historical Review*, Vol.68, No.3, April, 1963, p.701.

[62] *The Times Literary Supplement*, October (Friday) 25, 1963, p.850.

Economics, he urged the opposite conclusion: that while Newtonism did turn into something like a paradigm in Kuhn's sense, no such paradigm emerged during the long history of the theory of *matter*.[63] With the help of the seminar, the Popperian school prepared to combat Kuhn's approach, which it did in 1965. (We will look at this matter later on.)

Philosophical reviews of the book came later and were generally critical. From 1964 on, SSR was discussed in a very critical way in the field of philosophy. For example, H. V. Stopes-Roe first criticized the book in a so-called "sober view":

> In the face of such force and charm, it seems mean to question the lasting value of the work; but it must be said that many of its features are already well established (...); and the author's enthusiasm leads him to over-state his novelties in a way that prejudices the appreciation of those things of value he has to say. ... it gives a misleading picture of the modern history of the historiography of science. It will be good exercise for them (beginning students) to discuss the author's excesses.[64]

In the home country of the essay, the United States, philosophers of science seemed more sympathetic at the very beginning than the "sober views" from the United Kingdom. In 1964, Duddley Shapere wrote up a discussion article (not a book review) with the same title of SSR in which he said:

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[63] Cf. John Watkins, "Against 'Normal Science'", in *Criticism and the Growth of Knowledge*, (ed.) by I. Lakatos and A. Musgrave, Cambridge University Press, 1970, pp.25-34.

[64] H. V. Stopes-Roe: "Review of The Structure of Scientific Revolutions", *The British Journal for the Philosophy of Science*, Vol.XV, No.58, August 1964, pp.158-161.

> This important book is a sustained attack on the prevailing image of scientific change as a linear process of ever-increasing knowledge, and an attempt to make us see that process of change in a different and, ...more enlightening way. In attacking the "concept of development-by-accumulation," Kuhn presents numerous penetrating criticism not only of histories of science written from that point of view, but also of certain philosophical doctrines (mainly Baconian and positivistic philosophies of science, particularly verification, falsification, and probabilistic views of the acceptance or rejection of scientific theories) which he convincingly argues are associated with that view of history.[65]

But Shapere also points out that the whole book is beset by conceptual confusions, confusions that are mainly caused by Kuhn's blanket term, "paradigm":

> For his view is made to appear convincing only by inflating the definition of "paradigm" until that term becomes so vague and ambiguous that it cannot easily be withheld, so general that it cannot easily be applied, so mysterious that it cannot help explain, and so misleading that it is a positive hindrance to the understanding of some central aspects of science; and then finally, these excesses must be counterbalanced by qualifications that simply contradict them.[66]

Given the fact that Kuhn believes that the world is seen and interpreted "through" a paradigm, that paradigms are generally incommensurable, and that there is "meaning variance" between paradigms, Shapere holds that Kuhn's view is no more implied than relativism. In particular, he points out that Kuhn's view of scientific progress is not toward final truth. To get out of the

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[65] D. Shapere: "The Structure of Scientific Revolutions", *The Philosophical Review* (73), July 1964, p.383.

[66] Ibid., p.393.

chaos caused by Kuhn's conceptual confusion and relativistic approach, Shapere suggests that the book "will require not so much further historical evidence as -- at the very least -- more careful scrutiny of his (Kuhn's) tools of analysis."[67] Implicitly as well as explicitly, Shapere's suggestion alludes to a criticism that Kuhn lacks profound analytic training or technique of philosophical analysis which has been *the* basic requirement at philosophy departments in Anglo-American universities.

Kuhn seems to have accepted most of Shapere's critique and suggestion seriously and attempts to control the inflation of the notion of paradigm. In his essay "Logic of Discovery or Psychology of Research?"[68] Kuhn openly confesses that in SSR he uses the term "paradigm" rather than "theory" to denote what is rejected and replaced during scientific revolutions. Retreating to his original view of paradigm in the 1959 paper, "The Essential Tension: Tradition and Innovation in Scientific Research",[69] Kuhn explains that he introduced the concept of paradigm

> to underscore the dependence of scientific research upon concrete examples that bridge what would otherwise be gaps in the specification of the content and application

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[67] Ibid., p.394.

[68] This essay was initially prepared at the invitation of P. A. Schilpp for one of the volumes of living philosophers, *The Philosophy of Karl R. Popper* (1971), but first presented at the International Colloquium in the Philosophy of Science held at Bedford College, Regent's Park, London, from 11 to 17, July 1965. It was later published in CGK.

[69] Cf. ET, pp.225-239.

of scientific theories.[70]

For further clarification, Kuhn provides an example of knowing what swans are, with recourse to Wittgenstein's idea of "family resemblance". In connection with the issue of the necessity of logic, emphasized by his critics, in the course of scientific knowledge, Kuhn raises three questions:

(a) How much can one know about swans without introducing explicit generalizations like 'All swans are white'?

(b) Under what circumstances and with what consequences are such generalizations worth adding to what was known without them?

(c) Under what circumstances are generalizations rejected once they have been made?[71]

With these questions, Kuhn further points out that "one can have sound knowledge in forms to which logic can scarcely be applied".[72] For knowing what a swan is is having a grasp of a natural family resemblance. That is, exposure to a paradigm is, Kuhn interprets, like the procedure of knowing a swan, which provides the basis for rational action. He therefore stresses that scientific knowledge is of this sort of psychologically knowing a natural family through concrete examples or paradigms. In other words, both scientific

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[70] T. Kuhn, "Logic of Discovery or Psychology of Research?", in I. Lakatos and A. Musgrave (ed.): *Criticism and the Growth of Knowledge*, Cambridge University Press, 1970, p.16.

[71] Ibid., p.16.

[72] Ibid., p.16.

textbooks and teachers of science,

> present concrete examples together with a multitude of theoretical generalizations. Both are essential carriers of knowledge, and it is therefore Pickwickian to seek a methodological criterion that supposes the scientist can specify in advance whether each imaginable instance fits or would falsify his theory.[73]

Kuhn's revision of his stand in SSR was not quite recognized by any of the contributors to the symposium held on 13 July, 1965 on "Criticism and the Growth of Knowledge", except for Toulmin:

> In his (Kuhn's) new paper, he appears to be withdrawing somewhat from that original, exposed position, to a less extreme one; yet the effect of doing so (I shall argue) is to demolish entirely his original distinction between 'normal' and 'revolutionary' phases. That is evidently not his intention, but the consequence is (in my view) inescapable.[74]

By singling out the fact that Kuhn was withdrawing, Toulmin consequently questions the legitimacy of Kuhn's entire structure of SSR. And this was the mood of the symposium which Kuhn was, I assume, not expecting.

As Feyerabend who was supposed to be one of the key speakers could not come, one of Popper's disciples who attended his 1963 seminar on SSR, John Watkins, was invited to reply Kuhn's key speech with a paper entitled "Against 'Normal Science'". On the one

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[73] Ibid., p.19.

[74] S. Toulmin, "Does the Distinction between Normal and Revolutionary Science Hold Water?", in I. Lakatos and A. Musgrave (ed.), *Criticism and the Growth of Knowledge*, Cambridge University Press, 1970, p.41. But it should be noted that his paper is a slightly amended version of his original contribution to the symposium.

hand, in connection with Kuhn's views about normal science Watkins charges that "Kuhn sees the scientific community on the analogy of religious community and sees science as the scientist's religion".[75] On the other hand, in the course of recapitulating and criticizing five Kuhnian theses concerning "paradigm change"[76], Watkins teaches Kuhn that the only way out is that he abandons those five theses, i.e., the 'Paradigm-Monopoly', the 'No-Interregnum', the 'Incommensurability', the 'Gestalt-Switch' and the 'Instant-Paradigm', characteristic of SSR. Watkins concludes that Kuhn's approach can only be purified to the degree that can be further brought into "happy accord with methodological theses of Popper's."[77]

Unlike the boldness of Watkins' commentary, his master, the chair of the symposium's discussion, Karl Popper, responds to Kuhn's address carefully and patiently. In his comment, entitled 'Normal Science and its Dangers', Popper sees some real strength in Kuhn's approach. For Popper admits that Kuhn understands him very well -- indeed better than most of his critics -- and that Kuhn's two main criticisms of "Normal Science" are of importance. One is that, Popper concedes, he has "completely overlooked what Kuhn

[75] J. Watkins, "Against 'Normal Science'", in I. Lakatos and A. Musgrave (ed.) *Criticism and the Growth of Knowledge*, Cambridge University Press, 1970, p.33.

[76] Cf. ibid., pp.34-35.

[77] Ibid., p.36. However, I have to say that I have never read any philosophical paper as bad as Dr. Watkins'. His style of comment is not at any rate of an academic discussion at all, and what he says, as M. Masterman notes, is "a really very gross distortion of Kuhn's real view"(see CGK, footnote 1 on p.61).

calls 'normal science':

> I think that distinction between these two kinds of enterprise is perhaps not quite as sharp as Kuhn makes it; yet I am very ready to admit that I have at best been only dimly aware of this distinction; and further, that the distinction points out something that is of great importance.[78]

The other is, Popper regrets, that the existence of 'normal science' as a course relies on the dogmatic teaching of paradigms. From this second consideration, Popper points out the dangers of 'normal science', given that Kuhn suggests that normal science is 'normal' and even communication breakdown is 'normal' between scientists under different paradigms. All these abnormal features of science, Popper believes, are caused by that "Kuhn seems to propose the thesis that the logic of science has little interest and no explanatory power for the historian of science."(CGK, p.55) For this reason, Popper charges Kuhn's approach with historical relativism, the Myth of the Framework, irrationalism, and sociologistic and psychologistic tendencies. For him, "science is essentially critical; ... it consists of bold conjectures, controlled by criticism, and ... it may, therefore, be described as revolutionary."(CGK, p.55) He believes that

> while the Logic of Discovery has little to learn from the Psychology of Research, the latter has much to learn from the former.

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[78] K. Popper, "Normal Science and its Dangers", in I. Lakatos and A. Musgrave (ed.), *Criticism and the Growth of Knowledge*, Cambridge University Press, 1970, p.52.

But, interestingly enough, while the only historian of science in the symposium, L. Pearce Williams of Cornell University, wants to view both Popper and Kuhn with a somewhat jaundiced eye, Margaret Masterman, the only person who defends Kuhn's 'normal science' position in the symposium, strongly proclaims that "T. S. Kuhn is one of the outstanding philosophers of science of our time."[79]

In her convalescent contribution in 1966, which had been revised more in shape than the one she read in the panel discussion of the symposium in 1965, "The Nature of a Paradigm", Masterman reconstructs Kuhn's approach in SSR in line with her own expertise, computer science. She considers that though Kuhn describes his notion of a paradigm, with a quasi-poetic style, as having at least in twenty-one different senses,[80] it can be catalogued into three main ideas: (1) that of a *metaphysical paradigm,* or *metaparadigm;* (2) that of a *sociological paradigm*; (3) *that of a artefact paradigm* or *construct paradigm.*[81] Among these three main senses of paradigm, Masterman immediately catches the initial sense of Kuhn's 'paradigm' in 1965:

> If we ask, however, what a paradigm *does*, it becomes clear at once (assuming always the existence of normal science) that the construct sense of 'paradigm', and not the metaphysical sense or metaparadigm, is the fundamental one. *For only with a artefact can you solve*

[79] M. Masterman: "The Nature of a Paradigm", *Criticism and the Growth of Knowledge*, Cambridge University Press, 1970, p.59.

[80] Cf. ibid., pp.61-65.

[81] Cf. ibid., p.65.

> *puzzles*. ... A normal-scientific puzzle always has a solution (p.36) which is guaranteed by the paradigm, but which it takes ingenuity and resourcefulness to find. Typically (p.35), the solution is known beforehand, as with any other puzzle, but the step-by-step route to it is not. The normal scientist is a puzzle-solving addict (p.37); it is in this puzzle-solving -- not just vague 'problem-solving', but *puzzle*-solving -- that normal science prototypically consists. And a puzzle is always an artefact. ... this paradigm must be a construct, an artefact, a system, a tool; together with the manual of instructions for using it successfully and a method of interpretation of what it does. (CGK, p.70) [82]

Based upon her understanding of paradigm as a "construct", Masterman infers that "a paradigm has got to be a concrete 'picture' used analogically; because it has got to be a 'way of seeing'." (CGK, p.76) Contrary to what Shapere concludes, Masterman holds that Kuhn's paradigm actually draws a 'crude analogy' which has the following logical characteristics:

> (a) a crude analogy is finite in extensibility;
> (b) it is incomparable with any other crude analogy;
> (c) it is extensible only by an inferential process of replication, which can be examined by using the computer-programming technique of 'inexact matching', but not by the normal methods of examine inference. (CGK, P.79)

In a word, as an expert of computer science, Masterman insists that "we have got to re-examine what is true of analogy in the light of what Kuhn has shown to be true of paradigms" (CGK, p.88).

Nevertheless, the 1965 International Colloquium in London left a deep impression in Kuhn's mind. He spent almost four years

[82] All her page references in the quotation are to SSR.

digesting the criticism in the symposium and the other one from Shapere. The upshot of this four-year-meditation is his "three attempts in 1969" which were designed, among other things, to recover his original sense of paradigm:

(1) "Second Thought on Paradigms" (STP), the first written, though last published of the 'three attempts', was prepared for the Conference held in March 1969 at University of Illinois at Urbana;

(2) "Reflection on My Critics" (RMC) retraces some of the same ground in STP in order to reply the critique raised in the 1965 International Colloquium in Philosophy of Science held in London;[83]

(3) "Postscript-1969" (PS) was prepared as an extra chapter for the Japanese translation of SSR under the suggestion of the translator, his Japanese student, Shigeru Nakayama. Considered by Kuhn as "a brief but more balanced discussion of critical reaction" to SSR, this well-known 36-page essay (PS) was later included in the second enlarged English edition of SSR, one generally called as Second Edition (SSRE).[84]

In these three attempts, Kuhn not only planed to recover the original sense of paradigm, but also importantly to make drastic changes in his approach of *conceptual change* in SSR. For, in addition to clarifying the concept of paradigms, Kuhn seriously takes several criticisms into account, specifically

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[83] It is the closing chapter of I. Lakatos and A. Musgrave, (ed.), *Criticism and the Growth of Knowledge*, Cambridge University Press 1970, pp.231-278.

[84] Thomas S. Kuhn, *The Structure of Scientific Revolutions*, Second Edition, Enlarged, Volume II Number 2 of *Foundations of the Unity of Science*, The University of Chicago Press, 1970.

(1) Shapere's suggestion of more careful scrutiny of tools of analysis or of analyzability;

(2) Popper's and Shapere's charges of his historical relativism;

(3) Popper's critique of irrationalism and of sociological and psychological tendencies;

(4) Masterman's reference to the formalization of the process to find Wittgensteinian "families" in the area of contemporary computer sciences.

With these "constructive" critiques in mind, Kuhn's main effort was directed towards defending the analyzability of his entire approach and putting his views on the firm analytical basis of computer and language sciences. "Get rid of ambiguity" seems to become Kuhn's No.1 project for saving what he had established in 1962. He decides to do it, even at the expense of dropping the key notion of paradigm in SSR. In next chapter, I shall highlight these major shifts in Kuhn's three attempts in 1969 in order to get clearer about his changing views regarding the issues of ***conceptual change***.

5

From Historical Paradigms to Positivism?

--- *The Transformation Years after SSR (2)*

According to Frederick Suppe, by the late 1960s a general consensus had been reached among philosophers of science that it is a mistake to view the constructs of scientific theories as axiomatic calculi which are given a partial observational interpretation by means of correspondence rules. (This is the description that Putnam refers to, *The Received View on Theories.)* But it is also true that philosophers of science differ, at the same time, over what can fill the space after the *Received View* was successfully undermined. In this chapter, we shall consider Kuhn's first attempt in 1969, "Second Thought on Paradigms"(STP), to clarify the directions of his retreat, and to defend his original commitment to "conceptual change".

Although the various alternative philosophies of science proposed by Polanyi, Hanson, Toulmin, Kuhn, Feyerabend, and others were successful and provoking in undercutting the *Received View* their approaches came under increasing critical attacks from philosophers of science in the analytic traditions. As Suppe observes, "philosophy of science thus fell into a state of acute intellectual disarray"(SST, p.4), after the *Received View* had been dethroned. It is at this crucial moment that the 1969 Illinois Symposium on the Structure of Scientific Theories was convened in Urbana, March 26 to 29 " to sort out prevailing chaos and to search for new, productive intellectual directions to follow."(SST, p.4) The aim of the Urbana Symposium was originally set as follows:

> The purpose of this symposium is to bring together a number of the main proponents and critics of the traditional analysis, proponents of some of the more important alternative analysis, historians of science, and scientists to explore the question "what is the structure of a scientific theory?" (SST, p.vii)

Under this "urgency", Kuhn as one of the leaders of historical school of philosophy of science was invited to present his view of scientific theory. The paper Kuhn read at the Symposium was STP and was commented on by the organizer of the conference, F. Suppe.

For Kuhn, as a matter of fact, this symposium was also important to his heretic approach of science. Although his 1962 book continued to be widely read, appreciated, and much discussed, main-stream philosophers of science in Anglo-Saxon world were dissatisfied with the disparate direction represented by Kuhn. After four-year-reconsideration of his first combat with Popperian school at London Conference in 1965, Kuhn employed the Urbana Symposium as an opportunity to clarify his key notion of a paradigm. Monitoring conversations among the SSR's enthusiasts as well as the criticism from philosophers of science, Kuhn gradually realizes that SSR has become something that "can be too nearly all things to all people" and that for this "excessive plasticity, no aspect of the book is so much responsible as its introduction of the term 'paradigm'."(SST, p.459) For critics, whether sympathetic or not, had been unanimously underscoring the large number of different senses in which the term is used.

Further beyond what he has drawn of paradigm in his London paper, Kuhn's second thoughts on paradigms, is not only to clarify, but also, to separate the usage of paradigms in SSR into two different senses:

> One sense of "paradigm" is global, embracing all the shared commitments of a scientific group; the other isolates a particularly important sort of commitment and is thus a subset of the first.(SST, p.460)

Whereas Kuhn confesses, the term "paradigm" in SSR is closely connected to the phrase "scientific community" in such an unsatisfactory way:

> A paradigm is what the members of a scientific community, and they alone, share. Conversely, it is their possession

> of a common paradigm that constitutes a scientific community of a group of otherwise disparate men. (SST, p.460)

Consequently he realizes that the way he defined both concepts, paradigms and scientific community, was viciously circular. That is, he had failed in SSR to see that whatever kind of paradigms may, they are possessed by any scientific community, even the ones in the so-called preparadigm period. Otherwise a paradigm seems "a quasi-mystical entity or property which, like charisma, transforms those infected by it." (SST, p.461, footnote 4) During the "preparadigm period", Kuhn admits "there is a transformation, but it is not induced by the acquisition of a paradigm." (SST, p.461, footnote 4)

With this new discernment, Kuhn conceives that "if the term 'paradigm[2]' is to be successfully explicated, scientific communities must first be recognized as having an independent existence." (SST, p.460) He insists that the sociological dimension must be taken into account for philosophy of science to know more of scientific knowledge.[85] Kuhn therefore starts his demonstration of second thoughts on paradigms with sociologically defining the conception of scientific community:

> A scientific community consists, in this view, of the practitioners of a scientific specialty. Bound together

[85] This sociological thrust in STP is supported, Kuhn believes, by the fact that "the identification and study of scientific communities has recently emerged as a significant research subject among sociologists. ... I shall therefore assume that more systematic means for their identification will be forthcoming ..." (SST, p.461)

> by common elements in their education and apprenticeship, they see themselves and are seen by others as the men responsible for the pursuit of a set of shared goals, including the training of their successors. Such communities are characterized by the relative fullness of communication within the group and by the relative unanimity of the group's judgement in professional matters. To a remarkable extent the members of a given community will have absorbed the same literature and drawn similar lessons from it. Because the attention of different communities is focused on different matters, professional communication across group lines is likely to be arduous, often gives rise to misunderstanding, and may, if pursued, isolate significant disagreement. (SST, pp.461-462)

No doubt that this paragraph can be regarded as a new 'paradigm' of Kuhn's second thoughts in 1969. His reinterpretation of the notion of paradigm provides 'source materials' to his new focus on the scientific community. Of course, these 'source materials' are particularly important of how to observe the elements in a community that Kuhn now named as "disciplinary matrix", the first sense of paradigm.[86] That is, with the new phrase "disciplinary matrix", the existing tradition of scientific practice, Kuhn indicates that paradigm becomes more a common possession of a professional discipline. But, at the same time he uses "matrix" to denote something composed of ordered elements of various sorts, each requiring further specification. Nevertheless, he now admits that the notion of paradigms in SSR, now 'disciplinary matrix', in fact includes following constituents:

(1) symbolic generalizations, those expressions deployed without by the community and cast in some logical form like

[86] Cf. SST, p.462. footnote 6.

(x) (y) (z) & (x,y,z,), etc.;

(2)models, those which are what provide the community with preferred analogies with an ontology, are heuristic;

(3)exemplars, those which are concrete problem solutions, accepted by the community as paradigmatic.

The first two, Kuhn considers are already familiar objects of philosophical attention of science. Whereas the third, exemplars, he realizes, provides a new name for the second and more fundamental sense of "paradigm". It is this sense of "exemplar" of a paradigm that Kuhn draws as his primary concern to defend his approach which was already in crisis and, at the same time, to further defeat the idea of "correspondence rules" in philosophy of science.[87]

Thus in the first place, opposing the "correspondence rules" that attach theoretical terms to a previously meaningful basic vocabulary which attaches directly to nature, Kuhn doubts that "the correspondence rules discovered in this way would be nearly sufficient in number or force to account for the actual correlations between formalism and experiment made regularly and unproblematically by members of the group."(SST, pp.465-466) His answer to this difficulty is to stress, alternatively, the reality of scientific training and behaviour, and the reality that shows that very few correspondence rules are to be found in both science texts or science teaching. In this, Kuhn reveals and emphasizes a different dimension, echoing both Bridgman and Wittgenstein, that

[87] Cf. SST, pp.464-472 and footnote 11 on p.467.

"the student discovers a way to see his problem as like a problem he has already encountered."(SST, p.467) It is a dimension, Kuhn believes, that few of philosophers of science have registered in SSR when reading it:

> I suggest that an acquired ability to see resemblances between apparently disparate problems plays in the sciences a significant part of the role usually attributed to correspondence rules. Once a new problem is seen to be analogous to a problem previously solved, both an appropriate formation and a new way of attaching its symbolic consequences to nature follow. Having seen the resemblance, one simply uses the attachments that have proved effective before. That ability to recognize group-licensed resemblances is, I think, the main thing students acquire by doing problems, whether with pencil and paper or in a well-designed laboratory. ... These concrete problems with their solutions are what I previously referred to as exemplars, a community's standard exemplars.(SST, p.467)

In other words, what accounts for students of science gaining access to the cognitive achievements of his community is, Kuhn reinforces, their acquiring an arsenal of exemplars rather than his attaching theoretical terms to a previously meaningful basic vocabulary. Furthermore, Kuhn declares, this discernment is significant in that in the process there is no need to ask a question: Similar with respect to what? For Kuhn, this is the typical question of "correspondence rules" which always ask for the 'criteria of resemblance' instead of similarity. Doing problems, Kuhn emphasises, is a process of acquiring exemplars, a process which teaches students that there is nothing that certain rules can be supplied except applying concrete exemplars. Here stands the essential difference between "rules" and "paradigms":

> Where rules exist to guide them, he (the child), of course, deploys them. But his basic criterion is a perception of similarity that is both logically and psychologically prior to any of the numerous criteria by which the same identification of similarity might have been made. After the similarity has been seen, one may ask for criteria, and it is then often worth doing so. But one need not. (SST, p.472)

To further explicate this point, Kuhn undertakes an epistemological case study of how a child, Johnny, learns to identify swans, geese, and ducks in a zoological garden with his father through primary pedagogic procedure of ostension. With this case of learning by ostension, Kuhn claims, Johnny does not need to know phrases like "all swans are white". etc. Conversely, in this mode of learning, Kuhn insists, Johnny directly gains knowledge of swan as follows:

> Father points to a bird, saying, "look, Johnny, there's a swan." A short time later Johnny himself points to a bird, saying, "Daddy, another swan." He has not yet, however, learned what swans are and must be corrected: "No, Johnny, that's a goose." ... After a few more such encounters, however, each with its appropriate correction or reinforcement, Johnny's ability to identify these waterfowl is as great as his father's. (SST, pp.473-474)

Having finished this tour, Kuhn concludes, Johnny actually fulfils a transition from a situation that all ducks, geese, and swans are mixed together to one that they have clustered into discrete sets with distances between them. This cognitive transition might be altered in the event that later Johnny visits Australia where there are black swans. "But it will serve him well while he remains a member of the community that has discovered from experience the utility and viability of these particular perceptual discriminations and has transmitted the ability to make them from one generation to the next."(SST, p.478) That is, being cognitively programmed to recognize what his community already knows, Johnny has acquired the knowledge shared by his community only through learning the similarity relationship itself. But, the knowledge acquired by Johnny is, Kuhn claims, by no means the result of Johnny's acquiring explicit *criteria* for identifying swans, geese, or ducks. Rather, Johnny has learned to apply symbolic labels to nature without anything like definitions or "correspondence rules". What is more, says Kuhn, Johnny does not need to acquire those definitions or rules at all.

Based on the example of Johnny case and the above mentioned accompanying arguments, Kuhn suggests a pluralist view of scientific knowledge and thereby challenges the one-way conception, the view that scientific knowledge is only acquired and stored with recourse to verbal generalizations. In terms of pluralist view, Kuhn alternatively proclaims that it is likely the opposite that human knowledge is acquired in plural ways such as symbolic generalization, modelling, as well as the cognitive process like those exemplified in the Johnny case,through perception of similarity. And this pluralist attitude vis-à-via the conference title, The Structure of Scientific Theory, should be, in Kuhn's view, indispensable to an adequate reconstruction of scientific knowledge.

Nevertheless, Kuhn holds that the cognitive process by learning exemplars is in fact a more essential feature and function in the structure of human knowledge and, of course, in understanding its structures. On the one hand, therefore, he considers the Johnny case an example of socialization for any community member. For acquiring solutions to a problem that the members of one's prospective community had already solved is a socialization procedure that makes one part of that community and that makes one learn about the world which the community inhabits. On the other hand, not only sciences like taxonomy, but also the more abstract sciences employ the same technique as the Johnny case has shown. For example, only after a number of such problems as the inclined plane and the conical pendulum have been assimilated, can a student or a professional proceed to identify other Newtonian problems for himself. Kuhn therefore maintains that

> Johnny's case should suggest why I continue to insist that shared examples have essential cognitive functions prior to a specification of criteria with respect to which they are exemplary.(SST, p.479)

Also to convince the mainstream philosopher of science of the analyzability of Johnny case, Kuhn in STP makes another interesting and tentative move, that of modelling the Johnny case on a computer. Apparently, this move was specifically inspired by the Cambridge computer expert, Margaret Masterman, who in her paper, "The Nature of a Paradigm", envisaged that a paradigm has got be a 'way of seeing', "a concrete model at the heart of any mathematics used in science."(CGK, p.78) In terms of this, Masterman believes that a paradigm has got to have the property of concreteness, or 'crudeness', or simply the logical characteristics:

> This means that it must either be, literally, a model; or, literally, a picture; or, literally, an analogy -- drawing sequence of word -- uses in natural language; or some combination of these. (CGK, p.79)[88]

Although the formalization of the process of finding Wittgensteinian families, i.e. the mathematics of classification, or of 'clumps', has quite a literature in computer science, Masterman confesses, the set of procedures for making a digital computer make an 'inexact match' between two formulae which are highly similar to one another has almost no literature.[89] Whereas this fact does not mean, Masterman holds, that we do not make the effort to ascertain Kuhn's thought and to develop it in the light of progressing replicating-systems in the area of computer science. Rather, at the last sentence of her essay, Masterman urges that "we have got to re-examine what is true of analogy in the light of what Kuhn has shown to be true of paradigms." (CGK, p.88)

In addition, Kuhn admits that he came to see that paradigms can be understood in two different ways after he read Masterman's later version of an earlier paper which she originally read in the

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[88] These kinds of 'crude analogy', Masterman defines, have the following logical characteristics:
(a) a crude analogy is finite in extensibility;
(b) it is incomparable with any other crude analogy;
(c) it is extensible only by an inferential process of 'replication', which can be examined by using the computer-programming technique of 'inexact matching', but not by the normal methods of examine inference. (CGK, p.79)

[89] Cf. CGK, pp.84-87.

1965 Colloquium in London (not yet the one in CGK).[90] Therefore, the second sense of paradigms, exemplars, Kuhn now reports in STP, can also be regarded as a process which can readily be modeled on a computer. Although he admits that he was in the early stages of such a computer experiment, Kuhn envisages that the transformation in Johnny case can eventually function on a computer.

> If the machine is given stimuli which can be grouped in clusters and if it is formed which stimuli must be placed in the same and which in different clusters, it can design an appropriate set of transformation functions for itself.(SST, p.474)

With this new recourse to computer science, Kuhn manifestly aims to challenging those charges from analytical philosophers of science that the paradigm approach is logically unanalyzable as well as irrational relativism.

But Kuhn does not elaborate much on the possibility of computer modelling for the first sense of paradigms which he distinguishes in STP as "disciplinary matrix". Only in one paragraph in STP, with the help of the Johnny case does Kuhn claim that "each new experience can demand some adjustment of the class boundaries."(SST, p.480) And he considers that in terms of boundaries change, there is such a thing as meaning change or change in the range of application of a term. Unfortunately, his demonstration of "disciplinary matrix" is so vague that it seems

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[90] Cf. CGK, p.85. In view of this relationship between Kuhn and Masterman, I assume that the two divisions of a paradigm in SST, disciplinary matrix and exemplars, might be the results of that communication.

that the most significant feature in SSR, the meaning change of a scientific "disciplinary matrix" has been totally replaced by the importance of exemplars, a learning process of acquiring scientific knowledge which already has certain shared meaning in a scientific community.

And surprisingly, without further explicating the historical feature of the development of a disciplinary matrix and its feasibility of computer modelling, Kuhn declares:

> Shared examples can serve cognitive functions commonly attributed to shared rules. When they do, knowledge develops differently from the way it does when governed by rules. This paper has, above all, been an effort to isolate, clarify, and drive home those essential points. If they can be seen, we shall be able to dispense with the term "paradigm", though not with the concept that led to its introduction.(SST, p.482)

However, Kuhn's defense as well as his suggestions regarding the plurality of our cognition did not convince the analytic philosophers of science. On the contrary, the commentator of Kuhn's paper, F. Suppe, suggested that Kuhn should "stop populating science with new entities" (SST, p.499) and completely divorce his attempt from those assumptions of "disciplinary matrix" and its shared "exemplars". Suppe's suggestive as well as destructive advice is based upon his critique to Kuhn's second thoughts on paradigms.

For Suppe, the study of exemplars is simply a process that one learns to apply symbolic generalizations to nature. Thus, Suppe maintains that the main indication of this kind of study, Kuhn's

Johnny case and possibly computer simulation of it, is no more than "cases of learning language through a process of ostensive definition."(SST, p.487) And this sort of definition, Suppe points out, is only for those who learns how to apply words to things, which does not give a plausible account of how one learns to employ whole sentences. Johnny case is too simple, Suppe insists, to account for the ability acquired by study of exemplars to apply symbolic generalizations to nature. For Suppe, "the exemplar presents an informal description of experimental set up and, *inter alia*, indicates how this description is to be translated or rendered into the language of theoretical generalizations."(SST, p.488) That is, the phenomena of exemplars, Suppe considers, is a problem of language, a problem of how to connect two different statements or descriptions in two discrete languages. The study of exemplars is therefore no more than a problem of translation, one "to acquire a fluency in translating from informal description into the canonical descriptions of the symbolic generalizations."(SST, p.488) It is manifest then that Suppe replaces Kuhn's similarity or resemblance approach in STP with an analytic interpretation:

> Only after one has acquired the ability to make such translation fluently does one acquire the ability to think in the language of symbolic generalizations and apply them directly to nature.(SST, p.489)

In light of this interpretation of exemplars, Suppe encourages Kuhn to abandon his newly developed notion of "disciplinary matrixes". For the study of exemplars presupposes a position "to apply the same or similar descriptions to things or phenomena"

(SST, p.490),[91] a position that "involves the acquisition of a *resemblance relation* which groups them together in clusters."(SST, p.490) And "the acquisition of the resemblance relation through the study of exemplars does, in a important sense, define the terms occurring in symbolic generalizations."(SST, p.491). Therefore, Suppe depicts the nature of Kuhn's exemplars in terms of a tacit assumption that employs *resemblance relation* to establish an individuating mark of scientific community which accounts for why full communication rarely occurs between different communities. With this understanding, Suppe further questions Kuhn's notion of incommensurability, the communication break-down between disparate communities, as follows:

> In effect, the connections in the symbolic generalizations establish a crisscrossed network of interlocking terms or concepts, and the resemblance relation enables one to apply that whole network of concepts to nature.(SST, p.492)

According to this interpretation, it is not the resemblance relation that supports Kuhn's interpretation of disciplinary matrix but rather converse, namely the relation itself undermines Kuhn's perspective of incommensurability. For Suppe, the symbolic generalizations occurred in the exemplars and the study of them supplies theoretical terms with the possibility of communication which also provides basis of a partial definition to nature. In

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[91] I do not think that at this point Suppe captures what Kuhn has in mind of learning an exemplar. Rather, it seems that Suppe interprets Kuhn with the knowledge he acquired from his way of learning which is apparently quite different from Kuhn's enlightenment in 1947.

fact this possibility is, Suppe alleges, in the coverage of correspondence rules to which Kuhn is absolutely opposed. Exposing this potential crisis in Kuhn's second thoughts on paradigms, Suppe suggests:

> The only way I can see to avoid this claim is to give up the assumption that possessors of different disciplinary matrixes invariably attach different meanings to whatever words they possess in common; doing so would in turn require that Kuhn give up or severely attenuate the various incommensurability claims made in *The Structure of Scientific Revolutions*. (SST, p.493)

Thus what Suppe tries to show is that Kuhn has no choice but totally divorces from the shared entities of a scientific community -- 'disciplinary matrixes' and 'exemplars' in STP.

Almost all those who participated in the discussion after Kuhn's reading STP questioned his second thoughts on paradigms. Dudley Shapere as usual asks Kuhn how much of the relativism in SSR he is still willing to keep and whether he plans to return to the analysis of the rationale behind scientific progress. Sylvian Bromberger conceives that the Johnny case does not at all show the "reconstruction" by means of correspondence rules cannot work and that, rather, what Kuhn has shown in the Johnny case is quite obvious that rule learning can do the job. Hilary Putnam argues on the one side that Kuhn gives too much to positivism, and on the other side that his new thoughts in fact are very similar with the famous Popperian logic of discovery. And Patrick Suppes argues that Kuhn emphasizes those aspects of psychology at the expense of the great philosophical problem, that of the general concept of scientific method.[92]

To all these charges, Kuhn systematically responds, especially to the commentary by Suppe.[93] Confronting the accusation of relativism and irrationalism, Kuhn declares that what he most intends to resist about the question of similarity relationship is the implication stressed by Shapere that the question must have a yes or no answer. Kuhn argues that to regard scientific development as a unidirectional and irreversible process is by no means a relativistic view, but it does not mean that a philosopher of science in that tradition can always seek for the sorts of rules which Johnny will have learned by the end of the afternoon tour in the park beforehand. It is this incommensurable difference between traditions, Kuhn implies, that makes philosophers in that tradition hardly see that what he commits to, a similarity-processing procedure rather than a criteria-processing. Defying the current American philosophic tradition, Kuhn states the following:

> I really want to know what sort of thing knowledge is, what it is all about, and why it is that it works the way it does. Now in order to do that, it seems to me the right move (I am glad somebody else said philosophy is an empirical enterprise) is to look around and try to see what is going on and what it is that people who have knowledge have got. If I then think that what I discover when I look gives me certain sorts of understanding of what goes on -- makes it plausible that knowledge should be the sort of thing it is and should develop the way it does -- then I can legitimately say that from the

[92] Cf. SST, pp.506-517.

[93] It should be noticed that Kuhn's reply in SST was a reconstruction of his original comments on the version of Suppe's commentary read at the symposium, one also revised after the symposium.

> examination of scientific communities I am beginning to become a better epistemologist.(SST, pp.512-513)

It is the analytical tradition, Kuhn argues, that now totally drives away the Received View while on the other hand barring elements of psychology and sociology as roots of relativism and irrationalism. It seems odd, for Kuhn that discussion of the *Structure of Scientific Theories* should not epistemologically mention anything of the supposed philosophical data from psychology and sociology.

Replying Suppe's critique, Kuhn holds not only that Suppe misunderstood his intention to introduce "disciplinary matrix" rather than to employ "theory" or a "conceptual framework", but also that Suppe misconstructs what he is in mind of a "theory" in terms of a traditional way to which Suppe currently opposes, i.e. "the received view". Suppe is still preoccupied, it seems to Kuhn, by a scientific tradition of theory which is incommensurable with Kuhn's approach of theory:

> What I have been trying to say, however, is that such traditional constructions are at once too rich and too poor to represent what scientists have in mind when they speak of their adherence to a particular theory. On the one hand, the traditional constructions contain a large number of specific generalizations than are shared by those who adhere to the theory in question; on the other, they omit elements which are critically important in providing the scientist's theory with content.(SST, p.501)

Among those elements they have omitted, Kuhn emphasizes, the concrete problem solutions, exemplars, is one of the essential vehicles for the cognitive content of a theory. Specifically, "exemplars can provide detailed knowledge of nature in the virtual absence of anything the philosophical tradition would recognize as a theory."(SST, p.501) Suppe is simply not aware of the fact that, Kuhn criticizes, it is the nature that language must be learned. And this explains why he is in favour of "disciplinary matrix" which consists of verbal and symbolic generalizations together with examples of their function in use, rather than "conceptual framework" which, Kuhn claims, retains all the plasticity and vagueness of what Suppe alleges in his position.

In further defense of the Johnny case, Kuhn states that Suppe misinterprets the case in two central aspects. First, more than "ostensive definition" is involved. For the information Johnny acquired in learning to use linguistic entities is in part law-like. Second, "the procedure employed during learning transcend the usual limits of ostension."(SST, p.504) It is the results of that *transcendence*, meaning rather than the verbal definition that Johnny acquired that accounts for the difference between Kuhn and Suppe. In this, Kuhn responds Suppe's charges in opposition of his philosophical tradition:

> My main and most persistent criticism of the recent tradition in philosophy of science has been its total restriction of attention to syntactic at the expense of semantic problems.(SST, p.504)

That is, with this claim, Kuhn argues that Suppe's accusation of corresponding rules to STP, i.e., attaching word-strings to nature, misses its central intention.

Although he concedes that Suppe's formulation of translation is relevant to exemplary problem solutions, Kuhn considers that there is something about learning to do these translations that Suppe misses. For it is not only the language that accounts in the issue, Kuhn assumes. In the Johnny case, its relation to translation is more of "Quine's radical translator who requires not simply utterances but also the stimuli which prompt them before he can begin to learn to translate."(SST, p.505) In this, Kuhn insists that his understanding of translation would still be the reverse of Suppe's:

> Though exposure to exemplars teaches students to translate, there need be nothing about any exemplar which "describes" or "indicates" how to translate.(SST, p.506)

His presentation to the symposium, STP, the first attempt to defend his approach in SSR, "was drawn from work-in-progress, and it is therefore at least as crude, preliminary, and simplistic", Kuhn does not "for a moment suppose it complete, and I have not stopped work."(SST, p.506)

Kuhn's further attempts, "Reflections on My Critics" (RC) and "Postscript-1969" (PS), will be considered in the next chapter. They were finished in the same year, 1969, in order to clarify his understanding of paradigms and incommensurability.

6

Kuhn's Linguistic Turn

--- *The Transformation Years after SSR (3)*

To confront the increasing criticism from philosophers of science, as we noted in last chapter, Kuhn in 1969 wrote out another two essays, RC and PS, to further explicate his key notions of paradigm and incommensurability. In this chapter, I shall detail his new shifts and thrusts in RC and PS, which are along the transformation line of STP, but are more systematic as well as more dramatic. This year marks, in certain sense, the final abandonment of Kuhn's notion of paradigm in its sense of SSR while the notion of incommensurability is exposed to more severe challenges.

In 1965, Kuhn's "Logic of Discovery, or Psychology of Research?" was finished in a rush. Since suddenly one of the three main speakers to the July 13 symposium, P. Feyerabend could not come, and the other of the three, I. Lakatos, found that his job in arranging the colloquium had kept him busy approximately twenty-four hours a day and could not write, John Watkins as the replacement of those two was eagerly waiting for Kuhn's paper so as to rush his reply, "Against 'Normal Science'".[94]

This was also the case when Kuhn wrote his second defence, "Reflections on my Critics"(RC), in 1969. For taking the advantage of the editorial policy of the *Proceedings*, CGK, of the 1965 Colloquium, a policy that encourages "rational reconstruction" and "expansion" rather than a faithful report of the actual discussion, Lakatos and Feyerabend found enough time to finish their discussions in 1969. But, their critique created a second round of rush for Kuhn's battle with the publication deadline, therefore, allowed RC almost no time for it, although he had to wait for their paper finished since in their contributions to CGK Lakatos and Feyerabend had respectively raised very critical charges to which he was bound to reply.

Lakatos' contribution to CGK, "Falsification and the Methodology of Scientific Research Programmes", was designed to

[94] Before the unexpected accident, Kuhn had thought that his paper would not need to be ready until July 13, 1965. Now Kuhn had to rush bits of his paper across the Atlantic as they just left his typewriter as Watkins, a replacement commentator, had to see the paper beforehand.

defend the Popperian school.[95] Standing in the Popperian position, Lakatos planned to convince Kuhn to "understand a more sophisticated position the rationality of which is not based on 'naive' falsificationism", and to further strengthen the stronger Popperian position which "may escape Kuhn's strictures and present scientific revolutions as constituting rational progress rather than as religious conversions."(CGK, p.93)

Comparing Kuhn's dogmatic attitude towards science, Lakatos considers his own conceptual framework for dealing with ***continuity*** in science as "normative" vis-à-vis Kuhn's "socio-psychological" approach.[96] Lakatos assumes that this methodology difference accounts for the reason why where Kuhn sees 'paradigm', Lakatos sees rational 'research programmes'. Lakatos also criticizes Kuhn's exclusion of any possibility of rational reconstruction of the growth of science.[97] That is, "*in Kuhn's view there can be no logic, but only psychology of discovery*", Lakatos alleges, "in Kuhn's view scientific revolution is irrational, a matter for mob psychology."(CGK, p.178) With as much force as Watkins, Lakatos concludes the current battle field in philosophy of science viewed

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[95] Not long after 1965 London Colloquium, Lakatos first wrote out "Criticism and Methodology of Scientific Research Programmes" for *the Aristotelian Society* (in the *Proceedings of the Aristotelian Society*, 69, 1968, pp.149-186). Further expanding its content, Lakatos developed the paper into a lengthy contribution to CGK (almost a third of the whole volume) and finally finished it in 1969.

[96] Cf. CGK, p.177.

[97] Probably this is the reason, I suppose, why Lakatos set up the editorial policy of 'rational reconstruction' to cope up with his belief of rational continuity in discourse.

through faithful "Popperian Spectacles":

> After the collapse of Newtonian physics, Popper elaborated new, non-justificationist critical standards. Now some of those who had already learned of the collapse of justification rationality now learned, mostly by hearsay, of Popper's colourful slogans which suggested naive falsificationism. Finding them untenable, they identified the collapse of naive falsificationism with the end of rationality itself. The elaboration of rational standards was again regarded as a hopeless enterprise, the best one can do is to study, they thought once again, the Scientific Mind. Critical philosophy was to be replaced by what Polanyi called a 'post-critical' philosophy. But the Kuhnian research programme contains a new feature: we have to study not the mind of the individual scientist but the mind of the Scientific Community. Individual psychology is now replaced by social psychology; imitation of the great scientists by submission to the collective wisdom of the community. (CGK, pp.178-179)

In Lakatos' view, Kuhn's psychology of science obscures the fact that there exist 'three worlds' in terms of Popper's theory of knowledge: "the -- rationally reconstructed -- growth of science takes place essentially in the world of ideas, in Plato's and Popper's 'third world'."(CGK, pp.179-180) So, Lakatos disparages Kuhn's psychological description of science as "a caricature of a caricature".

As for Feyerabend's argument, it is rather a different story. Knowing each other quite well, Feyerabend and Kuhn, as colleagues in the philosophy department at the University of California at Berkeley in the years 1960 and 1961, had frequently discussed and debated various aspects of science. In particular, Feyerabend had read earlier drafts of Kuhn's SSR and had discussed its contents with Kuhn in detail. Although their debates at Berkeley were inconclusive, they had each benefited from them.[98] What Feyerabend could not accept was Kuhn's view about the general *ideology* of science which Kuhn proposed as the background of his thinking. For Feyerabend regards the ideology of science as one that "could only give comfort to the most narrow-minded and most conceited kind of specialist."(CGK, p.197) Not only would the ideology tend to inhibit the advancement of knowledge, Feyerabend believes, but also, in terms of the quality of our life. For "it is bound to increase the anti-humanitarian tendencies."(CGK, p.197)

In his contribution to CGK, "Consolations for the Specialist", Feyerabend criticizes Kuhn's concept of 'normal science' while defending their shared notion of incommensurability.[99] Feyerabend's paper was also carefully prepared, a earlier version of it had read in Popper's seminar at the London School of Economics in March, 1967. Thus unlike his 1963 book review of Kuhn's "The Function of Dogma in Scientific Research" in *Scientific Change*,[100] Feyerabend

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[98] Their debates were sometimes carried out in the now defunct *Cafe Old Europe* on Telegraph Avenue at Berkeley and even amused customers there, Feyerabend once recalled.

[99] However, eighteen years later, Feyerabend has softened his resistance of Kuhn's "normal science". In his 1987 book, *Farewell to Reason*, Feyerabend realizes the importance of "traditions", something can be certain in history: "But while I was still using abstractions (such as the idea of a 'free society') to arrive at a wider and more human point of view, her (Grazia Borrini's) ideas were part of 'historical traditions' (to relapse into my own constipated manner of speaking). I *did* know about these traditions and I had written about them even before I met Grazia, but again it needed a concrete encounter to make me realise what that implied" (*Farewell to Reason*, Verso, 1987, p.318).

[100] A. C. Crombie (ed.), *Scientific Change*, New York: Basic Books, 1963, pp.347-369.

in the essay systematically deconstructs Kuhn's approach from points such as its ambiguity of presentation, the aim of science, to the existence of normal science. But, Feyerabend's attitude towards those flaws is quite different from Lakatos'. Too much rationality, Feyerabend holds, is involved in Kuhn's commitment to science. Feyerabend argues that Kuhn "assumes that a tremendous historical change must exhibit a logic of its own and that the change of an idea must be reasonable in the sense that there exists a link between the *fact* of change and the *content* of the idea changing."(CGK, p.213) Whereas this account, says Feyerabend, is only plausible if we are dealing with 'reasonable' people. For Feyerabend conceives that the nature of science is of proliferation which "means that there is no need to suppress even the most outlandish product of the human brain."(CGK, p.210) In this, Feyerabend pleads for hedonism as an alternative to replace Kuhn's image of normal science.

But on the other hand, Feyerabend defends Kuhn against Lakatos in view of that science both is, and should be, more irrational than Lakatos is prepared to admit. However, Feyerabend considers his defence of Kuhn's commitment to the irrationality of science, cannot be considered part of the business of philosophy of science. In his position, Feyerabend claims, the issue is connected with the nature of science itself, as Kuhn has shown with its complexity, with the fact that it has different aspects, and that it cannot be readily separated from the remainder of history. Criticizing Lakatos' model of scientific growth, Feyerabend defends Kuhn's irrational aspects of science, especially his notion of incommensurability, a point that Feyerabend whole-heartedly accepts.[101] What is more, irrationality, Feyerabend seems to imply, introduces incommensurability, and vice versa -- there exists no universal commensurability.[102] And Feyerabend insightfully sees through the intention of the current linguistic turn, in fact the intention of commensurability or reductionism:

> *Yet just what is anathema in linguistics is now taken for granted by logical empiricists*, a mythical observation language, replacing the English of the translators.(CGK, p.225)

He advises Kuhn that once the fact of incommensurability is thoroughly understood, and taken seriously, many ways of interpreting science will be open to us.

Kuhn's final reply to his critics in the volume of CGK,

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[101] As one of the introducers of the concept of incommensurability in 1962, Feyerabend says that he does not remember which of them, Kuhn or him, was the first to employ the term. He recalls: "I still remember marvelling at the pre-established harmony that made us not only defend similar ideas but use exactly the same words for expressing them. The coincidence is of course far from mysterious."(CGK, p.219) Their agreement is that "it might be extremely difficult to compare successive theories in the usual manner, that is, by an examination of consequence classes."(CGK, pp.219-220) Unlike what Kuhn in STP has attempted, Feyerabend continues pointing out that a perfect translation between two language community is never possible, even if we use complex contextual definitions. "This is one of reasons for the importance of *field work* where new language are learned *from scratch* and for the rejection, as inadequate, of any account he relies on (complete, or partial) translation."(CGK, p.225)

[102] This is how I read Feyerabend's paper, "Consolation for the Specialist". He himself never explicitly registers the relation between irrationality and incommensurability.

"Reflection on my Critics" (RC), not only carries on where STP leaves off, but also introduces a substantial move towards the *Linguistic Turn* that Feyerabend opposes.

As though testifying to what Kuhn describes as the gestalt-switch that divides readers of SSR into two groups, the collection of essays in CGK provides an extended example of his perspective of partial or incomplete communication. This is the typical phenomenon of communication breakdown between participants Kuhn points out, due to their incommensurable points of view. As an example of incommensurability, CGK rather illustrates a phenomenon at the heart of Kuhn's own view, This explains, says Kuhn, why his critics fire at SSR is frequently misplaced, and thereafter that it often obscures the deeper differences between them.

For Kuhn it is odd to discriminate methods used by the contributors to CGK as logic versus history and social psychology, normative versus descriptive. On the one hand, "whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains."(CGK, p. 238) There is, Kuhn thinks, nothing wrong with explaining science historically and socio-psychologically. The whole issue is, Kuhn repeats, that "there can be no set of rules of choice adequate to dictate desired *individual* behaviour in the concrete cases that scientists will meet in the course of their careers."(CGK, p.238) What is most telling, on the other hand, is the fact that "Lakatos' position is socio-psychological in its repeated reliance on decisions governed not by logical rules but by the mature sensibility of the trained scientist."(CGK, p.233) So is Popper's, Kuhn points out.[103] Concluding two aspects of above analysis, Kuhn explicates that if he differs from Lakatos, Popper, Feyerabend, Toulmin, or Watkins, it is likely with respect to substance rather than method. Because, they fail to note the special feature of SSR which follows from taking the normal group rather than the normal mind as unit. Having misconstrued the sociological basis of SSR, his critics, Kuhn argues, place their gun at their own rational reconstructions.

Where he and his opponents really differ, Kuhn holds, is concerning normal science.[104] In particular, Kuhn deems that all of his critics except Toulmin share the conviction that the central episodes in scientific advance are revolutions. Given this fundamental agreement, Kuhn argues that then they cannot disagree about normal science: something different must necessarily go on between scientific revolutions. Even Popper himself stresses, Kuhn recognizes, that "scientists necessarily develop their ideas within a definite theoretical framework."(CGK, p.51) And importantly, since revolutions always involve the rejection and replacement of some of its integral parts, the hold of a framework, Kuhn infers, the concept of normal science follows from Popper's premises. And along this premises, Kuhn pushes even further since something else follows as well: the hold of a framework on a scientist's mind may

[103] Cf. CGK, pp.235-238.

[104] But, at any rate, Kuhn does not think his position departs as far from his Popperian critics as they believe.

not be accounted for merely as the result of what Popper asserts, "the normal scientist... has been badly taught... a victim of indoctrination."(CGK, p.53) Rather, the hold of a framework simply catches the essential implications of his own description of normal science, as Popper himself acknowledged in 1934 when he said that "this is why he (a scientist) may leave it to others to fit his contribution into the framework of scientific knowledge."[105] With such a Popperian episode in hand, Kuhn insists that "the time for steady criticism and theory proliferation has passed."(CGK, p.246)

Kuhn suggests three considerations that might account for their difference about normal science. The first is of "higher morality" because, for Popper, normal science is "a danger to science and, indeed, to our civilization"(CGK, p.53) while for Feyerabend, "its defence is also incompatible with a humanitarian outlook"(CGK, p.210) is perhaps liable to "corrupt our understanding and diminish our pleasure."(CGK, p.209) The second is, Kuhn assumes, "their apparent inability to see in historical examples the detailed functions of the breakdown of normal science in setting the stage for revolutions."(CGK, p.247) The third is Watkins' charge that by the contrast with the idea of testability, Kuhn's notion of normal science, in the sense of its "ceasing adequately to support a puzzle-solving tradition"(CGK, p.30) is essentially vague.

Regarding the first charge of normal science as irrelevant and

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[105] Interestingly enough, says Kuhn, this quotation is right adopted from the first paragraph of Popper's famous book, *The Logic of Scientific Discovery*.

the third as Watkins' strange failure to see his master's idea of "testability" in principle, Kuhn challenges their second charge in that "discrimination of normal from revolutionary episodes demands close historical study, and few parts of history of science have received it."(CGK, p.251) That is, not simply the name of the scientific chronology, Kuhn emphasizes, "but the nature and structure of group commitments before and after it occurred" (CGK, p.251) accounts for what he means the deeper aspect of normal science as well as of the history of science. He confesses that perhaps the structure of his arrangement in SSR obscures the intention of his argument, i.e., that it is missing an account of nature of scientific communities. In particular, Kuhn therefore declares that he would significantly change its organization if he were rewriting the book:

> I would ... begin by discussing the community structure of science, and I would not really exclusively on shared subject matter in doing so. Community is a topic about which we have very little information at present, but it has recently become a major concern for sociologists, and historians are now increasingly concerned with it as well.(CGK, p.252)

That is, the analytic unit would be the practitioners of a given speciality, people bound together by common elements in their education and apprenticeship, aware of each other's work, and characterized by the relative fullness of their professional communication and the relative unanimity of their professional judgement. And units like these which produce scientific knowledge should be regarded as the focus of philosophy of science. It is the units that consist of, Kuhn highlights, the sociological base of his entire approach.

In light of the above understanding of normal science, Kuhn rebuts the charge of irrationality as to suppose that we possess criteria of rationality which are independent of our understanding of essentials of the sociologically scientific process. Furthermore, Kuhn challenges the 'relativist' charge by pointing out the fact that to say that the later theory of a scientific community was better than the former as a tool for the practice of normal science is by no means guaranteed in belief that the later is a better approximation to the truth. On the contrary, Kuhn argues, all those charges, 'irrationatility', 'mob rule', and 'relativism', themselves are based on a dubious assumption of translation manual that

> theories can be compared by recourse to a basic vocabulary consisting entirely of words which are attached to nature in ways that are unproblematic and, to the extent necessary, independent of theory. (CGK, p.266)

But, Kuhn regards that no such Popperian vocabulary is available. Instead, in the transition from one theory to the next, Kuhn holds, words change their meanings or conditions of applicability in subtle ways. For the successive theories after transitions are in nature incommensurable. This is the central issue, Kuhn stresses, which separates him from most of his critics and which he still wants to defend. For Kuhn, translation, which Popper regards as a weapon to undermine Kuhnian notion of incommensurability, is never as manageable as Popper assumed.[106]

With reference to Quine's theory of 'radical translation', Kuhn conceives that "a translation manual" inevitably embodies a theory an analytic hypothesis with implication for the translating of other terms as well. But thanks to the fact that translation theory or an analytic hypothesis cannot and need not be right, Kuhn concludes, the result of any error in those hypothesis may be later difficulties in communication or translation. For, although the signs used in two languages might be identical or nearly so, that most of them function the same way in both languages, and that, where function has changed, there are nevertheless good reasons for retaining the same sign. The inestimable advantage eclipses, Kuhn points out, a general fact that functional changes would be apparent if they had been accompanied by a change of sign. And those advantages brings with them penalties illustrated in both scientific discourse and history of science.

For Kuhn, learning to translate a language or a theory is learning to describe the world with which the language or theory functions, or it can be understood in the light of learning a language or a science:

> In learning either a science or a language, vocabulary is generally acquired together with at least a minimal battery of generalizations which exhibit it applied to nature. In neither case, however, do the generalizations

[106] In view of Kuhn, translation always involves compromises which alter communication. For the translator must decide what alterations are acceptable. To do that, the translator needs to know what aspects of the original it is most important to preserve and experience of those who will read his work (cf. CGK, p.268).

> embody more than a fraction of the learning process. Much of it is embodied instead in the mechanism, whatever it may be, which is used to attach terms to nature.(CGK, p.270)[107]

However, when concerning the question of how we acquire the knowledge of nature that is built into language, Kuhn seems to tone down his approach in STP:

> The definitions in a dictionary tell us something about what words mean and simultaneously inform us of the objects and situations about which we may need to read or speak. About some of these words we learn more, and about others everything of sentences, by encountering them in a variety of sentences ... These procedures for language-nature learning are, however, purely linguistic. They relate words to other words and thus can function only if we already possess some vocabulary acquired by a non-verbal or incompletely verbal process.(CGK, p.270)[108]

What is more, Kuhn acknowledges that 'a non-verbal or incompletely verbal process' is by ostension or some elaboration of it, the direct matching of whole words or phrases to nature.[109]

But, Kuhn also argues that when speaking about 'non-linguistic process like ostension', he is still making the same point that SSR

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[107] I read these sentences many a time, but I still cannot quite catch what Kuhn wants to express. The reason probably is in that Kuhn's expression, I guess, was still in the transition period or written in a rush.

[108] Kuhn's toning down is in the fact that he now agrees on the procedures of language-nature learning which he considers now as "purely linguistic"(cf. CGK, pp.270-271).

[109] Kuhn seems certain that the relevance of this last mode of language-nature learning to philosophy of science is the fundamental philosophic dispute between him and Popper. But Kuhn makes his discussion pretty muddy as he is manoeuvring to soften those charges of irrationality etc. while holding firmly his new views regarding exemplars.

aimed actually to make by repeated reference to the role of paradigms as exemplary objects of an ostension. However, employing the same division of paradigms in STP, 'disciplinary matrix' and 'exemplars', he interprets exemplars, the sorts of standard examples of solved problems, in a quite different linguistic fashion:

> doing problems is learning the language of a theory and acquiring the knowledge of nature embedded in that language. (CGK, p.272)

That is, Kuhn argues, once students have acquired the ability to see different physical situations as like each other, they can write down the symbolic forms without any problem. In other words, contemplating exemplars is an essential part of learning how the words attach to nature; equally, it is part of learning how the world behaves. With this linguistic turn, Kuhn views that the characteristic by which we make the similarity-dissimilarity relationship are parts of a language-conditioned or language-correlated way of seeing the world. Until we have acquired those parts, Kuhn claims, we do not see a world at all.

With recourse to his linguistic interpretation of scientific knowledge, Kuhn tries to convince his critics of the computer programmes (the criterion-learning and the similarity programmes) that they can virtually remove the taint of 'intuition' or 'irrationality' from SSR. In light of the linguistic nature of computer programmes, Kuhn further explains his notion of incommensurability with a view of computer programming:

> The sorts of communication breakdowns now being considered are likely evidence that the men involved are processing certain stimuli differently, receiving different data from them, seeing different things or the same things differently. I think it likely myself that much or all of the clustering of stimulus into similarity sets takes place in the stimulus-to-sensation portion of our neural processing apparatus; that the educational programming of that apparatus takes place when we are presented with stimuli that we are told emanate from members of the same similarity class; and that, after programming has been completed, we recognized, say, cats and dogs (or pick out forces, masses, and constraints) because they (or the situations in which they appear) then do, for the first time, look like the examples we have seen before.(CGK, p.276)

Moreover, Kuhn assumes that such programming must be the same, for the men involved share a history (except the immediate past), a language, an everyday world, and most of a scientific one. Therefore, he proposes, "therapeutic efforts" can be carried on to cure the sickness of communication breakdowns, i.e., the unsatisfied phenomenon of incommensurability.

Since those experiencing communication breakdown can discover the area in which it occurs, the linguistic centre of the breakdown often involves a set of terms which are deployed unproblematically but which can now be seen as they attach to nature in different ways. So, it is this set of terms, terms in a basic vocabulary (at least in the sense that their normal intra-group use elicits no discussion), that request for explication or the reason of disagreement. With this linguistic explanation Kuhn's "therapeutic technique" of incommensurability follows:

> Having discovered, however, that for inter-group discussion, these words are the locus of special difficulties, our men may resort to their shared everyday

> vocabularies in a further attempt to elucidate their troubles. Each may, that is, try to discover what the other would see and say when presented with a stimulus to which his visual and verbal response would be different. With time and skill, they may become very good predictors of each other's behaviour, something that the historian regularly learns to do (or should) when dealing with older scientific theories.(CGK, p.277)

Kuhn then optimistically concludes that this procedure provides the way for participants to translate each other's theory into his own language and simultaneously to depict the world in which that theory or language applies: incommensurability thus vanishes. And having finished a process of conversion, one is likely to find that one is already using an alternative theory. It is to this point, Kuhn declares one learns one had been sick before and the "therapeutic techniques" have dissolve the problem of communication breakdown, or the problem of incommensurability, or, in other word, the problem of paradigm transitions.

Probably this is why Kuhn considers his second attempt, RMC, as somewhat too radical and he takes his third attempt in 1969, Postscript-1969 (PS), which was originally prepared for the Japanese translation of SSR, as a more balanced contribution to defend his approach. Appeared as a postscript to the second edition (the 1970 enlarged edition: SSRE), "Postscript-1969" (PS) aims at systematic reconstruction of his approach, a reconstruction that few philosophers of science have paid much attention to. Although in the second edition Kuhn did not plan to systematically rewrite SSR, he employed PS to highlight his outline of the book had he rewritten it. As early as in his second attempt, RMC, Kuhn had expressed his expectation, if rewriting SSR, with a different focus, one that would not rely exclusively on shared subject matter in doing science, but would inaugurate by discussing the structure of scientific communities.[110] In PS, Kuhn discusses the following topics, in the first place, by distinguishing two different senses of paradigms: sociologically, 'disciplinary matrix' and, philosophically, exemplars. Here is the outline of PS:

1. Paradigms and Community exemplars;
2. Paradigms as the Constellation of Group Commitments;
3. Paradigms as shared Examples;
4. Tacit knowledge and Intuition;
5. Exemplars, Incommensurability, and Revolutions;
6. Revolutions and Relativism;
7. The Nature of Science.

Comparing with the "Contents" of SSR, the organization of PS emphasises sociology and psychology of science rather than the history of it. In this, PS can be regarded as an alternative development of Kuhnian approach from 1965 to 1969, especially a summary of his attempts or struggles in 1969. And the development ultimately aims at dissolving those charges that Kuhn has made of science a subjective and irrational enterprise.

Inaugurating with the 'analytic separation' that disentangles the concept of paradigms from the notion of a scientific community,

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[110] Kuhn states that "if I were rewriting the book now I would significantly change its organization."(RMC, p.252)

Kuhn defines the latter as a *previously determined* group in scientific society.[111] The reason that a scientific community is taken to be more fundamental rather than a paradigm as in SSR, Kuhn emphasizes, is that the latter can only be discovered by scrutinising the behaviour of a given community and that the former can and should be isolated without prior recourse to the latter. Following this change, Kuhn casts his notion of incommensurability in sociological and behaviourist terms:

> The members of a scientific community see themselves and are seen by others as the men uniquely responsible for the pursuit of a set of shared goals, including the training of their successors. Within such groups communication is relatively full and professional judgement relatively unanimous. Because the attention of different scientific communities is, on the other hand, focused on different matters, professional communication across group lines is something arduous, often results in misunderstanding, and may, if perused, evoke significant and previously unsuspected disagreement.(SSRE, p.177)

Also, a series of issues in SSR are referenced to the structure of scientific communities alone, such as

1) the transition from the pre- to the post-paradigm period;

2) the identification of scientific communities with scientific subject matters;

3) the unanimity of scientist in their allegiance to a paradigm;

4) the scientific revolution construed as involving the reconstruction of group commitments; and

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[111] Compared with the emphasis on "The Priority of Paradigms" in SSR, Kuhn has made a substantial change of his "paradigm" in PS.

5) the view that "crisis need not be generated by the work of the community that experiences them and that sometimes undergoes revolution as a result."(SSRE, p.181)

Having related his main issues to the theme of a scientific community, Kuhn reiterates his two different senses of paradigms in PS except, specifically, adding 'beliefs in particular models'. Although he continues emphasizing exemplars as "the central element of what I now take to be the most novel and least understood aspect of this book" (SSRE, p.187), Kuhn declares, in accordance with his linguistic turn in RMC, that exemplars-learning now has such linguistic feature:

> That sort of learning is not acquired by exclusively verbal means. Rather it comes as one is given words together with concrete examples of how they function in use; nature and words are learned together.(SSRE, p.191)

However, borrowing Polanyi's phrase "tacit knowledge", Kuhn considers that what results from this process is in nature tacit for it is learned by doing science rather than by acquiring rules for doing it.

In connection with the above line, Kuhn further argues in section 4 that construing "tacit knowledge" in the process of learning exemplars as well as in its rejection of rules not only by no means provides any evidence for charges of subjectivity and irrationality, but also relate nothing with charges of 'unanalyzable individual intuitions'. Pointing out the fact that intuitions are the tested and shared possession of the members of a successful group, Kuhn claims that these intuitions are not individual but that the novice acquires them through training as a part of his preparation for group-membership. Therefore, to combat the charge of unanalyzability, Kuhn optimistically mentions his ongoing computer program in order to convince readers of that he is not suggesting "a process that is not potentially fully explicable in terms of neuron-cerebral mechanism."(SSRE, p.192) But, on the other hand, Kuhn proclaims, that suggestion will not answer the question, "Similar with respect to what?". For the latter requests for rules, but the former for abilities to recognize a given situation as like some and unlike others that one has seen before.[112]

The recognition of similarity must be, Kuhn points out, as fully systematic as the beating of our hearts. What is more, that recognition does not mean that it is voluntary, that it is a process over which we have control. Due to this, it reveals a fact that it is the psychological reason that we may not properly conceive it as something we manage by applying rules and criteria. Echoing Wittgenstein, Kuhn concludes:

> To speak of it [recognition of similarity] in those terms implies that we have access to alternatives, that we might, for example, have disobeyed a rule, or misapplied a criterion, or experimented with some other way of seeing. Those, I take it, are just the sorts of things we cannot do.(SSRE, p.194)

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[112] Psychologically, that is, what has been acquired from learning exemplars in a scientific community is, Kuhn holds, that the members of it have learned to see the same things when confronted with the same stimuli. And it is true, therefore, that their seeing a situation as like ones they have encountered before must be the result of neural processing, fully governed by physical and chemical laws.

Interpretation, Kuhn further argues, is only a deliberative process in which we engage and by which we choose among alternatives as we do not in perception itself.[113] This interpretation is a cognitive process that we try to explicate our sensation already at hand, to analyze what is for us the given. Although the process is governed by the same *psysico-chemical* laws that govern perception as well as the beating of our hearts, there is no reason "to suppose that our neural apparatus is programmed to operate the same in interpretation as in perception or in either as in the beating of hearts."(SSRE, p.195) Rather, says Kuhn, interpretation only begins where perception ends; the two processes are not the same. What perception leaves for interpretation to complete depends drastically on the nature and amount of prior experience and training. To this extend, Kuhn declares the intention of his approach in PS as follows:

> What I have been opposing in this book (i.e. SSR) is therefore the attempt, traditional since Descartes but not before, to analyze perception as an interpretive process, as an unconscious version of what we do after we have perceived.(SSRE, p.195)

With this strong philosophical intention, Kuhn draws the reason why he takes learning science by doing exemplars so seriously:

> What makes the integrity of perception worth emphasizing is, of course, that so much past experience is embodied in the neural apparatus that transforms stimuli to

[113] For Kuhn, we do not deliberate in perception itself, i.e. we do not seek criteria and put them to use when perceiving. We contemplate in interpretation what we have previously perceived, searching for what we perceive in a given family has in common.

> sensations. An appropriately programmed perceptual mechanism has survival value. To say that the members of different groups may have different perceptions when confronted with the same stimuli is not to imply that they may have just any perceptions at all. (SSRE, p.195)

Challenging the traditional use of 'knowledge', Kuhn takes it as 'knowledge' of what is built into the neural process that transforms stimuli to sensation. His understanding of knowledge thus has the following educational characteristics:

> it has been transmitted through education; it has, by trial, been found more effective than its historical competitors in a group's current environment; and, finally, it is subject to change both through further education and through the discovery of misfits with the environment. (SSRE, p.196)

In addition, however, Kuhn reinforces another important characteristic of knowledge which is generally missing:

> We have no direct access to what it is we know, no rules or generalizations with which to express this knowledge. Rules which could supply that access would refer to stimuli not sensations, and stimuli we can know only through elaborate theory. In this absence, the knowledge embedded in the stimulus-to-sensation route remains tacit. (SSRE, p.196)

Granted for these characteristics of knowledge, Kuhn remarks, they provide a psychological basis for philosophically clarifying the notion of incommensurability.

There are three points to note here. In the first place, the notion accounts for the fact that debates over "theory-choice" cannot be cast in a form that fully resembles logical or mathematical proof. Rather, these proofs reveal the nature of theory-choice that premises and rules of inference are stipulated from the start:

> Only if the two discover instead that they differ about the meaning or application of stipulated rules, that their prior agreement provides no sufficient basis for proof, does the debate continue in the form it inevitably takes during scientific revolutions.(SSRE, p.199)

In the second place, it is the debate itself to prelude a fact that there are no good reasons for not being persuaded and that those reasons are ultimately decisive for the future of a scientific community. It is important in that, Kuhn considers, those reasons function as "values", as most of his critics assume, which can thus be differently applied, individually and collectively. However, the value-function is not of the details of biography and personality but rather that

> a particular set of shared values interacts with the particular experiences charged by a community of specialists to ensure that most members of the group will ultimately find one set of arguments rather than another decisive.(SSRE, p.200)

Thirdly, Kuhn infers to be further unravelled, i.e. problems occurring in communication breakdown between two different communities:

> Two men who perceive the same situation differently but nevertheless employ the same vocabulary in its discussion must be using words differently. They speak, that is, from what I have called incommensurable viewpoints.(SSRE, p.200)

After any scientific revolution, namely, two men whose discourse had previously proceeded with apparently full understanding may suddenly find themselves responding to the same stimulus with incompatible descriptions and generalizations. Or, more generally, two scientists perceive the same situation differently but nevertheless employ the same vocabulary in its discussion. Communication breakdowns occur.

In fact, this view of communication breakdown is quite different from that in RMC.[114] Communication breakdowns of this sort, Kuhn thinks, means that two scientists in different communities "must be using words differently". Now Kuhn seems to realize that communication is not merely linguistic, and thus cannot be breakdown simply by stipulating the definitions of troublesome terms. For the participants in a communication breakdown cannot say that they employ those terms in ways determined by certain criteria. In other words, the problem itself is that they cannot simply resort to a neutral criteria or language which both agree in the same usage. Part of the difference in the communication breakdown is prior to the application of the languages in which it is reflected. What they can first resort to, Kuhn claims in a tone quite different from that of RMC, is to

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[114] In RMC, Kuhn with recourse to Quine's "translation manual" interprets that the linguistic centre of communication breakdowns can be seen as that a set of terms is attached to *nature* in different ways. And these different ways can be solved to resort to their shared everyday vocabularies in a further attempt to elucidate their troubles. However, in PS, Kuhn retains his stance in SSR that even in the area of everyday vocabularies scientists in different communities must be using words differently. Perhaps this is of the reason that Kuhn considers PS as a more balanced account of his original approach (cf. CGK, p.277).

understand the terms about which difficulties cluster have been learned in part from direct application to exemplars, and that the practice of normal science depends on the ability acquired from that application. For the ability is to group objects and situations into similarity sets in the sense that the grouping is done without an answer to the question: "Similar with respect to what?". Recalling the view in STP, Kuhn then insists that those who experience such communication breakdowns now can have recourse to resolve the problem in reference to the nature of similarity relations change.

But it is also confusing that Kuhn suggests that the participants in breakdowns recognize each other as members of different language communities and then become translators, much like that of the "therapeutic technique" in RMC in terms of solving problems in communication breakdowns.[115] But now, Kuhn restricts the technique mostly into "what the historian of'science regularly dose (or should do) when dealing with out-of-date scientific theories."(SSRE, p.202)

On the other hand, Kuhn now distinguishes two kinds of translation experiences, persuasion and conversion, which he had only fully recognized after finishing RMC. Since translation, if

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[115] Interestingly, paragraph two on the page 202 in SSRE seems copying the view of paragraph three on the page 277 in CGK without any notice that the former is under the paradigm of similarity relations change rather than the one of translation skills. Kuhn was not aware in PS that Quine's translation manual is completely incommensurable with his view of similarity relations change in that Quine's view of translation has nothing to do with acquiring meanings by doing science.

pursued, appears a potent tool both for persuasion and for conversion experiences of the two are not always the same. To persuade someone is to convince someone that one's view is superior and ought therefore supplant his own.[116] This kind of success of persuasion, Kuhn remarks, comes particularly to those just entering the profession since they have not yet acquired the special vocabularies and commitments of either group. But, among those already admitted to a profession, Kuhn holds, translation is still needed in that as translation proceeds some members of each community may also begin to understand how a statement previously opaque could seem an explanation to members of the opposing group. And translation to this extent, Kuhn concludes, cannot be amounted to persuasion any more for the former starts a threatening process which is entirely foreign to normal science limits.

At the stage beyond translation, therefore, conversion experience, Kuhn adds, accounts for the transition at the heart of the revolutionary process rather than the persuasion in normal science. So, Kuhn links the conversion experience to gestalt switch, the revolutionary phenomenon in SSR. For Kuhn holds that, good reasons for choice only provide motives for conversion and a climate in which it mostly likely to occur, and translation may, in addition, provide points of entry for neural reprogramming that might underlie conversion. But neither good reasons nor translation

[116] And this much of persuasion, Kuhn recognizes, can be occasionally achieved without recourse to translation but only with recourse to concrete research results that cannot yet be accounted for by the other community in its own terms.

constitute conversion, Kuhn claims, and it is that process we must explicate in order to understand an essential sort of scientific change. In his mind, Kuhn still aims at the process of learning scientific exemplars.

Challenging those charges of relativism from Popperian school and Shapere, Kuhn still firmly insists in section 6 along the line of SSR:

> There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems to me illusive in principle. Besides, as a historian, I am impressed with the implausibility of the view.(SSRE, p.206)

Confronting the methodological charge of the "descriptive" from Lakatos, Kuhn states that the charge has been no longer honoured and that a number of contemporary philosophers have discovered important contexts in which the normative and the descriptive are inextricably mixed.[117] No matter what kind of methodology it is, Kuhn remarks, a theory about consequences for the way in which scientists should behave it their enterprise is nothing but to succeed.

All in all, Kuhn concludes, the nature of science should be viewed in the dimension that scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. In other words, the more adequate questions that philosopher of science should be asked, Kuhn suggests, ought to be those

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[117] In this context, Kuhn refers to Stanley Cavell's book, *Must We Mean What We Say?* New York, 1969, Chapter I.

focused on the following issues:

1. How does one elect and how is one elected to membership in a particular community, scientific or not?

2. What is the process and what are the stages of socialization to the group?

3. What does the group collectively see as its goals?

4. What deviations, individual or collective, will it tolerate?

5. And how does it control the impermissible aberration?

We shall return to these topics later.

7

From the Sociology and Psychology of Science to Hermeneutics?

--- *The Transformation Years after SSR (4)*

After his three attempts in 1969, Kuhn continued to change his views of paradigms. However, it is certain that the key issue he wanted to defend is still in the domain of incommensurability, the last and the most vicious perspective in his entire approach. In this chapter, I shall detail Kuhn's shifts regarding his idea of incommensurability in order to finally outline his changing views since 1965.

In the same year as the second edition of SSR (SSRE) appeared, *The Time Literature Supplement (TLS)* commented on Kuhn's "Postscript-1969" (PS). According to the author,

> (Kuhn) keeps his temper but loses much of the edge of his argument in diffuse philosophical profundity. He might recover his provocative attack by turning from ancient controversies to a modern instance, e.g., the plight of aerodynamicists since Ludwig Prandtl showed (in about 1920) that the velocity of a gas always falls into zero in flowing over an aerofoil, thus making beautiful nonsense of classic nineteenth-century aerodynamics. Boundary layer theory is as good a living example of painful paradigm change over as Dr. Kuhn will find. But in analyzing it he should try to write more simply.[118]

But the reviews from philosophers of science that came in the following year, 1971, were not so suggestive as the one in the *Times Literature Supplement*.

In the January issue of British journal, *Encounter*, Toulmin discussed Kuhn's defense in both SSRE and CGK in an insightful review of 20th-century history of ideas, "Rediscovering History -- New Directions in philosophy of Science". Toulmin conceives that in PS Kuhn has retreated much further than he need have done from his original position. That is, Kuhn has watered down much of his earlier striking claims about the role of 'intellectual revolutions' in the development of scientific thought which Toulmin takes as the central issue of western ideas since 19th-century. Against this background, he draws the nature of Kuhn's retreat in his three 1969 papers as follows:

> His present account of the difference between "normal"

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[118] *The Time Literature Supplement*, Friday, Nov. 20, 1970, p.1368.

> and "revolutionary" change in science, in fact, simply reduces it to the distinction between those (propositional) changes which call for some kind of a quasi-deductive justification, and those other (conceptual) changes which go beyond the scope of existing deductive procedures. ... For its effect would then have been to disguise a valid (but, a-temporal) *logical* distinction in irrelevant *historical* fancy dress.[119]

However, it seems to Toulmin that two main diachronic issues still remain after the debate between Kuhn and his critics:

1) how are we to set about describing the historical transitions by which one set of scientific concepts succeeds another?

2) how do questions about "rational choice" and "good reasons" find a place in the resulting theoretical debate?

For Toulmin, Kuhn's view of scientific development, one that it is irreversible and unidirectional, still flounders on the same rock that Lamarck and Spencer stands but that Darwin avoided.

Shapere, reviewing SSRE and CGK in the May 14, 1971 issue of *Science*, also detected that "it is important to recognize the extent and the significance of Kuhn's withdrawal from his original position".[120] Pointing out Kuhn's new distinction of two different senses of a paradigm and its intellectual association and parallels with the views of Bridgman and Frank, etc., Shapere holds that Kuhn's new move is of little help to those who already found the earlier concept of paradigm obscure. In particular, Shapere claims

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[119] *Encounter*, 36, 1971, p.60.

[120] D. Shapere, "The Paradigm Concept", *Science*, Vol. 172, No.3984, p.707.

that Kuhn never adequately clarifies how the remaining factors (beliefs, values, symbolic generalizations, etc.) covered by paradigm are related to exemplars. Like Toulmin, Shapere praises Kuhn for having moved into the direction of a concern with the details of scientific reasoning. But he is not satisfied with Kuhn's stubborn unwillingness to abandon "incommensurability" without attempting to explain that communication is possible:

> This is partly due to his residual ambiguity regarding the extent to which paradigms determine meanings and views of "nature" ... This ambiguity in turn destroys the effectiveness of his suggestion that Quine's views on translation can help alleviate the difficulty.[121]

Given Kuhn's explanation of theory choice which puts much more weight on "values" rather than "reasons", Shapere further criticizes that Kuhn still stands with the same old relativism. In this, Shapere concludes, Kuhn's new points in his attempts are not merely defective in that he denies the objectivity and rationality of the scientific enterprise, but also in that they muddy the situation much further, Kuhn's whole demonstration being even more unclear and unsatisfactory.[122]

On the other hand, Shapere seems quite "confused" regarding the degree of Kuhn's retreat:

> Kuhn appears to have retreated from his earlier position

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[121] Ibid., p.708.

[122] I think that Shapere is absolutely right, as I have showed in the last chapter, to express his confusion, although the reason for it might not be in connection to Kuhn's denial of the objectivity and rationality of science.

in just those respects in which it was most suggestive, important, and influential, and to have retained aspects which many have felt were the most objectionable features of his earlier view.[123]

Alan E. Musgrave also shares the same feeling as Shapere in his review article of Kuhn's attempts in 1969. He writes that

> In his recent writing, then, Kuhn disowns most of the challenging ideas ascribed to him by his critics. ... Kuhn's *Postscript* left me feeling a little disappointed. I find the new, more real Kuhn who emerges in it but a pale reflection of the old, revolutionary Kuhn. Perhaps this revolutionary Kuhn never really existed.[124]

Criticizing Kuhn's move to scientific communities, Musgrave argues how the membership of it can be somehow determined independently of the scientific content of its activities. Furthermore, given that communities can be isolated in Kuhn's sense, Musgrave infers that what will vanish in Kuhn's approach is his conception of "normal science". For the consensus condition of a scientific community, according to Kuhn's new definition of it, becomes innocuous when it dissolves into numbers of micro-communities. As a corollary, Kuhn's notion of "revolution" vanishes since any "anomaly" can, Musgrave remarks, produce a 'crisis' and be seen, by one, some or all members of the relevant community, as

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[123] Ibid., p.708.

[124] A. E. Musgrave, "Kuhn's Second Thought", *The British Journal for the Philosophy of Science*, Vol. 22, No.3, 1971, p.51. But, this does not mean that Musgrave aims to save Kuhn's old, revolutionary image. Instead, what is in Musgrave's mind is that, given those retreats philosophers of science can luckily decline Kuhn's sociological invitation without being "read out of the profession". So, for Musgrave, his first aim is to further attack on Kuhn's shift to scientific communities which has begun in PS.

'the end of an existing paradigm'".[125] For Musgrave, as a matter of fact, Kuhn's notion of "normal science" already vanishes because of his recent shift to sociological approach of science.

Although Kuhn's focus on paradigms as exemplars is different from his sociological approach, in Musgrave's view, it still implies that rational standards can play no role in theory-choice, and that, as a result, incommensurability, its psychological product, is still in the framework of relativism. In short, Musgrave considers that Kuhn's three attempts in 1969 will cause scarcely a flutter among those who reacted violently against what they saw in his first edition of SSR.

It is manifest that the critics' fire is now mainly aimed at Kuhn's primary intention of incommensurability, the one that enlightened him on the hot summer day in 1947.

Given what he had experienced in the PSA Boston Meeting in Fall, 1970, Kuhn did not rush to reply the 1971 critical reviews. For him, a supporting case of a real conversion to his commitment just appeared even in the arena of philosophy of science, which is, of course, more convincing than the critical analysis in those reviews. The case of a real conversion, was Lakatos' lengthy contribution to the PSA Meeting devoted to the memory of Rudolf Carnap in 1970, "History of Science and Its Rational

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[125] Ibid., p.291.

Reconstructions", one on which Kuhn was invited to comment.[126] Regarding this event, He was not only just keeping his temper but actually delighted what was happening in the frontier of philosophy of science:

> I conclude, finally, that I have read no paper on scientific method which expresses opinions so closely paralleling my own, and I am necessarily encouraged by the discovery, for it may mean that in the future I shall not be quite as alone in the methodological arena as I have been in the past.[127]

But, at almost the same time, there emerged a new campaign in philosophy to undermine Kuhn's last notion, that of incommensurability. The campaign was led by Quine's best known disciple, Donald Davidson. As early as 1970, Davidson began to prepare the sixth and last of his John Locke lectures, entitled as "Invariants of Translation", which he later delivered in Trinity Term at Oxford. In this paper, Davidson focuses on the conceptions of "conceptual schemes" and "relativism" in which Kuhn's concept of incommensurability is at centre of his fire. In January of 1971, Davidson further gave two lectures at University of London on "Alternative Conceptual Schemes" which already contained much of the form of his latter known essay, "On the Very Idea of a Conceptual Scheme".[128] As a challenge from the mainstream

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[126] *The Boston Studies in the Philosophy of Science*, D. Reidel Publishing Company, Vol. VIII, *PIA 1970: In Memory of Rudolf Carnap*, pp. 91-136.

[127] Ibid., p.137.

[128] Donald Davidson, *Inquires into Truth and Interpretation*, Clarendon Press: Oxford, 1984, pp.183-198.

philosophy to the heretic philosophy of science represented by Kuhn and Feyerabend, Davidson conveyed the result of those lectures again as the theme of his presidential address to the Eastern Meeting of the American Philosophical Association in Atlanta, on December 28 of 1973. Although his address made a big stir and encouraged mainstream philosophers to suppress Kuhnian notion of incommensurability, the final draft of Davidson's paper was not published until he gave a closely related talk, "The Third Dogma of Empiricism', to the Philosophical Society at Oxford.[129] The result was Davidson's 1974 version of his idea developed since 1970, "On the Very Idea of a Conceptual Scheme", a carefully prepared work which absorbed various opinions from Anglo-Saxon mainstream philosophers, in order to completely suppress Kuhnian temptation, the so-called "third dogma of empiricism'.

In his own understanding, Davidson interprets philosophers such as Kuhn as ones worshipping "conceptual scheme" which holds that "reality itself is relative to a scheme: what counts as real in one system may not in another".[130] Davidson regards Kuhn as a conceptual relativist who believes that where conceptual schemes differ, so do languages. In this interpretation, studying the criteria of translation for conceptual relativists becomes, as Davidson envisages, a way of focusing on criteria of identity for

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[129] The discussion on Davidson's paper at Oxford was opened by Quine, and Davidson admits that Quine's comments helped him to finish the final version of "On the Very Idea of a Conceptual Scheme". (Cf. *Inquires into Truth and Interpretation*, Clarendon Press, Oxford, 1984, p.xi.)

[130] Ibid., p.183.

conceptual schemes.

On the one hand, Davidson considers the complete failure of translatability. Believing that our form of activity is speech behaviour, Davidson assumes that "there can be no doubt that the relation between being able to translate someone's language and being able to describe his attitudes is very close".[131] With the assumption, he further argues that to give up the analytic-synthetic distinction, although it therefore gives up the idea that we can clearly distinguish between theory and meaning, has not proven a help in making sense of conceptual relativism. Conversely, in place of the dualism of the analytic-synthetic we get the dualism of conceptual scheme and empirical content, which Davidson doubts "the third dogma of empiricism". For Davidson,

> It is sentences that predict (or are used to predict), sentences that cope or deal with things, that fit our sensory prompting, that can be compared or confronted with the evidence. It is sentences also that face the tribunal of experience, though of course they must face it together.[132]

The general position, Davidson argues, is that "sensory experience" provides all the evidence for the acceptance of sentences (where sentences may include whole theories). So, what is in view here, Davidson points out, is rather the totality of possible sensory

[131] Ibid., p.186.

[132] Ibid., p.193.

evidence past, present, and future.[133]

On the other hand, the partial failure of translation, which Davidson criticizes as the typical view of 'conceptual relativists', is presupposed against a position that "the interdependence of belief and meaning springs from the interdependence of two aspects of the interpretation of speech behaviour: the attribution of beliefs and the interpretation of sentences."[134] When others think differently from us, Davidson continues, no general principle, or appeal to evidence, can force us to decide that the difference lies in our beliefs rather than in our concepts. Given this, Davidson concludes that we cannot give a solid meaning to the idea of conceptual relativism and hence to the idea of a conceptual scheme. That is, we cannot, he insists, share the relativist position to judge that others had concepts or beliefs radically different from ours. For we have found no intelligible basis, Davidson asserts, on which it can be said that schemes are different and thereby there exists an uninterpreted reality, something outside all schemes and science.

Three years after Davidson's criticism, Kuhn further amended his 1969 views. These changes appear in his preface to his 1977

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[133] As a result, with recourse to Tarski's Convention T, Davidson demonstrates that the concept of truth cannot be divorced from that of translation: "Neither a fixed stock of meanings, nor a theory-neutral reality, can provide, then, a ground for comparison of conceptual schemes. It would be a mistake to look further for such a ground if by that we mean something conceived as common to incommensurable schemes" (cf. ibid., p.195).

[134] Ibid.

book, *The Essential Tension.*[135]

In autobiographical remarks recalling the process of the birth, and growth, expansion of his notion of paradigms, Kuhn drastically shrinks the notion into a definition in his 1959 paper, "The Essential Tension: Tradition and Innovation in Scientific Research",[136] which he titles the current book in order to express what he is currently intending. In that paper, "paradigms" amount to the standard examples employed in language teaching, i.e., "the concrete problems solutions that the profession has come to accept as paradigms"(ET, p.292). Unfortunately, in Kuhn's view, in the expansion of the notion from 1959 to 1962, it took on a life of its own and largely replaced his previous usage of "consensus" for normal science. Thus he now prefers to abandon the more global use of the term in SSR and recapture its original sense in 1959. In 1977, Kuhn declares that "the word 'paradigm' is appropriate only to the original sense of the term"(ET, p.xx) which, he thinks, Wolfgang Stegmüller has in fact captured precisely in his second sense of the term, i.e. Class II.[137]

With regard to his concept of scientific community, Kuhn reports, his suggestion of researching the value systems that

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[135] Thomas S. Kuhn: *The Essential Tension*, The University of Chicago Press, 1977. It is very nearly a version in the original English of the German volume published under Lorez Krüger's supervision, *Die Entstehung des Neuen: Studien zur Struktur der Wissenschaftsgeschichte* (Frankfurt, 1977).

[136] Ibid., pp.225-239.

[137] Cf. the section "What Is A Paradigm?" in Stegmüller's *Structure and Dynamics of Theories*, (trans.) by W. Wohlhueter, Berlin, Heidelberg, and New York, 1976, pp.170-180.

govern the communities, his recognition of "meaning change" that is now persuaded largely by Quine's work. It is the latter, Kuhn proclaims, that makes him now treat the problems of incommensurability and partial communication differently:

> One thing that binds the members of any scientific community together and simultaneously differentiates them from the members of other apparently similar groups is their possession of a common language or special dialect. ... in learning such a language, as they must to participate in their community's work, new members acquire a set of cognitive commitments that are not, in principle, fully analyzable within that language itself. Such commitments are a consequence of the ways in which the terms, phrases, and sentences of the language are applied to nature, and it is its relevance to the language-nature link that makes the original narrower sense of "paradigm" so important. (ET, p.xxii)

In brief, revolutions in SSR can, Kuhn realizes, be understood now as languages expressing different cognitive commitments, suitable for different worlds. And scientists' abilities to grasp each other's viewpoints, the possibilities of communication, therefore, are inevitably limited by the imperfections of the process of translation and of reference determination, i.e., issues of partial communication. In other words, Kuhn's stance of incommensurability is largely softened under Quine's linguistic persuasion which heralds, of course, conversion according to Kuhn's analysis of persuasion and conversion in PS.

But Kuhn states that his endeavour following Quine's work of translation is only a working report which shows what currently concerns him most. Like his vanished report of computer project, Kuhn's current report on the "language-nature" link undergoes another interesting episode with which we shall deal later.

Nevertheless, Kuhn in the Preface to ET acknowledges another significant recognition of his kinship to the European tradition of ideas, especially to its hermeneutic tradition. Probably partly due to the fact that the book was originally initiated and published in Germany, the hometown of modern hermeneutics, Kuhn acknowledges its influence on his work in 1950s. Only noticing this historical background, can we appreciate that Kuhn's approach to history of science has a deep root in the European culture. This post-Kantian European tradition, hermeneutics, had influenced him and his historical colleagues so that consciously or not, Kuhn realizes, they are all practitioners of the hermeneutic method. For Kuhn, the discovery of hermeneutics, the unexpected effect of his encounter with Aristotle, did more than make history seem consequential. Its most immediate and decision effect was on his view of science. But Kuhn admits that although he was under the influence of the post-Kantian European tradition, the term "hermeneutic" was no part of his vocabulary as early as five years, i.e. in 1972. And he notices that to most his philosophical colleagues of science, they continue to find it opaque to employ the hermeneutic method in the area of philosophy of science. Therefore, Kuhn strongly suggests in 1977:

> Increasingly, I suspect that anyone who believes that history may have deep philosophical import will have to learn to bridge the long standing divide between the Continental and English-language philosophical traditions.(ET, p.xv.)

As a matter of fact, Kuhn's suggestion is one of the points focused on A. Rupert Hall's commentary on ET in *the Times Literature Supplement* in 1978:

> Kuhn has consciously restricted his vision to the United States. The only foreign scholar mentioned more than casually is Alexandra Koyré, ... Kuhn's opinions on the history of science as an academic discipline are, to say the least, rendered somewhat outmoded by his exclusion of such major historical enterprises as those undertaken by Howard B. Adelman, Joseph Needham and D.T. Whiteside,... Equally, apart from passing references to Pierre Costabel and Jacques Royer, there is no reflection of relevant continental European historical work, apart from that of Koyré.[138]

Given that most of reviews on ET were not by philosophers of science, it seemed that those collected essays in ET had been beyond the interest of philosophers of science. However, the more severe combat between Kuhn and the mainstream philosophers had already undergone along the line initiated by Davidson, the line that regards Kuhn's notion of incommensurability as "conceptual relativism". Along this line, Philip Kitcher's essay, "Theories, Theorists and Theoretical Change", cuts more deep on the issue raised by Davidson in 1974.[139]

In the same vein as Davidson but unlike him in many ways, Kitcher remarks that Kuhn and Feyerabend hold a view that we cannot

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[138] A. Rupert Hall, "Departures from Tradition", *The Times Literature Supplement*, June 23, 1978, p.698. In the comment Rupert Hall also regards Kuhn's work as in spirit to conventional philosophy of science or at least as not easy to label.

[139] Philip Kitcher, "Theories, Theorists and Theoretical Change", *The Philosophical Review*, LXXXVII, No.4, October, 1978, pp.519-547.

formulate past theories in contemporary language, a typical view of "conceptual relativism":

> *Conceptual relativism* is the doctrine that the language used in a field of science changes so radically during a revolution in that field that the old language and the new language are not intertranslatable.[140]

Starting from Frege's definition of two semantic functions of linguistic expressions---the functions of expressing a sense and of referring, Kitcher prefers the function of reference as recourse to undermining the doctrine of conceptual relativism. Arguing that for any two languages used in the same scientific field at times separated by a revolution, there are some expressions in each language whose referents can be specified in the other language, Kitcher assumes that translation would be adequate in the sense of preserving those referents. For example, Kitcher explains, Kuhn's incommensurable experience of Aristotle theory can be resolved as follows:

> A full theory of reference for the Aristotelian language is a set of completed matrices such that a name of each primitive expression of Aristotelian language occurs in the place *e* in exactly one matrix. A full theory of reference would help us to understand the sentence-*tokens* produced by Aristotelian.[141]

In particular, Kitcher studies one of Kuhn's favourite examples, the phlogiston theory, in detail and concludes that a term which is central to the presentation of the phlogiston theory

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[140] Ibid., p.520.

[141] Ibid., p.539.

does not resist translation into contemporary language and, what is more, the case does not force us to accept conceptual relativism. Rather, Kitcher claims, the case show the opposite:

> I am suggesting a rival picture is which the connections of terms to the world are often extended in subsequent uses. This picture appears to accord better with the continued reapplication and redefinition which is typical of scientific usage, and of which the case of Priestly furnishes a striking example.[142]

Also unsatisfied with Davidson's assurances that we can always translate any alien language, Kitcher points out that Davidson fails to show what is especially difficult about reconstructing the languages used by past scientists, and how the difficulties can be overcome. Kitcher proposes that

> We should abandon a traditional assumption of the philosophy of science, the assumption that we can reconstruct the language of a theorist by reconstructing the language of his theory. Instead, we should recognize that scientific expressions are associated with a complex apparatus---their reference potential---which changes as science develops. Claims about the development of scientific concepts are, I suggest, best understood as claims concerning the changes of reference potentials.[143]

In 1981, Hilary Putnam published his book, *Reason, Truth and History* in which he develops much the same criticism towards Kuhn's notion of incommensurability.[144] For Putnam, the tendency

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[142] Ibid., p.539.

[143] Ibid., p.547.

[144] Hilary Putnam, *Reason, Truth and History*, Cambridge University Press, 1981. The part concerning the topic here is in reference to pp.113-124.

represented by Kuhn, Feyerabend and Foucault is irrationalistic and relativistic. The central notion in this tendency is the product of Kuhn's 'extreme relativism', the thesis of incommensurability, to which Putnam defines as follows:

> The incommensurability thesis is the thesis that terms used in another culture ... cannot be equated in meaning or reference with any terms or expressions *we* possess.[145]

If this thesis were true, Putnam argues we could not translate other language -- or even past stages of our own language -- at all. For Putnam, given Kuhn's 'transcendental argument' of incommensurability, it is only an illusion that all possible translation schemes will fail to capture the 'real' sense or reference of the translated. That is, different sets of beliefs, or different conceptions, do not prove the impossibility of ever translating anyone 'really correctly'. As Putnam puts the point:

> We are committed by our fundamental conceptions to treating not just our present time-slices, but also our past selves, our ancestors, and members of other cultures past and present, as *persons*; and that means, I have argued, attributing to them shared references and shared concepts, however different the *conceptions* that we also attribute. ... However different our images of knowledge and conceptions and beliefs about what is reasonable with even the most bizarre culture we can succeed in interpreting at all.[146]

Putnam thus suggests that Kuhn ought to concede commensurability, and translatability, and thereby to restrict the incommensurability

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[145] Ibid., p.114.

[146] Ibid., p.119.

thesis to the extent of theoretical vocabulary (although Putnam admits that we interpret discourse always as a whole).

As usual, Kuhn took years to digest these philosophical criticisms of 'conceptual relativism' from the mainstream philosophers and to report his development on the issues they raised. Twenty years after SSR first appeared, twelve years after his three attempts to save the notion of a paradigm, and five years after his preface to ET, Kuhn reported another his drastic shift from the notion of incommensurability at *The Biennial Meeting of the Philosophy of Science Association* in 1982.[147]

The term of "incommensurability", borrowed from mathematics, Kuhn recalls, was first used in print both by Feyerabend and himself in 1962 to describe the relationship between successive scientific theories. "Each of us was led to it by problems we had encountered in interpreting scientific texts."(PSA, p.669) But now, Kuhn complains, most or all discussions of incommensurability have depended upon the overly interpreted assumption that, if two theories are incommensurable, they must be stated in mutually untranslatable languages. Retaining the original sense of incommensurability to the view that there is no common measure, on the one hand, Kuhn argues that lack of the measure by no means makes comparison impossible. He has converted the usage of 'no

[147] The shift was reported in his paper, "Commensurability, Comparability, Communicability", CCC, which was read at the Meeting on the Sunday, October 31, 1982. Later, this essay was collected into the association's annual proceedings, *PSA 1982*, Volume 2, pp.669-688.

common measure' to 'no common language', on the other hand. Kuhn considers:

> The claim that two theories are incommensurable is then the claim that there is no language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be translated without residue or loss.(PSA, p.670)

In terms of this revised version of the incommensurability thesis, Kuhn now holds that his claim of the notion is actually much more modest than what many of his critics have supposed. In CCC, he defines the modest version of incommensurability as 'local incommensurability' which, he says, was his 'original' intention of the perspective. But Davidson, Kitcher and Putnam misinterpret, Kuhn complains, his "local incommensurability" which is even incompatible with its success in the aspect that it describes the outcome of interpretation of an older theory as a translation or a translation schema. For their illusion is only derived, Kuhn criticizes with a different tone from his pro-Quine commitment, from Quine's equation to interpretation with translation:

> That equation is traceable at least to Quine's *Word and Object*. I believe it is wrong and that the mistake is important. My claim is that interpretation,... is not the same as translation.(PSA, p.672)

Probably with the strength of his commitment to hermeneutics since 1977, Kuhn further complains that recent analytic philosophy has concentrated exclusively on 'translation' and ignored the equally significant process of 'interpretation', the business of which is virtually practised by historians and anthropologists.

Therefore, unlike the translator, they may initially command only a single language or like Quine's "radical translator" seeks to learn a new language whether or not it can be translated into the one the interpreter has acquired. But, the Quine translation manual, i.e., the availability of learning a new term other than English by interpreting an utterance, Kuhn argues, does not mean translation since it may not do with a term which is known to its working language. Acquiring a new language is not the same a translating form it into one's own.

Even Kitcher's general theory of reference cannot, Kuhn further argues, shed light on the issue of translating phlogiston theory into modern language and thereby bring talk of incommensurability to a close. For old theories or texts are structures that must be evaluated as wholes. Thus use of a single word 'phlogiston' is only one of the ways by which the original text communicated the beliefs of its author. Therefore, substituting unrelated or differently related expressions for those related, Kuhn holds, sometimes identical terms of the original must at least suppress those beliefs leaving the text that results incoherent. In other words, it is apparent, Kuhn remarks, that a small group of terms remains for which the modern chemical vocabulary offers no equivalent, and that some have vanished from the language of chemistry entirely. That is, Kuhn believes that

> among the phrases which describe how the referents of the term 'phlogiston' are picked out are a number that include other untranslatable terms like 'principle' and 'element'. Together with 'phlogiston', they constitute an interrelated or interdefined set that must be acquired

> together, as a whole, before any of them can be used, applied to natural phenomena. Only after they have been thus acquired can one recognize eighteenth-century chemistry for it was, a discipline that differed from its twentieth-century successor not simply in what it had to say about individual substances and processes but in the way it structured and parcelled out a large part of the chemical world. (PSA, p.676)

And this, says Kuhn, is the untranslatable part of the phlogiston theory that the historian has had to discover or invent meanings for in order to render intelligible the texts on which he works. In other words, this is the process of interpretation, a process that has been, Kuhn argues, much discussed recently under the rubric hermeneutics. In the hermeneutic process of understanding old texts, Kuhn claims, the question of translation simply does not arise.

Also, Kuhn challenges Quine's idea of a *translation manual*:

> Quine discards the intentional constraints on adequate translation. ... by giving them up, Quine eliminates the very possibility of interpretation, and interpretation is, as I argued at the start, what his radical translator must do before translation can begin. (PSA, pp.680-681)

For Kuhn, Frege's two distinguished semantic functions of linguistic expressions, the dual functions of expressing a sense and of referring, ought to be both preserved while talking about translation. So, further defying Kitcher's position that the benefits of doing as much semantics as possible is better within the theory of reference, Kuhn believes that it is better to confront directly the deep duality in the concept of meaning, both of reference and sense or intention rather than preferring only one. That is, in matching terms with their referents, one may legitimately make use of anything one knows or believes, or different senses and intentions, about those referents. Thus different people may, Kuhn remarks, use different criteria in identifying the referents to pick out terms related to them. For Kuhn, none of the criteria used in reference determination are merely conventional, associated simply by definition with the terms they help to characterize.

But given the fact that a language community is adapted to the social and natural world in which it lives, and that world does not present the sorts of situations which would lead them to make different identifications, the individual in the language community must, says Kuhn,

> associate each individual term with a set of criteria sufficient to distinguish its referents from other sorts of objects or situations which the community's world actually presents, though not from still other objects that are merely imaginable.(PSA, p.682)

Recalling the Johnny case of STP, Kuhn insists that few referring terms or expressions are learned as Kitcher envisages in isolation either from the world or from each other. In this, Kuhn highlights two themes to further defend his position of local incommensurability:

1. The holistic element, the essential role of sets of terms that must be learned together by those raised inside a culture, scientific or other, and that foreigners encountering the culture must consider together during interpretation. Under these

circumstances, Kuhn claims some sort of local holism must be an essential feature of language.

2. The lexical network of a referring term. Kuhn takes this as a node in a lexical network from which radiate labels for the criteria that he or she uses in identifying the referents of the nodal term in the network.

> These criteria will tie some terms together and distance them from others, thus building a multi-dimensional structure within the lexicon. That structure mirrors aspects of the structure of the world which the lexicon can be used to describe, and it simultaneously limits the phenomena that can be described with the lexicon's aid. If anomalous phenomena nevertheless arise, their description (perhaps even their recognition) will require altering some part of the language, changing the previously constitutive linkage between terms. (PSA, p.683)

From this second theme, Kuhn draws that what a language community shares is homology of lexical structure. That is, although their criteria need not be the same, their taxonomic structures must match. Otherwise communication ceases until one party acquires the language of the other, Kuhn argues, since where structure is different, the world is different, language is private.

Here, appears Kuhn's new stand of the invariants of translation. For him,

> Translation is, of course, only the first resort of those who seek comprehension. Communication can be established in its absence. (PSA, p.683)

And this is the routine business engaged in every day, Kuhn points out, by historians, anthropologists, and perhaps small children. Of course, the lack of this hermeneutic insights, Kuhn assumes, requires the philosophical circle represented by those such as Davidson, Kitcher and Putnam to methodologically comprehend the Continental hermeneutics an approach of understanding, not only of translation and its limitations.

As a commentator of Kuhn's contribution to the PSA meeting, Kitcher responds to Kuhn's arguments in his paper called "Implications of Incommensurability".[148] For Kitcher, if the current Kuhn is right about "conceptual change" in science, then some of the claims he made in 1962 and 1970 are incorrect, and thus the Kuhnian discussion of scientific revolutions is far less radical than it initially appeared. Kitcher consequently continues defending the antecedent of his conditional thesis that Kuhn perhaps is right in claiming that the conceptual incommensurability, the phenomena that occurs in the history of science, is best understood by recognizing how different *languages* divide the same world differently.

But, oxygen theorists still can preface their discussion, Kitcher insists, with glosses that identify the reference potentials. In this, Kitcher continues holding that full communication across the revolutionary divide may be achieved. For he believes that there are no thoughts which lie too deep for

[148] P. Kitcher, "Implications of Incommensurability", in *PSA 1982*, Volume 2, The Philosophy of Science Association, 1983, pp.689-703.

words.

The other commentator, Mary Hesse, sympathetically sets Kuhn's approach against an ideological background of contemporary history of philosophy of science. That is, Hesse emphasises that the problem situation of Kuhn's and Feyerabend's discovery of "meaning-change" or "incommensurability", although it was considered "radical" in 1960s, is still essentially the same. For the ideal translation of current analytic philosophers still presupposes that most terms in a scientific theory retain their meanings through theory changes, and hence that ideal translation is possible. But, the problem of translation remains since "there is still no clear theory of meaning which recognizes the fact in natural languages and in scientific theory we cannot assume univocality of meaning or strict applicability of deductive logic."(PSA, p.704)

Being not quite content with Kuhn's new efforts, Hesse regards it strange when Kuhn rejects the concept of "meaning" as a useless irrelevance. For Hesse, Kuhn's approach is to drop the notion of *change of meaning*, which seems to be continuous and therefore unmanageable in contexts of interpretation, and to replace it by the notion of changing taxonomist. Hesse thus asks:

> What in the end is a theory of meaning for? So far it has had roughly three areas of application: philosophical logic, scientific linguistics, and philosophy of science. It has been shown to be bankrupt in philosophy of science, not least by Kuhn's own work; it is rapidly losing credibility as an explication of meaning in natural language; there remains logic, which no doubt can look after itself. But with regard to meaning in science and natural language, we need a theory that takes it as axiomatic that meaning is strongly holistic, and that it changes by however little with every change of context of

> application. This is going to involve a new start for the theory of meaning.(PSA, p.710)

Once again, Kuhn's response to commentaries was centred on complaint of misunderstanding. Kuhn insists that Kitcher does not catch his view that "full communication" is perhaps possible only if one of the two learns the other's language, becoming, in that sense, a participant in the other's practice of chemistry. What is really at issue, Kuhn repeats, is not significant comparability but rather the shaping of cognition by language. Likewise, Kuhn holds that Hesse and he in fact share more than she supposes. However, Hesse misses the thrust of his argument, Kuhn complains, when she asserts that his remarks about homologous taxonomies are not directed towards a theory of meaning.

However, Kuhn admits that it raises a central and crucial issue in that, Hesse holds that his position of shared taxonomy is too strong while Kitcher views that his notion of incommensurability is too common to be a criterion of revolutionary change (and hence that he is no longer capable to distinguish sharply between normal and revolutionary development in science). On the one hand, Kuhn sees the force of those positions and proclaims:

> My view of revolutionary change has increasingly moderated as Kitcher supposes.(PSA, p.714)

But on the other hand, Kuhn thinks that what Kitcher and Hesse urge regarding continuity of change is too far beyond what he stands to be accepted. Because, the concept of scientific revolution is originally derived from his discovery of the hermeneutic experience in 1947, and to understand any part of a past science, the historian must, says Kuhn, first learn the language in which that past was written. In this, Kuhn still believes that any attempt to translate the past into a later language is bound to fail. In light of this hermeneutic understanding of a past science, Kuhn points out of importance of enlightenment, "the sudden recognition",

> the language learning process is therefore interpretive and hermeneutic. Since success in interpretation is generally achieved in large chunks ("breaking into the hermeneutic circle"), the historian's discovery of the past repeatedly involves the sudden recognition of new patterns or gestalts. It follows that the historian, at least, does experience revolutions. Those theses were at the heart of my original position, and on them I would still insist.(PSA, p.715)

But given this mix of hermeneutic and linguistic explanations, Kuhn declares his position of 1982 in such a distinct statement:

> If I were now rewriting *The Structure of Scientific Revolutions*, I would emphasize language change more and the normal/revolutionary distinctions less.(PSA, p.715)

In practice, as a matter of fact, this strategy was already adopted in his 1978 book, *Black-Body Theory and the Quantum Discontinuity 1894-1912*.[149] In the Preface to this book, Kuhn suppressed all the philosophical terms of SSR, including 'incommensurability', 'paradigm', 'normal science' and 'scientific revolutions', and so on. Methodologically, nothing of philosophy of

[149] Thomas Kuhn, *Black-Body Theory and the Quantum Discontinuity 1894-1912*, Clarendon Press: Oxford University Press, 1978.

science has been mentioned other than 'the evolution of the so-called old quantum theory' and his reading program to find 'the central conceptual novelties'. Without any philosophical representation, Kuhn simply says:

> Only after studying the extended treatment of Planck's theory in the *Lectures* of 1906 was I quite able to believe that I was now reading his first quantum papers correctly.[150]

It is surprising that Kuhn has become so wary of being caught out by philosophers that he does not even mention how he has managed to interpret, or to translate, Planck's notions. Without being bothered by his notions such as incommensurability and revolutions, Kuhn has become a practical reporter.

It seems to me that Kuhn was somewhat scared even by his own philosophical approach around 1962 and only tends to localize his approach to history of science in terms of a largely reduced angle, that is, one of how to read an ancient scientific text. Localization of his whole approach into reading old texts, therefore, localizes his focus of philosophical commitment into traditional hermeneutics in its religious usage, the one concerned with finding and interpreting the truth in theologian texts. No wonder that, up to 1982, there is no any notion of SSR left except his special operation on the phenomenon of incommensurability, a notion now called 'local incommensurability', a product of his

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[150] Ibid., p.viii.

'local linguistic holism'.[151] For Kuhn assumes that this position was practically supported by his discovery in 1947 when reading Aristotle's *Physics*, which provides him with certainty of comprehending hermeneutics.

[151] Also compare Kuhn's 1983 paper, "Rationality and Theory Choice", *The Journal of Philosophy*, Vol. LXXX, No.10, October, 1983, pp.563-570. This paper was presented in an APA symposium on the philosophy of C. G. Hempel, December 28, 1983.

8

A Summary of the Development of Kuhn's Thought

Quite different from most of the philosophers who criticized him, who were well trained in philosophical reasoning, Kuhn's reflection of science stemmed mainly from his personal experiences of doing science and its history. Among them, it was his enlightenment in the summer of 1947 that fiducially altered his view of history and philosophy of science. Only can this explain why while Kuhn painfully abandoned many of his most thoughtful and provoking notions in SSR, he never gives up the hope to defend the notion of incommensurability, which once settled his whole academic orientation.

Kuhn's enlightenment in 1947 was, of course, not only the upshot of his personal interest. Without Conant's ambitious innovation plan of Harvard curricular system and his effort to usher Kuhn into the field of history of science, the latter would never have made contemporary philosophy of science so different. Second, the Harvard academic atmosphere in 1940s, especially the one created by the brilliant Nobelist and operationist, Bridgman, and famous physicist, Frank, in the department of physics, also influenced the development of Kuhn's thought.[152] Third, it was at the historical moment that the academic world in the West had become increasingly dissatisfied with the image of logical positivism while realizing the force of history and practice in the cognitive processing. Fourth, the combat against logical positivism, which was largely pioneered by the Harvard philosopher, W. V. O. Quine, philosophically briefed the young Kuhn, who fortunately had three years as a Junior Fellow of the Society of

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[152] It is striking, people would ask, that I do not take the influence of Kuhn's mentor, J. Conant, seriously. As a matter of fact, I had the same question when I wrote the thesis. Given that I have correspondence with Kuhn for a while, I wrote a letter asking him about the question. From Kuhn's reply (cf. the appendix 3 at the end of the thesis), I realize that Kuhn has an answer for it: "My relation with Dr. Conant were close: he introduced me to history of science, and I did my initial work in the field for the course he offered in the General Education program. I rapidly came to disagree with certain aspects of his philosophical position, but his role in my development has been crucial". And the disagreement was of Conant's view of scientific development, a view concurred G. Sarton's conviction of accumulative scientific knowledge (cf. Sarton's *A Guide to the History of Science*, 1952, pp.11-12).

Fellows of Harvard University, to be prepared to see history of science with insights that the analytic cannot be separated from the synthetic. Last, but not the least, it was the influence of contemporary European thinkers such as Koyré, Fleck, Wittgenstein, Piaget, Polanyi, Gestalt psychologists, etc.[153]

This period, for Kuhn, was a process from epistemological crisis to entry into the academic establishment. By contrast, however, the second period of the development of Kuhn's thought, those transformation years after 1962, was a painful experience for him. Gradually revising his old convictions step by step, Kuhn in this period encountered seven waves of criticism, which can be roughly divided into three stages:

The first stage: criticism of the whole of SSR

1) the first wave of criticism was at the British Conference held at Worcester College, Oxford, in the summer of 1961 when Kuhn first unveiled his heretic approach in a paper titled "The Function of Dogma in Scientific Research";

2) the second wave was represented by those book reviews after

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[153] Many readers of SSR get the strong sense that they are reading a work that was composed as response to Duhem, but this resemblance of 'holistic approach' in Kuhn's work, I conclude, is largely because of the influence of Quine rather than directly from Duhem. Kuhn mentions in quite a few occasions that not only he had chances to talk to Quine during his fellowship at Harvard, but also Quine gave him some feedback to his draft of first several chapters of SSR in 1958-59 when they both were at the Centre for Advanced Studies in the Behavioral Sciences at Stanford. Also, during the process of Kuhn's preparation of CR and SSR, I hardly notice that he was influenced by Duhem through Conant. The reason seems the same as I mentioned above. For Duhem has the conviction of respecting continuity in the historical development of science (cf. *To Save the Phenomena*, p.117).

the appearance of SSR, especially Shapere's;

3) the third, and the most severe, was those commentaries fired out at the London Colloquium, Bedford College, in the summer of 1965 after Kuhn delivered his keynote speech, "Logic of Discovery or Psychology of Research";

The second stage: criticism of the notion of paradigm

4) the fourth wave of criticism was conducted at the Illinois Symposium on *The Structure of Scientific Theories*, held at the University of Illinois in 1969, in which Kuhn read his first paper of the year, "Second Thoughts on Paradigms";

5) the fifth was from Lakatos and Feyerabend in the reconstructed collection, *Criticism and the Growth of Knowledge*, to which Kuhn replied in a second paper in 1969, "Reflections on my Critics".

(The third paper in that year, PS, was only a balanced summary of the above two towards those criticisms.)

The third stage: criticism of the concept of incommensurability

6) the sixth wave of criticism was inaugurated by Shapere's 1971 review, "The Paradigm Concept", which questioned why Kuhn still retains the last troublesome residual in his entire approach, the notion of incommensurability;

7) the seventh wave, actually a historic campaign from mainstream American philosophy, was marshalled by Davidson in 1970-1974, and later joined by philosophers such as Kitcher and Putnam, a line that aims to suppress Kuhn's 1962 approach by deconstructing

the legitimacy of Kuhn's primary concern with the philosophical topic of the century, conceptual change, and finally of the notion of incommensurability.

In the first of these stages, Kuhn was mainly concerned to further clarify his stance in SSR with minor and mostly surreptitious corrections. For example, he avoided the usage of "dogma" to describe normal science, reinforced the elements of sociology and psychology in philosophy of science, and redefined the notion of paradigm. However, he was still confident, at this stage, due to the overwhelming success of SSR, and those positive commentaries, especially the one by Masterman, which came from various fields and countries of academic world. That is, Kuhn hoped that by further revealing the analyticity of the notion of a paradigm, he might effect a reconciliation with mainstream philosophers on their charges of relativism, irrationalism, dogmatism etc. In short, he wanted to establish a way of communication to settle the real incommensurable phenomenon he theoretically insisted.

But at the second stage, which was largely encompassed by his three attempts in 1969, Kuhn lost his edge by reducing paradigms to exemplars which in turn led him shift to the areas of sociology and psychology of science. His reduction of the function of a paradigm and related approach to computer programming in fact put him into another problematic situation, his critics argued, of relativism and irrationalism. For (1) given the explanation that paradigm is only connected with similarities between examples, what is the obstacle to conduct the communication between two different scientific communities? And (2) if paradigm can be computerized to the extend that nothing cannot be analyzed, where is the reason for Kuhn's fiduciary notion of incommensurability?

It was Kuhn's compromise "partial communication", that unveiled the third stage which shows that Kuhn has been pushed into a corner by analytical philosophers. Very much like the strategy in the second stage, Kuhn gave in on the problem of incommensurability which, he thought, would be easily solved with recourse to Quine's radical translation manual. However, this was immediately challenged by Davidson who later developed his known thesis of the third dogma of empiricism in order to finally overthrow Kuhn's 1962 view. Under the attacks along this line, especially those of Kitcher and Putnam, Kuhn was squeezed totally out of his original intention of incommensurability and forced to withdraw to his 1947 position, a hermeneutic way of reading old texts of science, on which he once built up the magnificent structure of scientific revolutions.

To the mainstream philosophers, Kuhn's approach was viewed as having four main flaws: neo-positivism, irrationalism, idealism, and sociologism.[154] In a word, as Suppe charges,

Kuhn's position commits him to a metaphysical and

[154] Cf. F. Suppe (ed.) *The Structure of Scientific Theories* (second edition), University of Illinois Press, 1977, pp.647-648.

> epistemological view of science which is fundamentally defective since it makes discovering how the world really is irrelevant to scientific knowledge, reducing scientific knowledge to the collective beliefs of members of scientific disciplines.(SST, p.648)

So, Suppe concludes:

> Collectively these factors [those four flaws] have led increasing numbers of philosophers of science to reject Kuhn's approach as irredeemably flawed, although not as hopeless as Feyerabend's.(SST, p.648)

In a nutshell, Suppe is certain that Kuhn's views have undergone a sharply declining influence on contemporary philosophy of science since 1969. Instead, philosophy of science today is, Suppe envisages, seeking an adequate understanding of science, of the activity of scientific theorizing, or the nature of the knowledge resulting from such activities in new directions. These directions, Suppe insists, require that a premium be placed on the role of rationality in the growth of scientific knowledge, and that one be so placed in large part in response to its absence in positivistic philosophy of science and its near absence in the work of Kuhn and Feyerabend. And these new directions lead, Suppe hopes, to the position that it is their central aim to come to knowledge of how the world really is. A new correspondence theory between theories and reality, Suppe predicts, will be a central aim of science as an epistemic enterprise in sharp repudiation of Kuhn's "sociological" views of knowledge. In other words, Suppe emphasizes that contemporary philosophy of science ought to be, legitimately, philosophy of *science*, "a discipline concerned with science as actually practised yet at the same time doing philosophy" (SST, p.650) In this, his preference is in the direction of historical realism.

But Suppe was not clear, it seems to me, in the aspect of why science should deal with philosophy while it is practising its trade successfully. Also he did not, of course, answer why science should be ***ONLY*** dealt with in philosophy of science rather than combine any other branches of sciences, for example, sociology, or psychology of science etc. As long as Suppe cannot deliver legitimate 'rationality' to rationalize his stance, it seems to me, Kuhn's approach still cannot be ruled out of philosophy, or at least of the so-called philosophy of science.

In the following chapters, I plan to defend Kuhn's position in 1962. That is, I intend to argue that the rational ground of the notion of incommensurability not only exists, but also exists so commonly to the extent that philosophers of science would do well if they pay close attention to the existence of incommensurability. It would be fortunate, in other words, if they could limit their ambition to ask science only do with analytical philosophy without any pertinent reasons. I shall further argue, if the notion of incommensurability stands, Kuhn's original notion of paradigm also stands albeit he has, unfortunately, abandoned it in its insightful sense in 1962. For me, they are, I believe, twin notions to such a extent that we must either retain or jettison both of them. Their relation is, I shall convince readers, like the one between philosophical mind of a philosopher and his or her physical body.

PART II

Reflection on Kuhn's Changing Views

Preamble

Unfortunately, according to what we have discussed in Part One, Kuhn has not realized that his experience of enlightenment in 1947 intrinsically explains his discovery of incommensurability. Also he has not yet, up to now, apprehended that understanding of incommensurability naturally presupposes the legitimacy as well as the phenomenon of paradigm. Of course, these views, which I take as themes in the dissertation, need detailed clarification. In Part Two, consequently, I shall specify these views with quite a different approach from what I have contrived in Part One.

Methodologically, in Part Two, I shall mainly concentrate on establishing the concept of incommensurability with the help of case studies in Part One. I presume that once the perspective of incommensurability is clarified and defined, Kuhn's notion of paradigm in 1962 need not be treated as a product of irrationalism.

On the other hand, I shall reveal that the relationship between incommensurability and paradigm can, in fact, provide deeper ground of understanding the hermeneutic circle, one that Gadamer has not quite seen. In all this, however, the establishment of the notion of incommensurability is in the centre of the thesis. In other words, this approach is contrived not in the sense to reconstruct Kuhn's "notorious" notion of paradigm in the philosophical arena, but in the significance of the notion of incommensurability to shed more light on the bridge between cultures of the Anglo-American, the European as well as the Oriental etc.[155]

In particular, in establishing the concept of incommensurability, I shall essentially exhaust the cases I discussed in Part One, although other cases shall be additionally brought into discussion when we gradually enlarge the view of incommensurability. This is a strategy related to Kuhn's exemplar technique, which, I envisage, is much more solid than the merely language analysis. I hold that as long as one case of incommensurability is established, the mainstream philosophers ought to accordingly moderate their position to notice the edge of the notions of incommensurability and paradigm, the latter of which has been profoundly accepted by many intellectuals outside analytical philosophical arena.

In the concrete, I shall first embark on Kuhn's personal case

[155] Given my background as a Chinese, for example, my view is in certain effort to understand alien cultures, and, of course, it at the same time reflects my edification of Chinese culture and its philosophy.

of enlightenment in the summer of 1947 when he was reading Aristotle's *Physics*. The reason I commence with this personal experience is in that philosophers, however, prefer citing cases or examples in history, but few of them had ever encountered the process of thinking that Kuhn personally experienced. In other words, I insist, although they might not take Kuhn's personal experience seriously, personal experience of this kind cannot be ruled out of the scope of cases or examples they cite.

Probably no one can deny that the construction of human knowledge is based on personal operations or experiences and, what is more, that no epistemology of science can be convincing without sufficient provision of the genuine way of our everyday thinking. In this connection, the debate concerning the notion of incommensurability is not of how to analyze out personal experience from epistemology, but of whether an approach lacks cases concerning personal experiences of everyday thinking, and of whether they are rich enough to account for the cognitive phenomena in our everyday thinking. In view of this, I believe that epistemology could do better by principally revisiting cases such as Kuhn's personal enlightenment in 1947 and the debate of the conception of incommensurability. With this angle, I hope, we can enlarge the issue of incommensurability to the extent that we can see why Kuhn's approach to history and philosophy of science is significant and accountable concerning the development of contemporary epistemology.

9

History, Hermeneutics, and Enlightenment

---*Incommensurability Revisited I*

In 1947, having devoted every effort to understanding Aristotle's mechanics and having failed to appreciate its strength in relation to the work of Galileo, Kuhn was perplexed of why Aristotle was so wrong in mechanics while he was so acute in fields such as biology or political behaviour. In a painful commitment to seek the inner coherence of Aristotle's entire system, one hot summer day in 1947, Kuhn's mental crisis was disenchanted through a sudden enlightenment, a real mental transposition: Aristotle's subject had been "*change-of-quality*" in general, including both the fall of a stone and the growth of a child to adulthood. That is why in Aristotle's physics, Kuhn realized, the subject that was to become mechanics was at best a still-not-quite-isolable special case of those changes.

This discovery is the great event named by Kuhn himself as his "first scientific revolution", the nature of which is, Kuhn states, a "recapturing out-of-date ways of reading out-of-date texts."(ET, p.xiii) This chapter will focus on the epistemological significance of Kuhn's enlightenment in 1947 and related issues of his discovery of incommensurability.

Ten years later, Kuhn expounded his 1947 enlightenment as a new way to view the history of science:

> Aristotle's day is not our day, and a real mental transposition is therefore necessary in approaching his writings, particularly those dealing with physics and cosmology. Failure to make this transposition has resulted in some strained and distorted explanations of the endurance of Aristotelian physics in antiquity and during the Middle Ages.(CR, p.94)

In other words, failure to make the transposition or to overcome the "incompatibility", a term Kuhn preferred at that time, is due to the fact that we take many of the concepts in which we believe too easily as natural and indubitable products of our own unaided perceptions. So, dismissing concepts different from our own as errors is rooted in ignorance or stubbornness and perpetuated by blind obedience to authority.

In 1977, Kuhn in the preface to ET reinterpreted the event as an example of contemporary hermeneutics which eventually made him view history of science differently. In terms of hermeneutics, Kuhn claims that when reading an out-of-date text, one has to discover one's own conceptual scheme as well as its intellectual history. It is not a way of reading by supplementing knowledge with piecemeal correction of mistakes. Rather, Kuhn insists, text itself has its consistency in history to be laid out and thereby to be justified in its own context and system.

In 1982, Kuhn still related the notion of incommensurability to "interpreting scientific texts" in terms of hermeneutics. But he had drastically withdrawn from his stance of the notion in SSR to one that he called "local incommensurability". Kuhn declares that this "local incommensurability" is as a matter of fact his original conception of it. With this version of incommensurability, Kuhn claims that, on the one hand, "local holism" must be an essential feature of a language, and on the other, different languages impose different structures on the world.[156]

But, whether Kuhn's definition of local incommensurability is sound and acceptable or not, it is true that it is by no means the version of it either in CR or in SSR and in ET. Further analysis, therefore, is needed to trace back to Kuhn's distorted demonstration of his personal experience in 1947 and, of course, to his interpretation of it in 1960s.

In fact, Kuhn's distortion of incommensurability in 1982 not

[156] Cf. *PSA 1982*, Vol. 2, pp.670-671; pp.681-683.

only misshapes his personal experience in 1947, but also misrepresents the perspective of contemporary hermeneutics and historical approach of science in many ways.

It is certain that Kuhn's understanding of Aristotle's *Physics* in his 1957 book, CR, was not at all in the sense of "local holism", which Kuhn defines as "the essential role of sets of terms that must be learned together by those raised inside a culture, scientific or other, and which foreigners encountering that culture must consider together during interpretation." (PSA, p.682) However, Kuhn's experience of enlightenment in 1947 was in nature a process of sudden understanding of, as Gadamer notes, "the effective-historical consciousness" in an out-of-date text, Aristotle's *Physics*. In terms of this, the meaning [the effective-historical consciousness] of sets of terms in the out-of-date text or this kind could not be *learned* together either by those raised inside a culture or by foreigner's encountering that culture. For the text is, in other words, a *Dasein* in remote history which is out of our linguistic reach, even if with recourse to learning the language of that text. Thus the finitude and historicity of the text cannot be at any rate covered by Kuhn's later invention of 'local holism' in the present. This is *the* problem of Kuhn's 'local holism'. (Although it can be one of the essential features to explain why we need to learn a specific language of an ancient text.)

Distorting his 1947 perspective to such a degree, Kuhn has completely lost his fundamental insight and original strength about history and historicity of an out-of-date text. His approach in

1982 can be thus questioned: What is a historical research without a "historical question" except language question?[157] Putting the cart before the horse, Kuhn misinterprets the historicity of a text, or "the effective historical consciousness" of it, in terms of his 1980 distortion, i.e., an interpretation of the text with a local language in the present. No wonder Kuhn proclaimed in 1982 that "if I were now rewriting *The Structure of Scientific Revolutions*, I would emphasize language change more and the normal/revolutionary distinction less."(PSA, p.715)

In this, I consider that Kuhn's historical research in 1982 has become one without historical questions. Thus the research cannot be, in any sense, consistent with European hermeneutics to which he seeks to have recourse in 1982. It has less connection with hermeneutics not only due to that Kuhn's local holism lacks of essential elements of history, but due to that Kuhn has shifted his concern to a theme that different languages impose different structure on the world and its related assertion that the structure mirrors aspects of the structure of the world.[158] Local incommensurability of this kind can thus be anything but hermeneutics. For one thing, Kuhn misinterprets the proposition of contemporary hermeneutics.[159]

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[157] Gadamer once asks philosophers the sort of question. Cf. Gadamer's "Foreword" to the second German edition of *Truth and Method (After Philosophy*, The MIT Press, 1987, p.343).

[158] Cf. *PSA 1982*, Vol. 2, pp.681-683.

[159] For the contemporary representative of the European hermeneutic tradition insists that the sentence "Being that can be understood is language" merely implies: It does not claim any

Moreover, if in terms of the hermeneutic circle, understanding as a process of the to-and-fro movement between partial understanding and the 'sense of the whole', Kuhn's approach is better deployed in CR than his later explanation in 1982.[160] In consequence, without the sense of history and historicity, i.e. without awareness of Gadamer's concept of 'effective-historical consciousness',[161] Kuhn's so-called 'hermeneutic' defence of incommensurability only leaves more questions to his local holism. For Kuhn ignores the key point that 'experience' in history plays a role in hermeneutic investigation of history which language analysis cannot. In this, although on this issue he would have much to render, but he missed many chances to capture the spirit of hermeneutics which was deeply implied in his 1947 enlightenment.

Kuhn's personal experience in 1947 was so rich that it implies a significant issue in epistemology which even Gadamer's hermeneutics, as well as contemporary philosophers, never touched. It is the event itself, Kuhn's epistemological enlightenment

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absolute means, on the contrary, that being is not experienced where something can be made by us and is conceived by us to that extent; being is experienced, rather, where what is happening can only be understood (cf.ibid., p.348).

[160] We shall discuss Kuhn's distortion at this aspect on language/mirror distinction in the next chapter. But I might remark here that Kuhn's 'hermeneutic approach' in 1982 probably has nothing to do with contemporary hermeneutics to which he seems not yet to have enough knowledge.

[161] That is, I detect, it is Kuhn's view in 1982 which has no acknowledgement of the relativity of how to know the meaning of a text in history.

happened in a 1947 summer day, that requires new interpretation to the process of our cognition and of our knowing the world. It is such an event of epistemology:

> those perplexities suddenly vanished. I all at once perceived the connected rudiments of an alternate way of reading the text with which I had been struggling. (ET, p.xi)

Apparently, in contemporary western philosophy this type of experience concerning mental enlightenment has been rarely accorded much attention than those themes of 'local holism' Kuhn defends in CCC.[162] But, in my opinion, it is the experience that possesses the potential to challenge those charges of relativism, irrationalism and psychologism to Kuhn's approach in 1960s. It can challenge the mainstream philosophy to such an extent that it requests correction of the empirical basis of those charges. That is, given the fact that the contemporary philosophy of science is so absorbed into analyzing stable structures and languages of sciences, few notice that the consequence of Kuhn's enlightenment is a philosophical request to know the real process of reasoning of scientific knowledge in its transformation period from nothing to being. In terms of this, I consider, it will be of great importance to analyze Kuhn's enlightenment in detail in order to further disclose the nature of the current philosophical debate on incommensurability and its related issues.

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[162] This is the central philosophical theme, however, in the Oriental, in particular, in the Chinese school of *Zen* philosophy which has a buddhist root in the Indian wisdom and recently flourishes in Japanese thought.

First, it is obvious that this happening of mental illumination does not come from nowhere. It is the result of Kuhn's mental struggling in order to interpret Aristotle's intension of physics. As we have detailed in Chapter Two, Kuhn's perplexities were mainly of the relationship between two periods of history: one is about the history of the origin of seventeenth century mechanics, in particular, Aristotle's mechanics; the other about the current history under the then prevailing image of logical positivism. Trained in doctrines and methods under the atmosphere of the latter, Kuhn could not detect a reasonable answer for the former. Given the latter's condition of the structure of a scientific theory, the one of coherence, Kuhn failed to see any consistence and continuity between the seventeenth century mechanics and Aristotle's *Physics* and thereby to view Aristotle's *mechanics* meaningfully. Communication breakdown for the first time in his academic life dooms the possibility for Kuhn to read Aristotle's *Physics* lest other recourse can be resorted to. Fortunately, a rational (mental) process paved a way for interpretation of the text: a sudden illumination occurs.

So, second, and related to the first point, Kuhn's epistemological crisis in 1947 was not initiated by any irrational motivations but by conflicts between two standards of rational discourse in history. It is a crisis of historical legitimacy. That is, of those standards in history, which one is more legitimate than others? The one of current philosophy of science or the one of Aristotle's system? If the current, the question appears as whether it is conceivable that Aristotle's errors in *Physics* had been so blatant. If this is the case, it cannot explain why Aristotle had been an acute and naturalistic observer other than the subject of physics. But, on the other hand, Kuhn was also wondering why Aristotle's views had been taken so seriously for so long a time by so many of his successors. In this insistence, the edge of Kuhn's epistemological crisis appears: the legitimacy of the current rationale is challenged to such a paradoxical degree that holding the rationale has led to a problem that one of its values, the standard of coherence of scientific conceptions, must be suspended.[163] That is, the legitimate standard of the current value of a scientific theory cannot be proved to be measurable when applied to an ancient system of science or theory. This painful encounter of course disturbs Kuhn to such a breadth that his belief in the current rationale is comprehensively confronted.

Then third, it is Kuhn's "good will" and commitment to detect the truth of Aristotle's *Physics* that paves the way to further transcend his mental struggle into a sudden enlightenment which Kuhn had not ever experienced: Aristotle's day is not our day.

Like Kuhn before the enlightenment, most historians of science consider that no tradition of mechanics whatsoever could have provided a foundation for the work of Galileo and his contemporaries. They simply rejected Aristotle's view and began the

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[163] Interestingly enough, somewhat the crisis becomes a fascinating proof in the context of history of what Kurt Gödel once proved in 1931 in his famous "incompleteness theorem" that for any formal system that contains arithmetic there must be true statements of the system that cannot be proved within it.

study of mechanics over again. That is, although historians of science all marvel at Aristotle's and Galileo's brilliance, few really make the effort, a "good will", to figure out why Aristotle's mechanics is so blatant when comparing it with Galileo's. However, it was the young Kuhn who surprisingly acquired the hermeneutic benefaction which Gadamer once stresses:

> one does not go about identifying the weakness of what another person says in order to prove that one is always right, but one seeks instead as far as possible to strengthen the other's viewpoint so that what the other person has to say becomes illuminating. Such an attitude seems essential to me for any understanding at all to come about.[164]

It is this special philosophical gift of understanding that makes young Kuhn see what his contemporaries cannot. And, with the help of his mental illumination, he discovered one of the most consequential principles of philosophy in the century.

Fourth, the discovery is significant in that Kuhn's breakthrough had been motivated by the current rational values. In other words, although the discovery was not a corollary of the current rationale of philosophy of science, it is interesting that when concerning the interpretation of a text in history, one of its standards, the requirement of coherence of a theory, puts the entire image of that rationale in question. That is, unlike what his later critics alleged, in fact young Kuhn had been applying rational means, judgements that a theory ought to be consistent in

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[164] D. P. Michelfelder & R. E. Palmer (ed.): *Dialogue and Deconstruction: the Gadamer-Derrida Encounter*, State University of New York Press, 1989, p.55.

its own context, in order to wave the "irrational fog" out of his epistemological crisis. In certain sense, it is the rational standard itself that helped Kuhn to dig out the truth of Aristotle's great system, i.e., the rational of its own coherence in the Hellenic.[165] With a "good will" of this kind, Kuhn committed to rationally strengthen the Aristotle system in the direction to cohere it rather than to leave the inconsistency aside. Without this rational orientation and commitment, or "good will" of rational requirement, Kuhn can never encounter the incident of mental enlightenment suddenly connecting all Aristotle's elements in such an illuminating way.

Fifth, the process of illuminating the world view of Aristotle's system was prepared by Kuhn's rational commitment but finished by his mental operation to which not much has been known in sciences, even in the research of contemporary psychology.[166] Whereas in the tradition of western philosophy,[167] the mental enlightenment, or illumination, was long before thoroughly

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[165] Given the contradiction that Aristotle in such fields as logic, biology, political science, etc. was penetrating and profound whereas his mechanics had so many apparently absurd things, Kuhn's purpose was originally to find out the historical rationale in Aristotle's system in a rational commitment.

[166] But this sort of phenomenon had been discovered and studied both in the West and the East for a long time. In the West, from Plato to Hegel, there was plenty of bibliography on illumination or enlightenment; in the East, there existed strong tradition of nyaram in India and powerful influence of Zen buddhism in China and other Asian countries as well.

[167] I am not to touch the issue in its connection to Chinese *Zen* philosophy, although it is also comprehensive, which needs a different essay to sketch its relevance to Kuhn's illumination in 1947.

scrutinized by Hegel in his central notion to the dialectic in *Phenomenology*, ***Aufhebung***. Unfortunately, this German concept itself is untranslatable into English.[168]

According to Hegel, the German verb ***aufheben*** has four senses all together: (1) to actively take something up, to pick it up, or even to seize it actively; (2) to lift or raise something up; (3) to keep or preserve something; and (4) to abolish, annul, cancel, to put an end to something.[169] Nevertheless, except the superior spirit of the Notion in terms of Hegel, what exactly young Kuhn actually experienced in his enlightenment in 1947 is a process of ***Aufhebung***:

(1) he was actively taking up Aristotle's subject of mechanics in his *Physics* and seizing the subject firmly and seriously;

(2) something hidden in the text was lifted or raised up after Kuhn's long time of painful reflection and meditation;

(3) he suddenly realized the fact that Aristotle's subject was change-of-quality in general and, consequently, discovered that there exists no universal standard for one rationale to measure all scientific theories once for all;

(4) he legitimately began to put an end to the image of science of empirical positivism that believes in the image that

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[168] Although, the perception of ***Aufhebung*** has no position in the English language, I am certain, there does exist the phenomenon of the perception such as Kuhn's enlightenment in the English world.

[169] It is Hegel who first employs the verb, ***aufheben***, and its noun, ***Aufhebung***, in all four senses at once to describe how Notion develops itself from one stage of consciousness into a succeeding one through the negation of the former.

scientific tradition emerges from accumulation.

Several years later, this epistemological revolution, Kuhn's enlightenment, successively leads to his radical conclusions about the structures of scientific revolutions, those of paradigm and incommensurability. After the enlightenment, however, Kuhn ceases to be what he mentally was, but yet lives on in a new state of rationale viewing science and its history differently.

The result of Kuhn's enlightenment, sixth, now functions like an operon, in terms of biology, to orientate as well as to gradually reconstruct different constructs into a new way of his thinking. The first report of the research is his 1957 book, *The Copernican Revolution*. Through this book, Kuhn shows that his mental reconstruction is by all means a process of rational operations, although it is largely different from what he believed before the reconstruction. Given the simple fact that the book has demonstrated, his upshot of Aristotle's system in light of *quality* is not only the product of a rational process before the enlightenment, but also functions as an axiom that thereafter rearranges Kuhn's mental system in a rational way.[170] That is, to get out of the dark of epistemological chaos, to catch the coherency of Aristotle's entire system, and accordingly, to reconstruct the way of his mentality in history, the book characterizes Kuhn's approach before and after the process of his *experience of Aufhebung*. Indeed, it would be irrational not to adjust one's view of the world according to one's new discovery

[170] Cf. CR, pp.94-98.

about it and, even worse, to stubbornly stick to one Reason without any reason.

Seventh, Kuhn's mental enlightenment, i.e. the welcome fruit of Kuhn's experience of *Aufhebung*, proves that, significantly, there exists no *Reason* but reasons in the archaeology of our knowledge. Namely, Kuhn's enlightenment concludes that rationality of our human being, our concept of the world, is never the same. It changes in various ways in history. However, this result means that in history there exists neither a mental stage of rationality-free nor one of rationality-bound. It thus shows that there has nothing wrong with the view which remarks that rationalities in human history are relative to each other. In a word, there exists the relativity of rationality in history. The fact is so apparent that the rationality of Aristotle's times was quite different from that of Galileo's, and so on so forth. So are the theories of value system or measurement to two different systems in history which are relatively incommensurable. That is, although the two systems are, of course, all rational products of science or our commitment, in order to understand them, one has to apply different standards of rationality (paradigms) to make historical texts meaningful. In other words, I hold, Kuhn's mental transposition, the process of *Aufhebung*, is by no means a proof that Kuhn has converted himself from the rational to the irrational. Quite the opposite, it establishes a process from knowing only one Reason to realizing that there exits a ladder of reasons cluttered along the history of human ideas which are incommensurable. In light of this, we can behold, to understand archaeology of science is to experience, not to refuse, "Gestalt Switch" as Kuhn had experienced, namely, the mental enlightenment.

In a nutshell, it is through this dimension that we are able to comprehend the phenomenon of historical incommensurability and its related issue of conceptual changes. Also, in terms of mental experience, to see incommensurability is not only an academic subject concerning philosophical analysis, but also a kind of living experience for philosophers to encounter in the process of understanding history. It is certain that the philosophical issue of enlightenment is not at all an issue of theoretical argumentation but of encounters in experience in one's life.[171]

From the above analysis, I feel it a shame that Kuhn never made a sound defence to clarify his experience of incommensurability after the appearance of SSR. That is, it seems that without thoroughly understanding and explanation of his own experience of the enlightenment in 1947, Kuhn distorts his experience and reduces the process of understanding history to linguistic issues locally interpreted, i.e. local obstacles of linguistically interpreting an out-of-date text in the process of communication. But, as I comprehend, Kuhn's enlightenment and its upshot, the notion of incommensurability, is never of "local

[171] In Kuhn's case, as a matter of fact, historical incommensurability provides fresh experience of the Hegelian ***Aufhebung***, a new reason and necessity for the contemporary hermeneutics, which even Gadamer has not quite seen.

holism", the first theme about local incommensurability Kuhn insisted in CCC. What is more, with the help of above analysis, I perceive that to understand an out-of-date text is seldom connected to "the essential role of sets of terms that must be learned together by those raised inside a culture."(PSA, p.682)[172]

On the one hand, it is irrefutable that language has history which in turn has different cultural stages in certain culture. As Kuhn has noticed, the cultural stage of Aristotle's times is completely disparate from the one we are experiencing. For this reason we ought to acknowledge that we are able neither to capture the sense and perception of Aristotle's culture thoroughly nor to declare that we have a good grasp of the language or terms of that times. In consequence, Kuhn's first theme in CCC is no doubt a distorted interpretation of his experience of enlightenment in 1947. Members of the same language community, as Kuhn notes, are members in a common culture, but they are not those members to be understood when reading an out-of-date text, namely not those of the language community and the culture in which the text was written.[173]

On the other hand, given that the reference of a term cannot

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[172] It is clear, Kuhn must admit, that when he discovered the paradigm of Aristotle's entire system, he himself was never raised inside the Greek culture.

[173] In other words, Frege's two semantic functions of linguistic expression, the functions of expressing a sense and of referring, cannot be fully applied to the case of understanding an out-of-date text. For neither the sense nor the reference can be successfully established between two language communities in such a historical context.

be established, as Kuhn's struggling before his enlightenment had shown, meaning of any term in an out-of-date text cannot be successfully registered in the sense of those terms we know now. Therefore, one has to be aware that understanding an out-of-date text is rather a painful process of establishing certain new relations across many points or locations between at least two different times in history. In this, it is mainly a theme of "historical holism" rather than Kuhn's 'local holism'. In terms of this, Kuhn's hermeneutics in 1982 was supposed to depict the process of understanding between locals, to be precise, not only of one *Local*. Although 'local holism' is an essential feature of a language, it is only relevant to the issue of communication in one local. While concerning different essential features of languages or meanings of them, a 'local holist' has to open his history-bound compartment to, as Gadamer holds, "lose oneself in order to find oneself".[174] The same as Kuhn himself held in 1956:

> The combination of science and intellectual history is, however, essential in approaching the plural structure of the Copernican Revolution. ... Besides, I am not convinced the two components are really distinct. ... Scientific concepts are ideas, and as such they are the subject of intellectual history.(CR, p.viii)

It is therefore the disparate locals in history, which have their own holism, that accounts for the basis of historical holism and that are essentially incommensurable. Thus it is obvious that to understand an out-of-date text is in fact to comprehend

[174] D. P. Michelfelder & R. E. Palmer (ed.): *Dialogue & Deconstruction*, State University of New York Press, 1989, p.57.

something beyond one's own "local" which, according to Kuhn's experience of enlightenment, cannot be interpreted intelligibly by learning a language in one's own local.

Last, but not least, the immediate conclusion of my above analysis is that meaning-change, or conceptual change, across various locals in history cannot be detected by those who have no interest in seeing beyond their own local culture. However, conceptual change can be enlightened through personal experience of those who have "good will" to undergo the experience of *Aufhebung*. In light of this, I argue that there is in fact no need to retreat to the 'local incommensurability' of hermeneutics as Kuhn named in 1982. For historical incommensurability occurred in understanding an old text is bound to first decode all kinds of epistemological phenomenon between different locals in history rather than those within one local. Communication of the latter has nothing to do with the archaeology of science.[175] For the issue of historical incommensurability appears only when disparate local holism encounter each other.

Therefore, in the case of Kuhn's enlightenment, it is not in the sense of typical communication but of understanding since the ancient text itself is silent. In understanding, unlike in the situation of communication, the only way to get grasp of meaning or rationale in an out-of-date text is through hermeneutic circles, i.e. the frequent private experiences to go beyond the current

[175] Another kind of incommensurable phenomenon within different locals in spontaneity shall be tackled in following chapters.

local holism. Given the fact that there is no absolute recourse to the language and its local holism of the ones in past, the process of *Aufhebung* plays an indispensable role in deciphering historical relics of science. And the process of *Aufhebung* is primarily originated in our practice of understanding the nature and it is heuristically projected into the situation of our understanding history. This process, as Aristotle once points out, involves practical wisdom, something that can only be acquired through the practical commitment to understanding different particulars.[176]

The result of *Aufhebung* also presupposes paradigms, different ways of reasoning. For all these above reasons, meaning-change across disparate locals in history cannot be interpreted in one conceptual scheme of one local holism. That is, history, even culture, has not sustained any attempt to localize a subject into the current local holism when tackling communication in history.

In light of this, I consider that Kuhn's CCC severely distorted hermeneutics he seems to have cherished and, what is more, his experience of enlightenment. It is unfortunate that he never obtains a pertinent account for the role of enlightenment phenomenon in contemporary philosophy of science.

Although the further account for the importance of enlightenment in hermeneutic approach will be linguistically dealt with in the next chapter in terms of contemporary philosophy of

[176] Cf. Aristotle's *The Nicomachean Ethics*, trans. by David Ross, Oxford University Press, 1925, pp.148-149.

language, I shall summarize this chapter to highlight my essential positions on my perspective of enlightenment:

(1) In the process to understand the meaning of an out-of-date text or scientific theory, mental enlightenment often abets to create constructs to help clear the fog in understanding the obsolete text.

(2) The mental enlightenment of this kind, as the one Kuhn once experienced in 1947, not only proves that in history there have disparate reasons, but also that incommensurability is by all means a indispensable notion when reasoning about history.

(3) Enlightenment is such an important way of overcoming the limit of one Reason that it is more of practice and experience rather than philosophical or theoretical demonstration.

(4) Enlightenment, in terms of Hegel's ***Aufhebung***, is a mental process of synthesis rather than analysis. It therefore provides new basis for the synthetic methodology that can balance the handicapped situation in contemporary philosophy.

(5) Another beneficial information from Kuhn's enlightenment is that to know the world, one has to be a practitioner who keeps a 'good will' to know, as Gadamer points out.

(6) Enlightenment attests to the fact that the logic process of Reason often breaks down after the commitment under one conceptual scheme. The experience of enlightenment emerges as a process suddenly connecting two or more different reasons together against a larger historical background. Its result may show relatively distinct historicity and incommensurability, but its

happening and development are by no means irrational.

(7) Enlightenment is an event that one has to personally experience rather than learn from teachers. So it becomes mysterious not for those experiencing enlightenment but for those interpreting it without experience.

(8) The phenomenon of enlightenment is therefore the area that now we should do more research on rather than ignore or simply label it as myth under the image of one Reason.

(9) To understand history, we employ hermeneutics; to apply hermeneutics to history, we have to prepare experiencing enlightenment. For the vision of our "local holism" is so limited that we cannot be taught how to translate or interpret history in its own way. Rather, the way we have used to ought to be questioned before we find its position in the history of ideas.

In one word, it is the time that philosophy does more research on the conception of enlightenment.

10

The Limit Of Analysis

--- *Incommensurability Revisited II*

Dudley Shapere is always in search of clearness in philosophical analysis. That is the reason why he tried to persuade Kuhn in 1964 that the difficulties in Kuhn's book will require not so much further historical evidence but "at the very least -- more careful scrutiny of his [Kuhn's] tools of analysis."[177] This is also why he emphasizes at the end of his 1966 paper, "Meaning and

[177] D. Shapere, "The Structure of Scientific Revolutions", *Philosophical Review*, 73, 1964, p.394.

Scientific Change", the need for adequate analytical tools in approaching the investigation of science. Shapere warns that we must beware of analytical tools that have not been adequately clarified. Challenging Kuhn's way of analysis, Shapere alternatively suggests:

> An adequate vocabulary for talking about science must, of course, be developed if understanding of science is to be achieved. But the vocabulary we adopt must not be laid down in advance of detailed examination of cases, as a set of logical categories which the cases must fit; this was a fault too often with philosophers of science in the past. Nor must our terms be employed without adequate analysis, and in such a way that those fundamental tools themselves determine the outcome of our investigation, as we have found to have been the case with some more recent attempts to interpret science.[178]

What Shapere aims at here is the newly professionalised discipline, the history and philosophy of science, especially Kuhnian approach of science. For Shapere, Kuhn's demonstration is more of a logical outgrowth of conceptual confusion owing primarily to the use of a blanket term, paradigm. To clarify this vague and ambiguous term, Shapere advises Kuhn to completely abandon his notions of incommensurability and paradigm. Once these two mysterious concepts are given up, Shapere predicts, Kuhn's approach in 1962 will be clear and satisfactory in his analysis of science.

This chapter is concerned with the issue of language analysis in relation to Kuhn's commitment to analyzability.

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[178] D. Shapere, "Meaning and Scientific Change", in Robert G. Colodny (ed.): *Mind and Cosmos*, University of Pittsburgh Press, 1966, pp.80-81.

Indeed, Kuhn takes Shapere's advice very seriously and regards it as "the most thoughtful and thorough negative account of this problem [i.e. that of a paradigm]."[179] Only noticing this thread, can we notice why as early as in 1965 Kuhn started controlling the inflation of his notion of paradigms in "Logic Discovery or Psychology of Research?" In a certain sense, his three attempts in 1969 were further devised in response to Shapere's suggestions of philosophical analysis as well as to M. Masterman's detailed constructions in her paper, "The Nature of a Paradigm". In all these commitments, one of Kuhn's strategies aims at mending the analyzability of his entire approach of scientific revolutions.

In 1965, for example, Kuhn first retreated from his stance in SSR to the one in "Logic discovery or Psychology of Research?" by interpreting 'paradigm' as a notion underscoring the dependence of scientific research upon concrete examples. In particular, under the influence of Wittgenstein's view about "family resemblance", Kuhn designed an "analyzable" swan case which he later exploited in ST to stress a constructed epitome of some elementary scientific knowledge.

However, Kuhn's understanding of analyzability is not entirely in the way Shapere appreciates, and his motivation was not merely to meet Shapere's requirement of analytical tools which have been emphasized throughout the current philosophical tradition. For

[179] Cf. footnote 2 in Kuhn's "Second Thoughts on Paradigms", in *The Structure of Scientific Theories*, (ed.) by F. Suppe, University of Illinois Press, 1977, p.459. Also see the footnote 9 in SSRE, p.186.

Kuhn, "logical articulation is not a value for its own sake, but is to be undertaken only when and to the extent that circumstances demand it."(CGK, p.16) His main concern is how to save the SSR approach which appears problematic to many mainstream philosophers. Thus analyzability for Kuhn, on the other hand, ought to be accounted for, in terms of a Wittgensteinian analysis of a natural family, "a class whose members resemble each other more closely than they resemble the members of other natural families."(CGK, p.17) For the analysis of family resemblance, Kuhn remarks, is much more thorough than the logical articulation since the former does not have to introduce explicit generalizations like "All Swans are white".[180]

In other words, for Kuhn, that the articulation of family resemblance is prior to the one of analytical tools is in that the former can properly meet Shapere's restriction of analysis.[181] This is the kind of analysis, in Kuhn's view, that has been ignored in contemporary epistemological discussions. Also, Kuhn warns, the analysis of family resemblance reveals the aspect that "we may do much violence to our understanding of the nature of knowledge."(SST, p.482) Once discerning the magnitude of learning the similarity relationship in science, we shall see, Kuhn is

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[180] Kuhn conceives that generalizations of this kind are always needed beforehand. For the logical articulation, unlike his "example articulation", always needs an axiom before analysis can be conducted.

[181] That is, as Shapere requires, the vocabulary we adopt must not be laid down in advance of detailed examination of cases, as a set of logical categories that the cases must fit.

persuaded, that "shared examples can serve cognitive functions commonly attributed to shared rules."(SST, p.482) In consequence, those logical rules should be adequately clarified only after understanding the more substantial feature of our way of knowing the world, i.e., learning the similarity relationships.

Given this understanding about philosophical analysis, Kuhn proclaims that the search for analyzability is perhaps not at all in the direction pointed by Shapere, the direction clarifying analytical tools we have been employing throughout a long philosophical tradition. Rather, analyzability should be first understood as to involve a disparate view that our basic criterion "is a perception of similarity that is both logically and psychologically prior to any of the numerous criteria by which that same identification of similarity might have been made."(SST, p.472) In this, the search for analyzability is, Kuhn insists, in the process of what we learn by doing science rather than by acquiring rules of analysis for doing it.

On one side, Kuhn insists, what we learn by doing science is analyzable in that it is the tested and shared possessions of the members of a successful group, and the novice acquires them through training as a part of his preparation for group-membership. On the other side, exemplars are not in principle unanalyzable because, Kuhn reports, a computer program designed to investigate their properties at an elementary level is on the way. With this trendy project which was originally suggested by Masterman, Kuhn's essential point aims at, what is more, the potential of mechanical analyzability of his project:

> When I speak of knowledge embedded in shared exemplars, I am not referring to a mode of knowing that is less systematic or less analyzable than knowledge embedded in rules, laws, or criteria of identification. Instead I have in mind a manner of knowing which is misconstrued if reconstructed in terms of rules that are first abstracted from exemplars and thereafter function in their stead. (SSRE, p.192)

In this thrust, Kuhn is not suggesting an analyzable process that is not potentially explicable in terms of neuro-cerebral mechanism. Rather, what he wants to stress is that the only way that the explication would differ from traditional analysis is that it will not answer the question of requesting for rules beforehand. Or, as he puts it, it is not "system but a particular sort of system that [he is] opposing."(SSRE, p.192)

It is no doubt that Kuhn epistemologically uncovers a vicious flaw of the analytical tradition, i.e., the limitation of traditional logical analysis. For the tradition ignores the essential position of learning similarity relationship in the construction of our knowledge. It is evident that this kind of learning cannot be cast in a form that fully resembles the logical or mathematical proof envisioned by academics. However, it is even more plain, I conceive, that this kind of learning reveals, in certain sense, Kuhn's own flaw in his commitment to analyzability of exemplars when reinforcing the relevance of an unresolved computer program.

Although the project seemingly corresponds with Kuhn's appeal that one need not have to ask questions such as 'similar with respect to what' when doing science, it is simultaneously inserting a 'what', the confidence in the power of computer program, that presupposes the typical way of traditional logical analysis.[182] That is, with this new project, Kuhn envisages that a particular artificial system, a product in the so-called computer age, can eventually provide answers to analyzability concerning the cognitive process of learning similarity relationships. In other words, Kuhn's new project in turn yields a new interrogation of whether a computer program possesses such capacity to fulfil Kuhn's search for analyzability of learning similarity relationship. All in all, it points to an awkward position in Kuhn's attempts in 1969. For in either way, he did not make any sound defense. Rules, or exemplars? Kuhn simply further muddles the situation of analyzability. In this connection, I shall first detail the inconvenience of his computer program, and then further address the implication of the notion of incommensurability and its relation to philosophical analysis.

It is Masterman, the computer expert at Cambridge University, who inspired Kuhn's commitment to the computer program for the sake of analyzability of his exemplars:

Now there are two forms of formal thinking which are

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[182] The program is in contrast to Kuhn's belief. For digital computers operate arithmetically, or according to other logical rules. And, what is more, digital computers work by manipulating symbols and not by direct analogues of the quantities represented. That is, they do work by storing the instructions or programmes which are generally based on the result of mathematical logic.

> relevant to the analysis of main-feature replication; both of these have emerged from the computer science. The first of these, on which there is now quite a literature, is the mathematics of classification, or of 'clumps'; i.e. the formalization of the process of finding Wittgensteinian families. The second of these, on which there is almost no literature, apart from the general literature on mechanized pattern recognition, is the set of procedures for making a digital computer make an 'inexact match' between two formulae which are highly similar to one another, but not quite the same.(CGK, pp.85-86)

Kuhn envisions obtaining analyzable results from the second approach. That is, a process like Johnny case, Kuhn predicts in 1969, can be readily modeled on a computer (although he confessed that he was in the early stages of such an experiment). Theoretically, it is quite feasible, Kuhn holds:

> A stimulus, in the form of a string of *n* ordered digits, is fed to machine. There it is transformed to a datum by the application of a preselected transformation to each of the *n* digits, a different transformation being applied to each position in the string. Every datum thus obtained is a string of *n* numbers, a position in what I shall call a *n* dimensional quality space... (SST, p.474)

Here Kuhn seeks to set up a relationship between a digital computer and the Johnny case in order to dispel the charge of unanalyzability of his entire approach since 1962. But the set-up involves problems and danger. What is the nature, for example, of the relationship between the computer and the Johnny case? Simulation, or equation? How does it support the analyzability of the case if it is the former? How could it be if it is the latter? And the toughest of all: How does Kuhn manage to use this set-up to present that the process of learning similarity relationship need not answer the question "Similar with respect to what?" No doubt that in his computer commitment Kuhn was not aware that a computer has been distinctively set up by a received standard of analysis, a 'what', in its system for the answer of analyzability. Also no doubt that, by recourse to the seeming power of digital computers, Kuhn contrives to satisfy those philosophers of science who require the clearness of his philosophical analysis.

But contemporary cybernetics tells us that the black-box theory, which paved the way to compare a computer and the behaviours of a living body, cannot so far provide a basis for equation between the two. Not only in that there exists incommensurability between the sets of simulation language employed respectively by a computer and a living body, but also in that, up to now, science has had no means to link a computer to human mind.

In this, what Kuhn seeks is at most simulation between a computer program and Johnny's mental activities rather than equation of the two. To this extent, we can focus on the limitation of computer simulation to further clear Kuhn's unresolved computer project. That is, in my opinion, following problems of computer should be first taken into account:

(1) What computers have simulated is only on the basis of those parts of formal and mathematical logic which hither to have been adequately clarified. Given that there are still many problems in these logics to be further solved, what computers have simulated is at most part of our thinking activities even in terms of these logics. So in order to make a comprehensive mechanical simulation

even in light of logic, we still have a long way to go.

(2) In addition to formal and mathematical logic, there exists dialectical thought accompanying the logical thinking activities of the human brain. And this is the area that contemporary philosophy somewhat avoids tackling so far and that should be seriously researched on without any excuse. From any aspect, it is time to make efforts to investigate the tradition of dialectics from Socrates to Hegel. Without further advancement in this area, computers of next generation still cannot make fully simulation between machines and living brain. It is philosophy, therefore, not the computer program, that should first provide computer science with adequate clarification of a dialectical way of our thinking if the science is to carry out concrete research and eventually to formalize it even for the aim of simulation.

(3) Among the human being's conscious activities, there also exists the process of enlightenment or illumination which we already investigated in Chapter 9. Up to now, few philosophers have made an effort to scrutinize the phenomena; most contemporary philosophers simply regard it as irrational and ignore its consequence. However, this is also one of the essential features of our creative thinking which badly needs further research in philosophy. Without further adequate clarification on this issue, the simulation between computers and a living brain would lose its substantial steam to catch the real strength of the required knowledge. And, what is more, I sincerely doubt that the way of mental enlightenment can be formalized in the near future to

satisfy the computer's ambition of programming, not even mention from simulation to equation, ways of human being's thinking.

(4) In addition to conscious activities, there are unconscious activities among all kinds of operations in our brain. According to Ivan Pavlov's theory, an animal's sensation and will are basically instincts; conditioned reflex is based on unconditioned reflex. And conditions for human beings are even more complicated since they are influenced by many social factors. In this, it becomes very difficult to conduct satisfactory human experiments in this field to formalize the unconscious. As we know, it is still in an early stage of establishing a theory for such a phenomenon. In this aspect, consequently, computers have difficulties to overcome its own limitation even in order to perform simulation operations.

(5) The human brain is supported by, and only part of, human body. A part cannot be equal and thereby a substitute for the whole body. Furthermore life of a human body originated in the ecological environment; it will perish if it is separated from the environment. So, our thinking activities are closely connected with conditions of our body as well as those of our environment. But, on the other hand, a person, unlike plants or animals, has a strong initiative to reconstruct the environment to adapt to his needs. Due to all these factors human beings are both social beings and ecological beings who connect nature and their minds in unique ways. In this aspect, to simulate a person like the above kinds of being does not appear a scientific subject at all. Perhaps it needs more interdisciplinary research, in the first place, which might be

achieved in patient cooperation between all walks in which computer science is only one means of the subject.

(6) For human beings, after all, a phrase, or an exemplar, is a reservoir of experience in history. That is, what is meaningful is not merely a question of linguistic analysis but also of experience and understanding during a period of time. It is noticeable that a proverb spoken by an old man has a disparate meaning from that spoken by a young lad. Because, they not only have different experience but also experienced quite different accumulated experiences on life time to interpret that proverb. So it is a dilemma, at least for the present, for a computer to program historical conceptions of these kinds. For the quality as well as the quantity of information that the same amount of language can be delivered to the machine depend variously from individual to individual. On the other hand, the information of different language expressions is only meaningful to those who have different experiences. This incomplete equivalence between language analysis and meaning also has to be accounted when conducting computer simulation.

Nevertheless, a computer employed to simulate a human attribute is only an extension of part of human being's brain and hands. Under the circumstance, the computer can be rather than a person only with respect to those attributes which have been formalized. Other than that, the simulating computer does not possess any other human attributes and it is subject to human's control or ignorance in these areas. Therefore, a computer is by all means human being's tool which is attached to them merely in a one-way direction since it is only to simulate human being's way of thinking, not vice versa. Suppose computers can, on the other hand, simulate all the attributes of our human beings and thereby they could be independent of human being, the simulation programming, however, is still done by a human being who then becomes superior, again, to the computer. (Metaphorically speaking, the human being becomes superior in the sense of God of computers.)

Thus simulation cannot, in any sense, get rid of its basic limitation that it can never become the judge of analyzability in above situations.

As Norbert Wiener puts it in one of his books entitled *The Human Use of Human Beings*,[183] it is human beings who provide the possibility of analyzability for computers. That is, if a man's cognitive ability is suspensive, then his cognitive ability to theorize about himself will also be suspensive; if the ability of a person continuously develops, his ability to cognize himself develops continuously too. For Kuhn, it seems to me, it is in fact a deeper epistemological question to his enterprise of computer programming: How can it be possible to write out a computer program of an exemplar when it cannot itself be analyzable?

Also, as Wiener insightfully points out:

> We are in the position of the man who has only two ambitions in life. One is to invent the universal solvent which will dissolve any solid substance, and the second

[183] Norbert Wiener, *The Human Use of Human Beings*, Houghton Mifflin Company, 1954.

> is to invent the universal container which will hold any liquid.[184]

Whereas, as have just discussed, whatever a computer can be, it is neither the universal solvent nor the universal container. "*What is used as an element in a machine, is in fact an element in the machine.*"[185] So, Kuhn's search for analyzability of exemplars may be not urgently in the hope of digital computers. Rather, it should be clarified at first in the area of Kuhn's exemplar itself or the process of learning similarity relationships.

Unfortunately, we have not heard of any progress report from Kuhn whatsoever since he optimistically forecasts the exciting project in 1969.

Indeed, it is odd for Kuhn to, on one side, employ computer as a universal arbitrator of analyzability while, on the other side, opposing the role of logic rules, which in fact is the basis of the former, as the last judge. He was not even aware, unfortunately, of the fact: where the above contradiction points is where the incommensurability exits. For the school that believes in the limit of logical rules, for example the Kuhnian school, ought to recognize, through the limit of the analytical school of philosophy, the limit of the analyzability of a computer. Kuhn conflates what he ought to defend with what he ought to abandon, and distorts his promising defense of learning similarity relationship. The distortion develops to such an extent that he has

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[184] Ibid., p.129.

[185] Ibid., p.185.

no realization of utilising incommensurability to further strengthen his stance.

Another crucial element in the process of learning similarity relationship is also missing in Kuhn's description of the computer program. It is "the procedures employed during learning transcend the usual limit of ostension."(SST, p.504)

As the second aspect to reply Suppe's charge that his Johnny case is at most a "ostensive definition", Kuhn argues that transcending thrust in the process of learning reveals another trait of learning similarity relationship. To my mind, it is a process of gradual enlightenment in our cognition, a gradual procedure of ***Aufhebung***. For some reason, I regret to see, after that justification Kuhn did not put any weight on this "transcending" thrust whatsoever. However, this is another cognitive process which is, I consider, largely ignored by contemporary analytical philosophy.

As a matter of fact, as Kuhn realizes, Johnny is not only shown by his father dogmatically what ducks are, but also allowed to attempt identifications for himself. Given the context that Johnny is not only shown ducks, but also swans and geese, he must himself conceptually transcend what he is perceiving in order to grasp the generalization of ducks. In the transition phase from ostensive experience of ducks to profound understanding of the concept of ducks, Johnny's mental facility of learning similarity transcends the limit of concrete encounters with ducks. This is the process of, as I above termed comparing with the sudden operation of ***Aufhebung***, gradual enlightenment, a continuous accumulation as well as illumination in one's cognitive experience.

Suppose at the very beginning, Johnny can outline the upshot of his transcendence of ducks, with the mental function of family resemblance, say, "ducks can swim", "ducks necks are shorter than swans' and geese's", etc. But when grown older, with the same support of the mental function, he should be able to apply to nature, transcending his past resemblance of natural families, a symbolic law-sketch like 'f = ma' to the movement of ducks, geese, and swans. His knowledge of nature thus grows not only with the accumulation of experiences, but also with his continuous reflection of those experience, transcending the limit of ostensive experience to linguistic as well as tacit perception of the world. And this is not a topic of analyzability in its old sense. Given the long-standing inadequacy of thorough accounts to mental enlightenment of this kind, the workings of transcending from experience to mental generalization, and fresh approaches to phenomenon of this sort are badly needed.

In other words, to catch 'family resemblance' must imply the acknowledgement of a transcendental function in epistemology. Otherwise we cannot explain where our mental constructions, such as rules, laws, axioms, and theory, are all from. Although there are in general no rules prior to the process of learning similarity relationship of the sort as Kuhn envisions, rules are the result, we must admit, of that transcending process. Including what I have outlined of the sudden enlightenment in Chapter 9, a sudden way to see 'family resemblance', transcending function of ***Aufhebung*** occupies a distinctive position in the operation of our cognition.[186] And the result of the transcending process is 'family resemblance', either of 'natural' or 'mental' families, in our self-consciousness. In this sense, 'family resemblance' or 'exemplar' can be amounted to what Kant once called, 'transcendental unit of self-consciousness'.[187]

Hegel also agrees with Kant at this aspect although he laughs at Kant's odd expressions like 'transcendental unity of self-consciousness' (taking it to be an ugly expression).[188] For Hegel,

[186] This is why Kuhn himself cannot finally clarify the relation of exemplars to scientific theory. Kuhn actually was forced, under Suppe's accusation of ostension, to point out of process of transcending during ostensively learning family resemblance of ducks. Other than the occasion, Kuhn seldom mentions the transcending function. Perhaps he was afraid of being caught again of irrationalism.

[187] As Kant says: "The *I think* must be able to accompany all my ideas ... This idea is an act of spontaneity ... I name it pure apperception ... or original apperception ... because it is that self-consciousness which can be accompanied by none further. The unity of it I also call the transcendental unity of self-consciousness, in order to denote the possibility of cognition *a priori* from it."(I. Kant, *Kritik der reinen Vernunft*, §16.)

[188] Hegel considers: "Another transcendent of the same kind is the self-consciousness which is identical with itself and infinite in itself, as distinguished from the ordinary consciousness which derives its form and tone from finite materials. That unity of self-consciousness, however, Kant called *transcendental* only; and he meant thereby that the unity was only in our minds and did not attach to the objects apart from our knowledge of them." (Hegel, *Encyclopedia Logic,* (trans.) by William Wallace with Foreword by J. N. Findlay, Oxford University Press at the Clarendon Press, 1975, p.70.)

the transcendental unity is *"Being-for-self"* which, as reference to itself, "contains abstract Being and Being modified as non-substantial elements."[189] As simple Being, the unity is simple self-reference, Hegel defines, while as Being modified it is determinate. However, Hegel further delineates:

> the determinateness is not in this case a finite determinateness -- a somewhat in distinction from an other -- but infinite, because it contains distinction absorbed and, annulled in itself.[190]

With this expression, Hegel alludes the unity, being-for-self, to the result of *Aufheben*. For he conceives that the word *aufheben* connotes much more clear implication than Kant's usage 'transcendental' which readily causes confusion with *'transcendent'* that may be said to be what steps out beyond the categories of the understanding.[191]

As a matter of fact, it seems to me, what Kuhn considers as "the procedures employed during learning transcend the usual limits of ostension" has more 'family resemblance' to Hegel's 'aufheben' than Kantian sense of 'transcend'. For the latter merely denotes that something passes beyond its limits, sometime even beyond a human limit. Whereas the former connotes, as Hegel puts it,

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[189] Ibid., p.141.

[190] Ibid.

[191] Cf.ibid., p.70. Hegel clarifies the notion 'transcendent' that it expresses a sense that is first employed in mathematics and thus never uses it himself as one of the basic notions in his system.

> Thus double usage of language, which gives to the same word a positive and negative meaning, is not an accident, and gives no ground for reproaching language as a cause of confusion. We should rather recognize in it the speculative spirit of our language rising above the mere 'either-or' of understanding.[192]

After this long journey, however, the issue of learning exemplars, both learning family resemblance and transcending the limits of ostension, regresses to the one we discussed in Chapter 9 on Aufheben. The only difference is that what we discussed in Chapter 9 is about Kuhn's sudden enlightenment in 1947, and here we are concerned with a gradual transcending process of *Aufhebung*, a process I shall call gradual enlightenment. But, after all, their nature is still in what Hegel clarifies:

> (1) to clear away, or annul: thus, we say, a law or a regulation is set aside; (2) to keep, or preserve: in which sense we use it when we say: something is well put by.[193]

So, in the process of gradual enlightenment or learning similarity relationship by transcending limits of ostension, to know the family resemblance of ducks is not the process resulted in ostensive teaching, as Wittgenstein would say. Rather, as Hegel highlights, "natural things never attain a free Being-for-self."[194] In terms of Kuhn's Johnny case, it is Johnny and we who first undergo the process of Aufheben and then learn family resemblance

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[192] Ibid., p.142.

[193] Ibid.

[194] Ibid., p.141.

of natural families such as ducks or theoretical families.[195]

And the family resemblance as such is, as Bridgman once holds, only our inventions referring to the former. Given this, scientists are not of the community just content with the result of learning similarity relationship and thereby with no intention to know "Similar with respect to what?" On the contrary, they are also the community eager to know not only how to acquire the family resemblance, but also what the similarity relationship is, and what their transcending inventions exactly refer to as rules, etc.

In this, I conclude that Kuhn's defense is inadequate in two ways when attempting to search for analyzability.

First, he need not give in to the analytical tradition of philosophy by appealing to the judgement of a computer program in a wish that it might provide the proof of analyzability of his approach. For one thing, Kuhn was not aware that there are incommensurable paradigms on the issue of analyzability: one is represented by Shapere who believes in analytical tools which are linguistic in nature; the other by Kuhn himself who believes in exemplars which can be mainly grasped by doing and bearing sciences. Thus, if defending the analyzability of exemplars, in consequence, Kuhn ought to point out the analyzability of exemplars itself rather than to address analytical programming, and to

[195] Kuhn's enlightenment is the case that he suddenly grasped the theoretical 'family resemblance' in Aristotle's entire system and thus realized that there exist incommensurability between theories in history.

convert himself as a computer programmer. In my opinion, the exemplar can demonstrate itself clearly without any recourse to the analyzability. Also, as we have discussed, computers have not yet developed to such a degree that they can be programmed to show what Kuhn expects in the aspect of learning exemplars.

Second, learning an exemplar can demonstrate itself clearly in that it is in nature against the traditional philosophical image of analysis, especially of logical analysis. The latter is of course incommensurable with Kuhn's view of analysis, exemplar analysis. It is a completely different way of clearance and can be itself sufficiently supported by cases like Johnny's. In other words, cases of this sort do not have to resemble the traditional logic of induction and deduction which can be in certain degree programmed into computers. Since, unlike the induction, learning the similarity relationship includes the process of transcending, exactly put, ***Aufhebung***, which traditional logic has less to offer and thus to analyze. Also unlike the deduction, grasping family resemblance does not presuppose, as Kuhn views, axioms that can generally raise questions like "Similar with respect to what?".

In a nutshell, whether from traditional or contemporary points of view about logical analysis, analyzability is no longer a weapon of analytical philosophy to suppress the phenomenon of ***Aufhebung*** in our cognitive process. The phenomenological issue of this kind is a philosophical example of incommensurability, as Feyerabend has recently noted, between analytical school and historical school of, at least, philosophy of science.[196] What is more, the latter as a matter of fact has as long a tradition as the former, and it is somewhat ignored.

Indeed, the tradition ignored is one of the lost western philosophical roots which can be traced as early as the era of Greek philosophy. In particular, Aristotle posits that to know *"What is Being?"* is to know *"What is substance?"*[197] And to know substance, Aristotle states, we have to be aware of the difference between parts and whole, a pile of heap and unities, or artifacts and entities etc. For Aristotle, substances are entities, unities, and wholes which are thus composed as well as can be divided by parts.[198] But Aristotle holds that the whole is more than the sum of its parts. Thus in short of grasping the whole, logical analysis cannot always be employed as the only means of analyzing the way we really think.

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[196] Cf. Feyerabend, *Farewell to Reason*, Verso, 1987, pp.294-319.

[197] Cf. Aristotle: *Metaphysics*, (trans. & glossary) by Hippocrates G. Apostle, The Peripatetic Press, 1979, p.109.

[198] Aristotle considers: "a whole" means (1) that form which is absent none of the parts of which it is said to be by nature a whole.(2) That which contains the objects contained in such a way that they are one, either in the sense that each of them is some one thing, or in the sense that what is composed of is one. ...(3) In the case of a quantity which has a beginning and a middle and what is last, if position in it makes no difference, it is called "all" (or "a total"), but if position makes a difference, it is called "a whole"; and if both are possible, then it is called both "all" and "a whole". the latter is such that its nature stays the same by a change in position, but not its *shape*, ... (ibid., pp.97-98)

Detailed investigation of this holistic and anti-reductionist line of philosophy would enumerate a long list of Western thinkers who, in one way or another, contributed perspectives to defeat the analytical image that putting together the analysis of parts can account properly for the appraisal of whole. Nonetheless, the analytical tradition only represents part of western philosophical traditions. In precise, it is the tradition represented by Descartes who claims that science is just to break down every problem into as many separate simple elements as might be possible.[199] So are the contemporary opponents of Kuhnian approach.

Epistemologically speaking, for a long time, the holistic tradition opposes to the rubric of analytical procedures of science which requires resolution only from logical clearance and one-way thinking or linear causality analysis. But as Aristotle remarks, there are at least three different ways of reasoning: practical reasoning, theoretical reasoning, and intuitive reasoning.[200] Of these reasoning, practical one is of human action and ends; theoretical one of logical affirmation; intuitive one of grasping the unchangeable and first terms. As practical reasoning is with regard to human goods, it is concerned with the particular, the whole, and the substance. Aristotle regards,

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[199] Cf. the second maxim of Descartes' *Discourse on Method*.

[200] Philosophers do know Aristotle's division of practical and theoretical reasoning, but few of them nowadays admit the distinctive position of intuitive reasoning which Aristotle defines as one of the components of philosophic wisdom (cf. Aristotle's *The Nicomachean Ethics*, (trans.) by D. Ross, Oxford University Press, 1980, pp.144-146).

> the term 'good' is used both in the category of substance and in that of quality and in that of relation, and that which is *per se*, i.e. substance, is prior in nature to the relative (for the latter is like an offshoot and accident of being; so that there could not be a common Idea set over all these goods. ... clearly it cannot be something universally present in all cases and single; for it could not have been predicted in all the categories, but in one only.[201]

That is, in light of practical wisdom, the result of practical reasoning, what we can really tackle is generally concerned with the ultimate particular fact. But "it is opposed, then, to intuitive reason".[202] For intuitive reason is of the limiting premises, Aristotle claims, for which no reason can be given. Like the further fulfilment of practical reasoning, Aristotle recognizes:

> the intuitive reason involved in practical reasoning grasps the last and variable fact, i.e. the minor premises. For these variable facts are the starting-points for the apprehension of the end, since the universals are reached from the particulars; of these therefore we must have perception, and this perception is intuitive reason.[203]

However, Aristotle considers that intuitive reason is by no means from nowhere. History always accounts, Aristotle insists. Because, "we think our powers correspond to our time of life, and ... a particular age brings with it intuitive reason and judgement."[204]

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[201] Ibid., p.8

[202] Ibid., p.148.

[203] Ibid., p.153.

[204] Ibid.

In light of this, Aristotle urges that,

> we ought to attend to the undemonstrated sayings and opinions of experienced and older people or of people of practical wisdom not less than to demonstration; for because experience has given them an eye they see aright.[205]

No doubt that this is the heritage sources of Hegel's insights which is further developed into his system of synthesis and notion of synthesis, ***Aufhebung***. Therefore the notion of *Aufheben* is also by no means from nowhere but exactly along the avenue of the western tradition of philosophy. To be precise, it is a well-lost tradition of philosophy itself, a huge blank field for contemporary philosophers to cultivate now.

Also no doubt, of course, that Shapere's appeal to analytical tools, which is outlined at the very beginning of the current chapter, appears quite pale especially when he seeks the answer in an adequate vocabulary which must be developed. It is even more problematic when he insists that we have to presuppose "the adequate vocabulary", if understanding of science is to be achieved. It seems that Shapere regards the dubious "adequate vocabulary" as a new domain of analytical philosophy. As a matter of fact, it is Shapere himself who holds suspicious bias in the arena of contemporary philosophy of science and tries more ambitiously than Kuhn did in 1962. Shapere is not aware that what he calls for contemporary philosophy of science to withdraw to the 'long philosophical tradition' is actually a call only to one of

[205] Ibid.

the western traditions of ideas. For he has not yet seen that the attempt is also contradictory and, of course, confusing when he criticized Kuhn in terms of 'analytical tools' which, he considers, have not yet developed an adequate vocabulary, like Kuhn's report of computer programme, to solve the conflict.[206]

Obviously, Shapere aims to construct a "realistic domain" of information in order to finally surrender "some more recent attempts to interpret science",[207] especially those of Kuhn's exemplars. But, people are perplexed in that, as Wiener might say, Shapere seems to try to invent both a universal solvent and a universal container at the same time. That is, one will be bewildered by Shapere's magic tools which seem neither 'a set of logical categories' nor 'those fundamental tools', nor Kuhn's exemplars or paradigms. But even oddly when he stubbornly demands that 'our terms' must be employed with adequate analysis while refusing both traditional and practical analytical tools.

However, there is a conducive clue in his well-known essay, "Scientific Theories and Their Domains".[208] In section II of this essay, Shapere starts with the conviction that, in science, items

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[206] Shapere tries to convince philosophers that "the vocabulary we adopt must not be laid down in advance of detailed examination of cases, as a set of logical categories which the cases must fit. But simultaneously he insists that our terms must not be employed without adequate analysis, and in such a way that those fundamental tools themselves determine the outcome of our investigation (cf. D. Shapere, "Meaning and Scientific Change", *Mind and Cosmos*, (ed.) by R. G. Colodny, University of Pittsburgh Press, 1966, pp.80-81).

[207] Ibid., p.81.

[208] SST, pp.518-565.

of information come to be associated together as bodies of information having the following characteristics:

(1) The association is based on some relationship between the items.

(2) There is something problematic about the body so related.

(3) That problem is an important one.

(4) Science is "ready" to deal with the problem.[209]

To this extent, I feel that it is ironic how Shapere shall manage to criticize Kuhn while retaining the substantial points of similarity relationship into his own "domain".[210] For me, Shapere's story of analysis becomes so interesting that with what Shapere has put out in his outline of the domain of information, he should legitimately ask himself the same question when he questioned Kuhn's exemplar approach in 1969:

> Prof. Kuhn talks about there being similarity relationships, and I want to ask first about this ... I want to start by asking whether the similarity relationships are there to be found? (SST, p.506)

The only change in the question should be 'Shapere' instead of 'Kuhn', 'some relationship' instead of 'similarity relationship'. Once this question is clarified, Shapere's attempt appears, unfortunately, something which has a "family relationship" with

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[209] Cf. SST, p.525.

[210] And interestingly, nobody even speaks on the side of Kuhn on his similarity relationship while philosophers, like Suppe, consider that Shapere is pointing a new direction which stands between "the extreme *Weltanschauungen* views of, for example, Kuhn and Feyerabend and the views of logical positivism"(cf. SST, p.683).

Kuhn's approach in light of the Wittgensteinian 'family resemblance'. And Shapere's realistic approach turns out to be one of human beings, especially of scientists. Consequently the domain of information will be, like Kuhn has partly shown in 1969, relative to what we act, but not to what we analyze. In all this, I conclude this chapter, by concluding Shapere's characteristics of the domain, as following:

(1) The associated is relatively based on what we experience and practice in finding some similarity relationships between items, for example, ducks.

(2) There is nothing problematic about the body of information so related. For nothing is hidden behind.

(3) One important problem would appear if we do not notice that *Aufheben*, or intuitive reasoning, or transcending procedures, in the process of knowing the world, is also essential to associate the domain of the body of information together.

(4) Philosophy should be "ready" to deal with the problem of *Aufheben*. For it is not only a worthy tradition to be reminded to students, but also a worthy field to be pluralistically and also realistically introduced to the academics in order to show the limitation of analytical philosophy.

(5) The conflict between different attitudes towards 'practical reasoning', 'intuitive reasoning', as well as 'aufheben' reasoning, (I believe they are all ways of our reasoning), has done nothing but support the unsurprising phenomenon in our reasoning, the one of incommensurability.

(6) Therefore, philosophy itself already has incommensurable problems for a long time within its domain. The problems have been so distinct that here is no need to hide them with sharpening the "analytical tools". The real ways of our thinking is, in any sense, much more rich and complicated than the way that analytical thinking has imagined.

In a word, it is nothing but the existence of the limit of language analysis that underpins the perspective of incommensurability in epistemology. It is a perspective that, as a matter of fact, has already existed as common phenomenon of our reasoning in the arena of philosophy *per se*.

11

The Limit of Education

--- Incommensurability Revisited III

In replacing 'paradigm' by 'exemplar', Kuhn did not adequately improve his 1962 approach to science. For the replacement, his new concept of 'exemplar' (say *the Johnny case)* and its related computer project, cause more problems that need to be further clarified. This chapter will discuss the flaws in Kuhn's defense of exemplars with an eye on the issue of incommensurability in its relation to the process of "learning" resemblance relationship.

As Frederick Suppe says in his commentary on Kuhn's STP at the Urbana Symposium:

> Kuhn tells us in Section III that the symbolic generalization applied often are special-case ones, and it is through the study of exemplars that one acquires the ability to determine the appropriate form of the generalization. Presumably the resemblance relation is supposed to play a crucial role in doing this. But if the resemblance relation only associates features of phenomena with terms occurring in generalizations, it is hard to see how that will enable one to "see" or determine the appropriate *form* of the generalization to use, for knowing which words to use does not tell you how they should be combined to obtain the appropriate description. (SST, pp.486-487)

In particular, Suppe argues that Kuhn's main flaw in STP, the learning resemblance relation in the process of learning exemplars, in fact has nothing to do with the forming of symbolic generalization. But, Suppe points out, the forming has been presupposed as the function of symbolic generalizations which are to be learned in the process of exemplar learning.[211]

Replying to the charge, Kuhn supplies the thrust of "transcending ostension". As I have discussed in last chapter, with recourse to the concept of "transcending", he hopes to disentangle Suppe's misinterpretation of the Johnny case. But for Suppe, Kuhn's thrust of transcending procedure in cognition only provides a partial answer to Suppe's question. Suppe argues,

> Since these symbolic generalizations occur in the

[211] Cf. SST, p.487; also SSRE, pp.182-184. In terms of this, Kuhn holds that the generalizations "function in part as laws but also in part as definitions of some of the symbols they deploy." (SSRE, p.183)

> exemplars, and the resemblance relation develops as one learns to use these generalizations in application to nature, it follows that there is an important sense in which the study of exemplars does supply the theoretical terms with a partial definition -- in the sense they become "implicitly defined" as a result of learning to apply symbolic generalizations to nature. The only way I can see to avoid this claim is to give up the assumption that possessors of different disciplinary matrixes invariably attach different meanings to whatever words they possess in common; doing so would in turn require that Kuhn give up or severely attenuate the various incommensurability claims made in *The Structure of Scientific Revolutions*.(SST, pp.492-493)

Here Suppe suggests to Kuhn an alternative: if exemplar is accepted as replacement of paradigm, the notion of incommensurability should be deprived of Kuhn's approach accordingly. For the process of learning similarity/resemblance relationships which is oriented by learning constitutes a way to reach a unanimous discipline rather than incommensurable disciplines according to the nature of the process, education.

It is no doubt that Suppe acutely identifies that Kuhn's replacement from paradigm to exemplar not only makes his attempts in 1969 more questionable, but also contradictory, in terms of 'family resemblance,' in many ways. The key problem is, I presume, that Kuhn overvalues the role of education, "the special nature of science", in the scientific enterprise:

> Acquiring an arsenal of exemplars, just as much as learning symbolic generalizations, is integral to the process by which a student gains access to the cognitive achievement of his disciplinary group. Without exemplars he would never learn much of what the group knows about such fundamental concepts as force and field, element and compound, or nucleus and cell.(SST, p.471)

In his second attempt in 1969, RC, Kuhn made a similar point:

> we have too long ignored the manner in which knowledge of nature can be tacitly embodied in whole experiences without intervening abstraction of criteria or generalization. Those experiences are presented to us during education and professional initiation by a generation which already knows what they are exemplars of.(CGK, p.275)

Regarding education central to the scientific enterprise, Kuhn at the end of his third attempt, PS, specifically highlights the sociological aspects of science,

> Scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all. To understand it we shall need to know the special characteristics of the groups that create and use it.(SSRE, p.210)

In fact, Kuhn in these three attempts draws a more muddy picture of science than the one he described in Chapter V, SSR, *"The Priority of Paradigms"*, in which he first reveals the rapport of late Wittgenstein to his approach of SSR. In the chapter, the function of resemblance is intrinsically connected with the established achievements, paradigms, of a scientific community.[212] Yet, paradigm itself is not a concrete application to such an extent in 1969; it can be employed as a way to learn metaphysical conceptions. Nevertheless, paradigms in 1962 were neither fully exemplars nor symbolic generalizations as those in Kuhn's 1969 exertion. What is more, Kuhn considered at that time that paradigms are not only not rules, but literally prior to any rules in terms

[212] Cf. SSRE, pp.45-46.

of their function in understanding normal science.

However, paradigms in 1969 have been otherwise transformed into concrete cases of science as "exemplars"; the collection of "established achievements" has been partially transmuted into the secondary class of the notion, "disciplinary matrix". To this extent, a new problem appears: then what is the relation between 'exemplars' and 'rules'? For Kuhn in 1969, he still firmly insists on the point of view about 'rules' in 1962.

> When I speak of knowledge embedded in shared exemplars, I am not referring to a mode of knowing that is less systematic or less analyzable than knowledge embedded in rules, laws, or criteria of identification. Instead I have in mind a manner of knowing which is misconstrued if reconstructed in terms of rules that are first abstracted from exemplars and thereafter function in their stead. ... I am claiming that the explication will not, by its nature, answer the question, "Similar with respect to what"? That question is a request for a rule, in this case for the criteria by which particular situations are grouped into similarity sets, and I am arguing that the temptation to seek criteria (or at least a full set) should be resisted in this case.(SSRE, p.192)

But given the fact that Kuhn's learning exemplars aim to acquire symbolic generalizations (rules), how could the former still be prior to the latter? And how can the process of learning exemplars avoid being comprehended as such that amounts to establishing a process of knowing rules? Or what if exemplars have no connection with symbolic generalization in this situation?

Obviously all the resemblance relationship acquired in the process of learning exemplars not only resembles the exemplar itself, but also serves to learn the symbolic generalizations. According to Kuhn, this is the nature of science, i.e., the function of an educational enterprise for students of science to learn exemplars. In this, learning exemplars as way of knowing symbolic generalizations cannot recapture the steam of a paradigm in 1962, the essence that paradigm is not only prior to rules but also guides the scientist to produce them when they are needed.

In other words, as means of scientific education, exemplars now function, no matter whether they are descriptive or symbolic, as practical manoeuvres to pilot students of science becoming familiar with certain rules, laws, theories etc. Simultaneously, the function of learning the resemblance relationship descends from the prior position organizing the entire cognitive process to the posterior status serving the role of exemplars for acquiring symbolic generalization. At most, Kuhn's resemblance relationship performs the function of assimilation of rules, and thus has nothing to do with scientific revolutions and discoveries any more.

Due to the fact that everything learned is settled as symbolic generalizations, rules, the process would attach no unexpected meanings to whatever disciplinary matrix had played. As long as scientific education accounts for the special nature of science, students seem to be merely learning rules. No wonder Suppe concludes that incommensurability should be legitimately abandoned if the resemblance relationship becomes a genuine nature of exemplars, a process of seeing symbolic generalizations.

That is, I infer, if the study of exemplars is able to supply the theoretical generalizations to nature through our acquired potential of learning the similarity relationship, Kuhn's concern of communication breakdown disappears. For the only thing we have to do is merely to promote the trade of studying exemplars, which thereby makes the phenomenon of incommensurability vanish. In view of this, it is by no means necessary for Kuhn in his second attempt, RC, to further borrow Wittgenstein's "therapeutic techniques" to tackle the headache of incommensurability, or communication breakdowns.[213] For the studying of exemplars, given that it has the "power" as Kuhn explains, can assimilate resemblance relationships to unanimous symbolic generalizations. Thus exemplars that Kuhn has defined as concrete applications will be invested with the potential of educating students of science to answer tens of thousand questions about, unlike what Kuhn expects, "Similar with respect to what?". Does this draw the primary nature of whatever education, especially the one of scientific education?

Nevertheless, I conceive, education can never fully account for the nature of science as Kuhn envisages. Because, education cannot cover the whole story of learning the similarity/difference relationship in the process of knowing the world. This is the deeper issue, I believe, that both Wittgenstein and Kuhn are not yet adequately aware of, i.e. the limit of education. They seem unaware of the fact that there even exists incommensurable stages

[213] Cf. RC, pp.276-277. For Kuhn, at that time, therapeutic efforts make incommensurability disappear and conversion thus fulfil as "what the participants in a communication breakdown have then found is, of course, a way to translate each other's theory into his own language and simultaneously to describe the world in which that theory or language applies."(RC, p.277)

between learning resemblance relationships ***with*** exemplars and ***without.*** The formal, the learning ***with*** exemplars, is of the conventional (Kuhn's "normal science"); the latter, the one ***without***, of the revolutional or evolutional process of knowing, sudden and gradual enlightenment or transcending process. This is the distinctive difference that Wittgenstein never differentiates and that Kuhn once importantly stressed in 1962 and, unfortunately, has blurred since 1969.

In 1962, Kuhn attempted to separate two different stages in the development as well as the knowledge of science, i.e., normal science governed by an established paradigm and revolutionary science aimed at establishing a paradigm. But in 1969, at the expense of the latter, without any explanation Kuhn began to combine the two stages into one in terms of linguistic translation. That is, the revolutionary period is interpreted as follows:

> Instead, at some point in the process of learning to translate, he finds that the transition has occurred, that he has slipped into the new language without a decision having been made. ... Intellectually such a man has made his choice, but the conversion required if it is to be effective eludes him. ... The conversion experience that I have likened to a gestalt switch remains, therefore, at the heart of the revolutionary process.(SSRE, p.204)

Apparently, Kuhn not only blurs his insights on revolutionary transition which is "a reconstruction that changes some of the field's most elementary theoretical generalizations as well as many of its paradigm methods and applications."(SSRE, p.85) But also, he has entangled many different ways of revolutionary transition into the business of scientific education, i.e., learning how to study exemplars or how to translate other languages.

However, I should point out, the process of reconstruction does not, at any rate, amount to learning the family resemblance under the guidance of an exemplar (paradigm). For the former is to establish a totally new family resemblance relationship through ***Aufhebung***, whereas the latter is to observe the ***established*** resemblance relationship. It is in this mingling, I consider, that Kuhn lost the edge to recapture the strength of historical insights in SSR. For his employment of the established exemplars as the centre of his approach no longer guarantees him the potential to spell out ways of establishing a new exemplar, ways of scientific revolutions.

As a matter of fact, the scientific nature of education that Kuhn depicts is solely of his notion of normal science in 1962, which functions in customization of the scientific community in order to learn how to conventionally acquire generalizations under the guidance of an established exemplar.

But another kind of learning resemblance relationship, reconstruction, should not be taken as a triviality in the process of learning. Because, reconstruction has, as Jean Piaget once emphasized, more essential traits than the process of normally learning exemplars to the formation of our knowledge.[214] In order

[214] According to Piaget, children are born without the internal mechanisms which allow them to be logical and that they eventually construct these logical mechanisms through their experiences with the world around them.

to throw more light on the controversial issues of cognitive philosophy of science and, more generally speaking, to spell out more to the contemporary analytical epistemology, it is an issue Kuhn ought to stress rather than suppress.

In my opinion, reconstruction of an exemplar is certainly not of education. Rather, it is a process of invention, enlightenment or transcending, i.e. ***Aufhebung***. In the opposite, procedures of education, periods of normal sciences, are conducted between the periods of reconstructions. As H. Butterfield remarks, the transpositions that are taking place inside the minds of scientists themselves are not irrelevant to the following connection:

> of all forms of mental activity, the most difficult to induce even in the minds of the young, who may be presumed not to have lost their flexibility, is the art of handling the same bundle of data as before, but placing them in a new system of relations with one another by giving them a different framework, all of which virtually means putting on a different kind of thinking-cap for the moment. It is easy to teach anybody a new fact about Richelieu, but it needs light from heaven to enable a teacher to break the old framework in which the student has been accustomed to seeing his Richelieu.[215]

Reconstruction in this sense is thus the operation "to break the old framework". These cognitive workings are the area that need not answer the Kuhnian question, "Similar with respect to what?", although it does associate certain kind of similarity relationship. In certain sense, this level of grasping family resemblance without any guidance of exemplars is more substantial and primary in the

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[215] H. Butterfield, *The Origins of Modern Science, 1300-1800*, London, 1949, pp.1-2.

journey of our knowing the world than Kuhn's second level of paradigms, learning similarity relationship ***governed*** by exemplars (actually a way of learning rules). For the former can account more for the genetics of epistemology as well as the "abnormal" aspects of our knowing which have neither the Hegelian *'Notion'* nor a Kuhnian 'exemplar' presupposed at the very beginning of a certain cognitive process of human beings.[216]

In particular, the process of reconstruction is Kuhn's Johnny case without his 'Father', or Wittgenstein's cases of learning mathematics without the teacher. That there is a father or a teacher conducting the resemblance learning is in fact the image that the God is conducting the world, not that science develops itself through human beings' endeavour by doing it. Without the guidance of his father, we should admit, Johnny has the potential to differentiate/assimilate things by himself in the context of a society. Through experiences by himself, he will sooner or later know or reconstruct, through transcending talent, the knowledge of 'ducks', which is unnecessary to be directly influenced by his

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[216] Cf. L. Von Bertalanffy, "The History and Status of General Systems Theory" in *Trends in General Systems Theory*, (ed.) by G. J. Klir, Wiley-Interscience, 1972, p.21. This becomes an issue not only of epistemology, but also of anthropology and sociology. It is of anthropology in that, as Ludwig Von Bertalanffy points out, man in early culture experience themselves as being "thrown" into a hostile world, governed by chaotic and incomprehensible demonic forces. Finding orders and identity was, in consequence, naturally born when the primitives sought ways to survive out of the chaotic surroundings. It is of sociology in that certain kind of family resemblance is set up not only in a certain society in history, but also, as Kuhn stresses in 1969, it can be only on a certain communitive bases.

father's "exemplars". The same story shall be true for Wittgenstein's cases of education in the fact: in one side, we are not all children and, in the other, there are more situations which cannot be easily accounted for in terms of cases of learning mathematics, which are always ushered by a teacher. And it is even more true that we are already set in various kinds of games, not only language games. Also, most of the rules of those games can be merely learned on our own through our acquired competence of reconstruction. The question becomes, therefore, not whether we need a 'Father' or a 'Teacher'; rather, it is in that in most of the cases along the evolution of our knowing the world, people simply have no recourse to finding a "Father" or a "Teacher" at all. Indeed, these are the general 'games' that education has not yet covered, and perhaps cannot forever.

Genetically speaking, it is a fact received that the human conduct of education happened much later than our practice of discovering the world. As a matter of fact, what is more, without the endeavour of reconstructing the world, people shall have no "exemplars" to initiate the institute of education. In this, taking learning exemplars as the central process of cognition is historically putting the cart before the horse.

Given this, Kuhn's shift from paradigms to exemplars, in Shapere's words, "appears to have retreated from his earlier position in just those aspects in which it was most suggestive, important, and influential".[217] Because, keeping the revolutionary aspect of science in such a low profile while inflating the educational or normal aspect of it, Kuhn was not aware that he had indispensably destroyed the ***essential tension*** of his historical approach, which he badly sought in 1959.[218]

In other words, due to Kuhn's self-deconstruction since 1969, the essential tension in Kuhn's 1962 writing of the structure of scientific revolutions no longer exists. Instead, there appears a handicapped approach, one only of normal science or of educational commitment that focuses on the function of exemplars. To convince analytical philosophers of science that what he really intended to express in 1962 had been the structure of scientific education, Kuhn surreptitiously revises his original view of science, the one of the structure of its reconstruction. Thus the essential structure of scientific revolutions is impaired to such a degree so that Kuhn has lost the insight of viewing science on its evolutional bases. Namely, the historical developmental dimension in Kuhn's 1962 approach has been taken out or otherwise reduced to the dimension of education by the same author of scientific revolutions.[219] No wonder that Kuhn himself in 1982 declared that

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[217] D. Shapere, "The Paradigm Concept", *Science*, Vol. 172, No. 3984, 14 May 1971, p.708.

[218] It also effects the essential tension between his two distinctive styles of scientific thinking in 1959, style of divergence and style of convergence.

[219] Even Musgrave exclaims: "In his recent writings, then, Kuhn disowns most of the challenging ideas ascribed to him by his critics."(PR, p.51)

"if I were now rewriting *The Structure of Scientific Revolutions*, I would emphasize language change more and the normal/revolutionary distinctions less".[220] And the only correction should be "since 1969" instead of "now".

All in all, it is Kuhn himself who once won world fame by his synthetically conceiving science through its evolutional perspective, and who now overthrows his own verdict of historical investigation in favour of the nature of scientific education, especially of the relation between linguistics and education. But, even in this dimension, as Kuhn emphasizes, learning the similarity relationship should be linguistically based on the business of a scientific community. In this, when the sociological and historical conditions are taken into account, it reveals, Kuhn seems not aware of, another nature of science, learning the family resemblance in history. This is the aspect I want to draw in order to further spell out the limit of Kuhn's educational and linguistic thrusts.

It should be obvious for Kuhn that not only the scientific community, as Kuhn later argues, accounts for the "special sort of change involving a certain sort of reconstruction of group commitments"(SSRE, p.181), but also the entire social surroundings, as Fleck states, expounds why we reconstruct a special change in certain sort of way.[221] For example, in the Johnny case, the child

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[220] *PSA 1982*, Vol.2, p.715.

[221] Kuhn's 1957 book, *The Copernican Revolution*, is actually typical of this sort. For he stressed that the combination of science and the whole intellectual atmosphere provides the answer

is not just learning things from his father, but also in most of the cases, he is unconsciously or consciously nurtured in the society in which he lives, which makes him acquire all sorts of gifts to reconstruct the world. Of this process, education merely plays a limited role in acquiring the ability of learning the similarity relationship. For, in one's life, the process is actually initiated not only earlier than one's experience of education, but also much more comprehensively than the latter. That is, a scientist is first of all a member of a community in a certain society and then one in a scientific community. In consequence, his learning aptitude has already been nurtured due to his being in a society before he can be instructed by a masters in a scientific community. Such is the story that Kuhn once brilliantly depicts in the combination of science and intellectual history about the Copernican Revolution.[222]

Edification of this kind, I thus conceive, ought to be understood in the sense of Gadamer's notion of ***Bildung***.[223] *Bildung*

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why science in certain time in history is in certain sort of way.

[222] In this, I maintain that Kuhn's 1957 book, *The Copernican Revolution*, is worth a revisit. For it is penetrating in signifying a simple truth that "though the technical scientific materials are essential, they scarcely begin to function until placed in a historical or philosophical framework where they illuminate the way in which science develops, the nature of science's authority, and the manner in which science affects human life."

[223] Cf. H. G. Gadamer, *Truth and Method*, The Seabury Press, 1975, pp.10-19. Setting the notion as the primary concept of his entire system, Gadamer considers that it is the notion that reminds us that "key concepts and words with which we customarily operate acquired their definition then, and if we are not to accept language automatically, but to strive for a reasoned historical self-understanding, we must face a whole host of questions of

is on the one hand the first 'leading humanistic concept' that answers the question how we form our knowledge of the world when we have neither paradigms nor exemplars of the world whatsoever. Therefore, the notion of *Bildung*, on the other hand, by no means aspires after, in a certain sense, the Platonic tradition that man carries in his soul the forms of the world which must be cultivated in himself. *Bildung* simply highlights, Gadamer considers, the now ignored one of western philosophical traditions, i.e. the idea of self-formation or cultivation in the practice of our knowing the world.[224]

First of all, this process is significant in that *Bildung* is intimately associated with the idea of culture and designates primarily the proper human way of developing one's natural talents and capacities. Secondly, in terms of anthropology, the process appears one that the self-awareness of working consciousness contains all the elements that make up practical *Bildung*. Thirdly, *Bildung* is implied in the way of practice, "the frequent carry-over from becoming to being."(TM, p.12) Namely, *Bildung*, as a process of 'formation', describes more the result of this process of becoming than the process itself. Thus the cultivation of a talent is the development of something that is given, so that the practice and cultivation of it is a mere means to an end. Fourthly, *Bildung* is

verbal and conceptual history."(ibid., pp.10-11)

[224] In the Hegelian tradition, Gadamer indicates, a truly free self-consciousness 'in-and-for-itself' is "the essence of work to form rather to consume a thing."(TM, p.13) Formation in this sense is thus nurturing or cultivating oneself.

as the most cultivated cognitive process that should be accounted as a process of human reasoning. For the "carry-over from becoming to being" is especially clear here because the result of *Bildung* is not achieved in the manner of a technical construction, but grows out of the inner process of formation and cultivation and therefore remains in a constant state of further continued cultivation. And last, but not the least, *Bildung* is a process of historical formation accidentally happened in history. That is, *Bildung* is a genuine historical idea, and because of this historical character of preservation, it is important for understanding in human sciences.

In a nutshell, our memory is formed through our deeds in the process of ***Bildung***, the combination of practice and living in history. It is human beings who form or create, using Kuhn's term, exemplars as well as frameworks to be learned. Whereas what later Kuhn seeks in the scientific education is only the restricted moment when exemplars have been formed. And it is right from this point that Kuhn betrays the approach he adopted in *The Copernican Revolution* and *The Structure of Scientific Revolutions*.

What Kuhn betrays, in particular, is exactly what he once aspired in 1956, the view that "scientific concepts are ideas, and as such they are the subject of intellectual history."(CR, p.vii) Not only this, Kuhn also comprehends that science itself is the part of a much larger and more general historical formations, the process of *Bildung*:

> though the technical scientific materials are essential,

> they scarcely begin to function until placed in a historical or philosophical framework where they illuminate the way in which science develops, the nature of science's authority, and the manner in which science affects human life.(CR, p.ix)

But Kuhn never discerns the essential role of edification or Bildung to scientific epistemology. In this aspect, he is not alone in Anglo-American philosophy. For example, even Richard Rorty, who first introduced the notion of *Bildung* into contemporary Anglo-American philosophy as 'edification' in his *Philosophy and the Mirror of Nature*, did not quite notice the connotative distinctions between education and edification. For Rorty chooses 'edification' instead of 'education' as the English equivalent of German '*Bildung*' only because the latter "sounds a bit too flat".[225] Whereas, I conceive, this issue is perhaps more of the Anglo-Saxon philosophical as well as cultural traditions than of the semantics.

Etymologically, it is interesting to trace out the fact that the contemporary English verb 'build' actually used to be 'bilden' as early as in the period of Middle English;[226] this form is exactly the contemporary German verb 'bilden'. And surprisingly the English verb 'bilden' originally denoted almost the same

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[225] Cf. R. Rorty, *Philosophy and the Mirror of Nature*, Princeton University Press, 1979, p.360. And his understanding the notion of 'edification' is in the sense that it stands for "this project of finding new, better, more interesting, more fruitful ways of speaking" which attempts to edify either ourselves or others.

[226] Cf. *Webster's Third New International Dictionary of the English Language Unabridged*, G.& C. Merriam Company, Publishers, 1969, p.291.

connotations which the contemporary German 'bilden' implies.[227] But, the English word 'build' has been somewhat "materially" developed that contemporary philosophers seldom employ it as a philosophical terminology.

However, the English verb 'build' still has, according to its dictionary definitions, more implications of what the German noun '*Bildung*' implies. For its sense building-something-up is in one side establishing something only by the activity of building itself, and in the other, building-up is increasing something in the course of progressing toward a maximum. Only in this, the English combination of 'building-up' and 'edification' might be closer to what Gadamer seeks in the German notion of *Bildung*. For *Bildung* is not only of enlightenment and improvement of our mental situation, it is also of the course and progress of the situation. And it is only in this course that people are cultivated themselves and thus acquire the knowledge of how to form similarity relationships which are similar things only similar to each other and which, as such, need not be asked the question "Similar with respects to what?"

It is telling here that, according to the above etymological analysis, there exist incommensurability between at least the Anglo-American way of thinking and the European way of discourse. Etymologically speaking, the former either lost a certain way of

[227] Cf. *Middle English Dictionary*, Vol.I, (ed.) by Hans Kurath, University of Michigan Press, 1956, pp.849-851. Also *The Oxford Harrap Standard German-English Dictionary*, Vol.I, (ed.) by Trevor Jones, Clarendon Press, p.88.

its thinking tradition or ignores the once-in-a-while tradition due to its "unanalyzed" character. Nevertheless, it is barely true that there are different styles of comprehending between these two traditions for which are not easily accounted in terms of translation. As Ludwik Fleck points out,

> A stylistic bond exists between many, if not all, concepts of a period, based on their mutual influence. We can therefore speak of a thought style which determines the formulation of every concept. History shows that violent arguments can rage over the definition of concepts. This demonstrates quite independently of any utilitarian reasons just how little such conventions, which from the point of view of logic may seem equally possible, are in fact felt to be of equal value.[228]

Even Kuhn and Rorty, among the most synthetic philosophers in contemporary Anglo-American tradition, cannot quite understand, synthesize, and thus reinforce the notion of Bildung in its German connotation since they are never edified in that tradition.

As the verb 'bilden' denotes, formation itself is cultivation as well as dwelling things on a certain location, i.e. "to construct for a dwelling".[229] As Gadamer insightfully points out, "memory must be formed; for memory is not memory for anything and everything."(TM, p.16)[230] Given the fact that a race gives itself

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[228] Ludwik Fleck, *Genesis and Development of a Scientific Fact*, (ed.) by T. J. Trenn, The University of Chicago Press, 1979, p.9. I shall detail Fleck's notion of 'style' in next section.

[229] Cf. *Webster's Third New International Dictionary*, p.291, the sense number 1.a.

[230] It is as obvious as the reality that the memory of any computer system ought to be first formatted by certain DOS before it can do something for human beings.

its existence in its world, it works out from itself and thus externalizes what it is in itself. Kuhn and Rorty, of course, were both basically brought up in their own culture, and naturally understand things according to their upbringing. Apparently, the fact that understanding itself is culturally bonded proves not only that education will be regionally limited but also that: so is the edification itself. The fact that in the vocabulary of contemporary English there is no exact equivalent for the German term *Bildung* attests to the fact that incommensurability is not simply a linguistic problem. Rather, the example reveals that thinkers in a nation can never employ alien concept properly as it is if they have no experience either being edified within that culture or being thus edified in their own culture. No one can break through the bondage of one's mother tongue if one has no recourse to foreign languages and experience to exploit them. Understanding between cultures stops here; incommensurability appears.

Due to this, education in one region of course cannot omnipresently replace one in the other. For even *Bildung* itself is limited in terms of culture, history and particular "dwelling". In light of this, we ought to further notice that edification, in Rorty's sense, does not amount to education since the former is settled in the context of particular living or being whereas the latter is always an activity which tries to teach the universal.

As Edmund Husserl defines his notion of the life-world,

> It is this world that we find to be the world of all known and unknown realities. To it, the world of actually experiencing intuition, belongs the form of space-time

> together with all the bodily shapes incorporated in it; it is in this world that we ourselves live, in accord with our bodily, personal way of being.[231]

For it is the life-world we are settled that answers the question "Similar with respect to what?". Also as Husserl holds,

> In geometrical and natural-scientific mathematization, in the open infinity of possible experience, we measure the life-world -- the world constantly given to us as actual in our concrete world-life -- for a well-fitting *garb of ideas, that of the so-called objectively scientific truths*.[232]

Only understanding all these dissimilarities between *Bildung*, edification, building-up, and education, can we better realize why there exist different styles even in sciences, and what is more, why those styles cannot be simply taught or introduced by tutors.

Styles can never be taught. However, due to the fact that the issue of styles cover plenty of areas other than natural sciences, I shall only focus on the issue of scientific styles concerning the topic in this essay, especially of its 'thought style' as Fleck defined, in order to further disclose the essential role of *Bildung* in scientific epistemology.

As we discussed above, edification or building-up is based on one's or a community's whole experience in a certain life-world. Therefore, as Gadamer notes, scientific epistemology cannot be

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[231] Edmund Husserl, *The Crisis of European Sciences and Transcendental Phenomenology*, Northwestern University Press, 1970, p.50.

[232] Ibid., p.51.

separated from sciences of humanities since

> the human sciences presuppose that the scientific consciousness is already formed and for that very reason possesses the right, unlearnable and inimitable tact that bears the judgement and the mode of knowledge of the human sciences.(TM, p.15)

In this, *Bildung* has the function to yield ***tact*** which cannot be fully explained or analyzed within the framework of scientific education. It is true that every scientist occupies at least two communities, one in the sense of Kuhn's 'scientific community', the other in the sense of sociological community. And, as we have investigated above, a scientist's social being can edify himself to such a degree just as Kuhn once emphasized in 1956.

> Copernicus himself was a specialist, a mathematical astronomer concerned to correct the esoteric techniques used in computing tables of planetary position. But the direction of his research was often determined by developments quite foreign to astronomy. Among these were medieval changes in the analysis of falling stones, the renaissance revival of an ancient mystical philosophy which saw the sun as the image of God, and the Atlantic voyages which widened the terrestrial horizons of Renaissance man. ... Creative interdisciplinary ties like these play many and varied roles in the Copernican Revolution.(CR, p.vii.)

Cases of this kind show that the 'thought-style' of a scientific community is formed in its social being which nurtures the community to progress itself to its maximum. As Fleck insists,

> The explanation given to any relation can survive and develop within a given society only if this explanation is stylized in conformity with the prevailing thought style.(GDS, p.2)

Obviously, these historical, social, and cultural aspects of *Bildung* are the basis of any education. For the education itself only exploits and stands on the edified.

Even within a scientific community, scientific education provides limited means to establish community-style which is something fixed and objective and binding on individual forms of expression. In other words, style is something one has to acquire through *Bildung* while being edified inside one community rather than through something that one can ostensively imitates in way of being taught by a teacher. Thus as a certain way of thinking, not just of expression, style as the result of *Bildung* is something unique for a scientific community, and cannot be asked by the question: "Similar with respect to what?". Style like 'paradigms' or 'exemplars' is in nature cultivated along *the* way of building-up a scientific community. In this, style more represents those elements that are ***unlearnable and inimitable,*** i.e. personal or communitive ***tact*** that bear the judgment and mode acquired by doing science. It thus can be regarded as a mature scientific community only until it creates a style and, unlike what Kuhn requires, it is no longer merely engaged in imitation or education by learning exemplars. Grasping a certain style of a scientific community, of course, is requiring, unlike Kuhn's learning similarity relationships, scientist to gain the ability of operating science as well as being edified within the community. Because, a 'tact' of doing science cannot be showed to others in most of the case, even in the process of learning exemplars.

> By 'tact' we understand a particular sensitivity and sensitiveness to situations, and how to behave in them, for which we cannot find any knowledge from general principles. Hence an essential part of tact is inexplicitness and inexpressibility. One can say something tactfully; but that will always mean that one passes over something tactfully and leaves it unsaid, and it is tactless to express what one can only pass over. (TM, p.17)

Or as Fleck observes, "*The fact must be expressed in the style of thought collective.*" (GDS, p.102) For the collectives as comparatively stable communities, Fleck remarks, "cultivate a certain exclusiveness both formally and in content." (GDS, p.103) That is, the process of cultivation is the course of scientific *Bildung* in Fleck's "thought collective"; the cultivated exclusiveness becomes the identical thought-style of the community. "But even more important is the restricted content of every thought collective", as Fleck emphasizes, "as a special realm of thinking." (GDS, p.103-104) For it is in this sense, in my opinion, that Fleck highlight the aim of *Bildung*:

> *All paths toward a positive, fruitful epistemology lead toward the concept of thought style*, the varieties of which are mutually comparable and can each be investigated as a result of historical development. (GDS, p.99)

As such, thought-style, a result of historical development or *Bildung* of the thought collective, also consists of certain mood and of the performance by which the collective is realized. And a mood has two closely connected aspects, readiness both for selective feeling and for correspondingly directed action. In consequence, this mood creates the expressions appropriate to it depending in each case on the prevalence of certain collective motives and the collective means applied. With this knowledge of thought-style, Fleck defines 'thought style' as

> [the readiness for] *directed perception, with corresponding mental and objective assimilation of what has been so perceived.* (GDS, p.99)

With "what has been so perceived", Fleck means assimilation in historical perspective, an event in the history of thought." (GDS, p.100) That is, the thought-style of a scientific community is the important area of science in that it hardly has direct connection with what Kuhn seeks in exemplar education. The only way to acquire a thought style is undergoing *Bildung* in which education is only playing a small part.

In Fleck's view of thought style,

> Every didactic introduction is therefore literally a "leading into" or a gentle constraint. The history of science is pedagogically helpful, because long-established concepts have the advantage of less thought specialization and are therefore more easily understood by the novice. ... The initiation into any thought style, which also includes the introduction to science, is epistemologically analogous to the initiations we know from ethnology and history of civilization. Their effect is not merely formal. ... Such is the result of the assimilation of a thought style. (GDS, p.104)

No wonder fourteen years after SSR appeared and seven years after his three attempts were laid out, Kuhn in 1976 reluctantly drew his connection to Fleck's approach in his special "Foreword" to the English translation of Fleck's German book *Entstehung und Entwicklung einer Wissenschaftlichen Tatasache: Einfuhrung in die*

Lehre vom Denkstil und Denkkollektiv:[233]

> But I am not sure that I took anything much more concrete from Fleck's work, though I obviously may and undoubtedly should have. (GDS, p.ix.)

What he can remember is likely Fleck's remarks on the difficulties of transmitting ideas between two "thought collectives",[234] i.e. the possibilities and limitations of participation in several "thought communities". However, in 1976, Kuhn reconsiders that a thought collective in Fleck's perspective is not only "a hypostatized fiction" but also "intrinsically misleading and a source of recurrent tensions in Fleck's text" (GDS, p.x).[235]

It seems to me, Kuhn's critique towards Fleck's 'thought collectives' cannot make the special nature of science, education, more telling. For education itself is supposed to create, as Kuhn reinforces in his discussion of 'normal science', a rigid and convergent working atmosphere for a scientific community. Thus education has not yet provided the answer for why Fleck is wrong.

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[233] This book is originally published in 1935 by Benno Schwake & Co., Basel, Switzerland.

[234] Cf.GDS, Chapter 4, section 3.

[235] To clear the line concerning the 1976 difference between Kuhn and Fleck, of course, needs more space, perhaps another essay. However, what we can manage here is pointing out the fact that Kuhn has attempted to disown any relation of his 'paradigm' in 1962 to Fleck's notion of "thought style". It is enough to notice, as a matter of fact, that Kuhn in 1976 regards it problematic for Fleck to view that "the effects of participation in a thought collective are somehow categorical or a priori"(GDS, p.xi), while he himself starts putting scientific authority into the nature of education.

As a matter of fact, Fleck has been standing on a much richer background of historical development, or *Bildung*, which Kuhn's eduction thrust hardly draws.

So, to the topic of the present chapter, I conclude that the limitation of education ought to be spelled out as follows:

1. Kuhn's Johnny case, one of education between father and son, as well as Wittgenstein's educational cases are not the typical examples of education. Cases of this kind should be limited only in the very primary stage in the process of education.

2. Education as what Kuhn describes, learning similarity relationship under the guidance of exemplars, is literally answering the Kuhnian question: "Similar with respect to what?". And this is the typical characteristic of education, especially scientific education.

3. Learning similarity relationship is, as Kuhn realizes, central to establishing scientific enterprises, but it is by no means in the sense that Kuhn seeks solely in education.

4. Another function of learning similarity relationship, the process of reconstruction, is more essential in that it can be acquired without Kuhn's educational "exemplars". This process provides the negative reply to the Kuhnian question "Similar with respects to what?"

5. Gadamer's hermeneutic approach highlights the primary source of learning similarity relationship, i.e. ***Bildung.*** This concept further defines the limitation of education.

6. The concept of ***Bildung*** is not quite familiar in the community of Anglo-American philosophy for several reasons. Among them, the etymological research is helpful and illuminating. This reveals that *Bildung* itself needs certain dwelling, i.e. to live, as Husserl insists, in a certain living world which provides the material we learn as well as the way we think and behave.

7. Style as one of the significant results of *Bildung* cannot be accounted merely by education. It is history that provides opportunities to edify different styles.

8. It is *Bildung* and the style which the former creates that sustain the phenomenon of incommensurability. For this phenomenon is in nature of certain history, society and culture, and thus becomes an ethnological topic.

9. In this, the philosophical concept of incommensurability is basically a practical claim for the phenomenon. Thus the conflict between pro and against the notion, therefore, is not in any sense of philosophical analysis but acknowledgement of hermeneutic approach of scientific history as Kuhn once excellently did in his 1956 book, *The Copernican Revolution.*

10. In one word, Kuhn's account of education rather evidences that the universality and priority of education has never existed. For even its exemplars are, as Kuhn clearly notices, only of particulars. So, in particular, they are particulars in histories, societies, and cultures. In light of this, in turn, the phenomenon of incommensurability cannot be dissolved merely by scientific education.

12

The Value of Belief

--- *Incommensurability Revisited IV*

The conception of ***belief*** is a cornerstone to understand incommensurability. When scientists conduct their enterprise, they accept a certain set of presuppositions and use them as their interpretative framework, which ascribes value, imputes meaning, assigns import, and forms the premises of science. This is the scientific belief that recommends the kind of conceptions and relations that should be upheld as plausible and that rejects some conceptions and evidences as unlikely. In a word, a belief shape our anticipations and valuations which cannot be readily explained on other grounds; it consists of such general views and purposes implicit in the achievement and establishment of a scientific discovery that it becomes an essential element in the pursuit of scientific enquiries. Without the element, science cannot be conducted for constructing facts. For, in this, we have no valuations to construct or form facts as the intelligible.

And the issue of *Bildung*, the process in which one forms a way of understanding the world, the belief, in a certain history and its particular development, is closely related to the indispensable concept of Kuhn's approach, ***belief***. It is the concept that Kuhn at one time insisted to hold as one of the essential elements of a paradigm of a science.

The element remains controversial, however, among the community of contemporary philosophy, especially of philosophy of science. In this last chapter concerning Kuhn's notion of incommensurability, I shall detail, using insights from Dewey and Polanyi, my view to the issue of beliefs specially with its indispensable connection to the notion of incommensurability.

As John Dewey once remarked in 1906,

> the professional man, the philosopher, has been largely occupied in a systematic effort to discredit the standpoint of the common man, that is, to disable belief as an ultimately valid principle. Philosophy is shocked at the frank, almost brutal, evocation of beliefs by and in natural existence, like witches out of desert heath-at a mode of production which is neither logical, nor

physical, nor psychological, but just natural, empirical.[236]

It is still almost in the same situation. That is, contemporary philosophers of science were shocked and their beliefs were challenged when Kuhn's SSR appeared in 1960s, and especially when he stated that,

> No natural history can be interpreted in the absence of at least some implicit body of intertwined theoretical and methodological belief that permits selection, evaluation, and criticism. If that body of belief is not already implicit in the collection of facts -- in which case more than "mere facts" are at hand -- it must be externally supplied, perhaps by a current metaphysics, by another science, or by personal and historical accident. (SSR, p.17)

As a matter of fact, Kuhn's career in philosophy of science starts from recognizing the importance of belief in general. As early as in 1956 in the *Preface* to his first book, *The Copernican Revolution*, Kuhn pointed out the importance of belief in his research of history and philosophy of science:

> The combination of science and intellectual history is, however, essential in approaching the plural structure of the Copernican Revolution. ... Planetary astronomy was never a totally independent pursuit with its own immutable standards of accuracy, adequacy, and proof. Astronomers were trained in other sciences as well, and they were committed to various philosophical and religious systems. Many of their nonastronomical beliefs were fundamental first in postponing and then in shaping the Copernican Revolution. These nonastronomical beliefs compose my "intellectual history" component. (CR, p.viii)

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[236] John Dewey, "Beliefs and Existences", *The Middle Works: 1899-1924*, Vol.3 (1903-1906), (ed.) by Jo Ann Boydston, Southern Illinois University Press, Feffer & Simons, Inc., 1977, p.85.

For their part, however, Kuhn's critics have argued that the line of scientific belief of a paradigm in SSR is one of the vicious problems that makes his approach extremely mysterious as well as ambiguous. And they attempt to convince Kuhn that he should disown the view of belief in SSR in order to save the real "strengths" of the work.[237]

Accordingly, Kuhn started minimizing his view of belief since 1969.[238] In his updated view of two senses of 'paradigm', he then declared that the second sense of paradigm, 'the concrete puzzle-solutions' or 'exemplars', is deeper than the first sense of it, "the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community."(SSRE, P.175) It is the exemplar, Kuhn now maintained, that stands for the entire constellation.

But the problem of his new interpretation is, Shapere complains, that after that "Kuhn never adequately clarified how the remaining factors covered by that term [paradigm] were related to (embodied in) the concrete examples in such a way that the whole outlook [paradigm in the broader sense] of the tradition would be conveyed to students though such examples".[239] In other words, the

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[237] Cf. D. Shapere's "The Structure of Scientific Revolutions" (*The Philosophical Review*, Vol.73, 1964) and "The Paradigm Concept" (*Science*, Vol.172, 1971). Also cf. F. Suppe's argument on Kuhn's tendency of "irrationality" in *The Structure of Scientific Theories* (University of Illinois Press, 1977, pp.643-649).

[238] Cf. SSRE, pp.174-176.

[239] D. Shapere, "The Paradigm Concept", *Science*, Vol. 172, No. 3984, May 14, 1971, p.707.

original notion of belief in SSR has been largely moderated not only as attachment to exemplar, but also disconnected from it to such a degree that "the collection of facts" hardly has any relation to the body of belief in its original sense of "metaphysical paradigms" in 1962.

But Shapere does not notice, in fact, that in 1969 Kuhn no longer had any intention to retain the broader 1962 sense of "paradigm". That is, Kuhn refers to the first sense of paradigm, the constellation of beliefs, values, and so on, but he does not want to tackle the issue of how the whole outlook of the tradition is conveyed to students through examples. Rather, in his new attempts he seeks whatever means to water down the "mysterious" and "irrational" metaphysical elements such as the notion of belief. In fact, he tries to give up his primary commitment to 'metaphysical paradigms' or 'the metaphysical parts of paradigms' that appeared in SSR. Not only this, Kuhn says that what was in his mind about belief in 1962 was factually "shared commitments to such beliefs as: heat is the kinetic energy of the constituent parts of bodies; all perceptible phenomena are due to the interaction of qualitatively neutral atoms in the void, or alternatively, to matter and force, of to fields."(SSRE, p.184) In particular, Kuhn declares,

> Rewriting the book now I would describe such commitments as beliefs in particular models, and I would expand the category models to include also the relatively heuristic variety: the electric circuit may be regarded as a steady-state hydrodynamic system; the molecules of a gas behave like tiny elastic billiard balls in random

motion.(SSRE, p.184)[240]

In this, he simply changes the meaning of what he meant by "belief" in SSR. That is, as other remaining factors in the first sense of a paradigm have been defined as inferior to exemplars, the notion of "belief" is minimized in the second sense (1969) of a paradigm, and changed from its original metaphysical meaning. In consequence, a "belief" in 1962 has become a trivial addition to a paradigm, a model for concrete exemplars.

In this new explanation of 'belief' as models, Kuhn defines the concept of belief as that "along the spectrum from heuristic to ontological models, all models have similar functions."(SSRE, p.184)[241] It is the "belief spectrum" of this kind, Kuhn notes, that supplies the scientific community with preferred or permissible analogies and metaphors and thereby helps "to determine what will be accepted as an explanation and as a puzzle-solution."(SSRE, p.184)

It seems on the surface that Kuhn seeks to invite the original sense of metaphysical beliefs back to the scene with the idea of "the spectrum of models". But he immediately changes his tone by

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[240] In terms of his commitment to this direction, what Kuhn has done with the notion of beliefs is contrary to what Shapere complains. For Kuhn in fact has otherwise clarified how the remaining factors are related to his new sense of paradigm.

[241] I find this sentence opaque, especially with the relation between 'ontological models' and 'heuristic models'. I do not see why Kuhn differentiates heuristic models from ontological ones while assigning them "similar functions". For me, it is hard to accept his new sort of belief that "heat is the kinetic energy of the constituent parts of bodies" either as 'ontological models' or as 'heuristic models'.

saying that "the members of scientific communities may not have to share even heuristic models, though they usually do so."(SSRE, p.184) Because, he insists that, for example, "membership in the community of chemists during the first of the nineteenth century did not ***demand*** a belief in atoms".[242]

With this revision, Kuhn has completely disowned his vision of scientific belief in 1960s. For him from 1956 to 1962, the whole point of "belief" is not whether somebody is demanding a belief or not, but of where and how it is actually shaped: a scientific belief is not a thing that can be demanded as religion demands but rather a way of transition of one's sense of values.[243] The entire issue is, therefore, not that the chemists during the first of the nineteenth century did not demand a belief in atoms, but that they perhaps, unfortunately, had been in the position to acquire a belief of atoms.[244]

One cannot merely demand whatever beliefs without first living in a certain time in history, as I have discussed in the last chapter. Or, in other words, to believe is not at all an issue of metaphysics or of demanding, but of being in nature as well as of experiencing in nature. As I argued in Chapter 11, beliefs are those mental elements valued and simultaneously fermented or shaped

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[242] My italics. But the fact that "they did not demand a belief in atoms" does not mean that they did not have any belief at all.

[243] Cf. CR, p.2.

[244] I shall detail this point later with a case study to support the argument.

in the process of *Bildung*, no matter whether people subjectively demand them or not.

However, given that the concept of belief occupies a central position in understanding incommensurability as well as in communicating with mainstream philosophers who regard the conception of belief as a mysterious and irrational element in the process of cognition, Kuhn's stripping belief in atoms of its metaphysical dimensions cannot be left unchallenged.[245] To this end, it is worth to first recall the real event of how chemists built up their belief in atoms during the first half of the nineteenth century. And along the way of my discussion, I shall draw the characteristics of the notion of belief step by step.[246]

According to the history of modern chemistry, the reason that chemists during the first half of the nineteenth century did not 'demand' a belief in atoms is, contrary to what Kuhn alleged, that they had already had a metaphysical belief in atoms for a long time; their belief merely needed to be further quantified in terms of science. This story is typically represented by the development of modern chemistry in Britain, especially by the personal history of the father of modern chemistry, John Dalton.

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[245] Some of them simply treat the notion as diabolic commitment which should be analyzed out of the business of philosophy for ever. So, I consider that my undertaking of reconsidering the notion of incommensurability ought to first face the challenge both from the main-stream philosophers and the later Kuhn.

[246] I shall put my arguments along the way of my discussion and, also, summarize them at the end of the chapter.

The real history shows that it is true that the Manchester provincial science teacher, John Dalton, did not demand a 'belief' in atoms when he discovered the relative weight of atoms. But this does not mean that he had not acquired any belief before. On the contrary, for a long time, Dalton had been immersed with a hypothesis, a scientific belief, about atoms, i.e., Newton's doctrine of atoms.[247] Also, it is known that this popular belief in atoms was not the property of Newton; its origin was from as early as the Hellenic times.[248] In his notebook, Dalton quoted Newton,

> It seems probable to me that God in the beginning formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures, and with such other properties, and in such proportion, as most conduced to the end for which He formed them; and that these primitive particles, being solids, are incomparably harder than any porous bodies compounded of them; even so very hard as never to wear or break in pieces, no ordinary power being able to divide what God Himself made one in the first creation. While the particles continue entire they may compose bodies of one and the same nature and texture in all ages; but should they wear away, or break in pieces, the nature of things depending on them would be changed. Water and earth composed of old worn particles and fragments would not be of the same nature and texture with water and earth composed of entire particles in the beginning. And therefore, that nature may be lasting, the changes of corporeal things are to be placed only in the various situations, and new associations and motions of these permanent particles,

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[247] It is always interesting for me to wonder how a great scientist such as Newton conducts revolutions of science while feigning "no hypothesis; for whatever is not deduced from the phenomena is to be called an hypothesis; and hypothesis, whether metaphysical, have no place in experimental philosophy." (Cf. I. Newton, *Mathematical Principles of Natural Philosophy*, (ed.) by F. Cajori, Berkeley: University of California Press, 1934, p.547.)

[248] As a matter of fact, the atomic view was held not only by the Greek and Roman philosophers but also by the wise men of the East in Egypt and India.

compound bodies being apt to break, not in the midst of solid particles, but where those particles are laid together and only touch at a few points. . . . God is able to create particles of matter of several sizes and figures, and in several proportions to the space they occupy, and perhaps of different densities and forces. . . . At least, I see nothing of contradiction in all this. . . . Now, by the help of these principles, all material things seem to have been composed of the hard and solid particles above mentioned -- variously associated, in the first creation, by the counsel of an intelligent agent.[249]

In consequence, as the history of chemistry shows, such views of corpuscles had long been the current belief among scientists in the nineteenth century. The belief was popular at that time, as Dalton observes,

> These observations have tacitly led to the conclusion which seems universally adopted, that all bodies of sensible magnitude, whether solid or liquid, are constituted of a vast number of extremely small particles bound together by a force of attraction.[250]

According to his syllabus of the seventeenth lecture, although Dalton was in the habit, from his early days, of observing things from an atomic point of view, he once wondered why Newton had demonstrated so clearly in the 23rd Proposition of Book II of his *Principia*, namely that an elastic fluid is constituted of small particles or atoms of matter which repel each other by a force

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[249] Quoted from Henry E. Roscoe's *John Dalton and the Rise of Modern Chemistry*, Macmillan and Co., 1895, pp.128-129. Apparently, the quotation defies Kuhn's new definition of beliefs as models which have no metaphysical truth values.

[250] Ibid., p.129-130. In a word, the belief of atoms had been so popular that even Dalton was puzzled by the phenomenon.

increasing in proportion as their distance diminishes.[251] It seems to Dalton, however, a problem

> how this proposition of Newton's would apply to a case of which he, of course, could have no idea.[252]

So, contrary to what Kuhn alleged in 1969, scientists such as Newton and Dalton believed that "homogeneous elastic fluids are constituted of *particles*."[253] That is why nobody 'demanded' a belief in atoms at that time, and in fact nobody even 'demanded' a belief in atoms of modern chemistry at all. For, as I have pointed out, the belief in atoms as a metaphysical presupposition had been tacitly held among the scientific community for a long time, which is apparently contrary to Kuhn's 1969 definition of 'belief' only as concrete models.

My argument then is that the process of *Bildung* that shows that before valuing the world one has already lived in a *Dasein* of the world in which there exist particular history, culture, society, community, as well as special metaphysical belief etc. that one has been predetermined and therefore has not much choice. Moreover, this way of acquiring a belief has less to do with one's

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[251] Cf. ibid., p.132.

[252] Ibid.

[253] Cf. ibid. p.130. According to Dalton, although Newton conceived that an elastic fluid is constituted of small particles as we mentioned above, modern discoveries had ascertained that the atmosphere contains three or more elastic fluids of different specific gravities (cf. ibid., p.132). This idea occurred to Dalton in 1803, for he found that the *size* of the particles of elastic fluids must be different (cf. ibid., p.134).

later education, although the latter is also in the process of conventionally building up certain kind of belief in the times.

The reason that nobody even 'demanded' a belief in atoms of modern chemistry is because of the fact that even the father of modern chemistry, Dalton, did not plan to set up a new model as something to be believed for the upcoming chemistry. As a matter of fact, Dalton, the haphazardly educated Manchester teacher, originally focused on something else, the constitution of mixed gases, a view to the science of weather -- meteorology.[254] However, in his endeavour to obtain properties of gases, Dalton's mind was already occupied with the atomic conception that gases are composed of definite particles. It was through his experiments with these gases that he unexpectedly arrived at the results *of the Table of the Relation Weights of Ultimate Particles of Gaseous and other Bodies* in October 21st, 1803.[255] And it is this recognition of *relative weights* as the great foundation stone in chemical science that finally paved the way to his world-known Atomic Theory, a new system of atoms based on the old which "first explained the facts of chemical combination by a theory which has stood the test of time."[256]

In light of this, we can see that only through the process of

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[254] It emerged in the 18th century with studies of the behaviour of air and water vapour etc.

[255] Cf. ibid., p.106-107. What Dalton experienced was quite similar to Kuhn's enlightenment in 1947, an illumination of relative weights of certain particles.

[256] Ibid., p.129.

Bildung is not enough to build up a scientific belief. For a new belief, a scientist has to encounter, struggle, and run through enlightenment, the process of *Aufheben*, in his practice of scientific experiments. Also, Dalton's discovery of atomic theory convinces me that the process of *Aufheben* consists of gradual and sudden mental enlightenments which transcend one's personal experience to perceptions of objects as truth in a time.[257]

Not only the discovery of relative weight of atoms attests to the function of belief that can orient Dalton's research beforehand. In the connection to our current discussion, Dalton's insight was also penetrating in that, in the process of his investigation of atoms, he recognized a phenomenon of belief which is closely related to what we are concerned here as incommensurability:

> From the preceding remarks it will be perceived that I advanced thus far in my theoretic progress without meeting with much obstruction. The way had been paved by others. But when I directed my view to the compounds of charcoal and oxygen, and charcoal and hydrogen, I found that all the commonly received doctrines were adverse to my proceeding and ***irreconcilable*** with my views.[258]

In light of Dalton's reflection of the ***irreconcilable***, we can notice that the phenomenon of incommensurability, the one of communication breakdown, occurred during Dalton's revolutionary

[257] I consider that Kuhn's enlightenment in 1947 is a sudden one, comparing Dalton's gradually realization of atoms in chemistry. And I think that this is a philosophical topic that could be accorded more attention in the near future.

[258] Ibid., p.138, italics added.

establishment of modern chemistry. In Dalton's words, the phenomenon is of the irreconcilable views [beliefs] occurred in the process of understanding different doctrines in the nineteenth century.

Also, this kind of irreconcilable phenomenon happened, interestingly, under the same 'belief' of atoms while there existed different models of atoms:[259] a phenomenon of the communication breakdown appeared in the debate between Dalton and Sir Humphry Davy.[260] That is, after Dalton's pioneer work of the Atomic Theory, *A New System of Chemical Philosophy*, appeared in 1808, not every scientist recognized the importance of Dalton's work and adopted it as the great achievement of modern chemistry. For instance, the distinguished member of the British Royal Society, Sir Humphry Davy, who was then full of his own discovery of the decomposition of the alkalies, wrote to Dalton in 1809 that he "doubts whether we have yet obtained any elements",[261] although he was glad to be informed about Dalton's new view of the atomic system. In another letter to Dalton on May 25th, 1810, Davy further remarked,

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[259] Cf. ibid., pp.138-139. This case can sustain what Kuhn projected in his fourth reason for granting paradigms a status prior to that of shared rules in the section of "The Priority of Paradigms" in SSR, a status that "even the discovery of a new and unexpected phenomenon may be revolutionary"(SSR, p.49) and thus consists of an incommensurable paradigm. But, it is obvious, a detailed discussion of the topic is already beyond the intension of this essay.

[260] In this connection, I conceive that incommensurability is not only associated with different beliefs, but also occurred in the situation when there appears development of a belief or belief changes.

[261] Ibid., p.155.

> I shall be sorry if you introduce into your rising system an hypotheses which cannot last concerning the alkaline metals.[262]

And Davy continued his charge and objects in 1811:

> I shall enter no further at present into an examination of the opinions, results, and conclusions of my learned friend; I am, however,obliged to dissent from most of them, and to protest against the interpretations that he has been pleased to make of my experiments.[263]

However, this irreconcilable combat, the communication breakdown between Dalton and Davy, seems to be brought to an end not in terms of reconciliation but of conversion. In 1826, as the president of the Royal Society, Davy presented Dalton the Royal Medal and spoke in the warmest words of Dalton's achievement and of his service to science. Thus the battle of belief was over; the communication breakdown disappeared. And all chemists of later date acknowledge that Dalton's theory is the foundation stone of modern chemistry. This shows that belief is the union of abstract postulated meaning and concrete brute facts in a way that circumvents the latter by judging them from a new standpoint, one incommensurable with the old, while it tests concepts by using them as methods in the same active experience. So, for example, Davy will not believe Dalton's atomic theory until it all comes to experience personally conducted and personally consummated in his experiments. Only to such a degree can the incommensurable beliefs

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[262] Ibid., p.156.

[263] Ibid.

between Davy and Dalton be settled on the basis of Dalton's scientific belief of atoms. That is, a belief is not only a philosophical notion, but also a practical way which, unlike the convictions of the common man and the hypothesis of sciences, finds its proud proof in the fact that it does not demean itself so unworthily as to work.

To my knowledge, this is the real story about the establishment of atomic belief in the history of modern chemistry, a story that exactly attests to what Kuhn once remarked of scientific beliefs in 1962,

> The more carefully they read, say, Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics, 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 idiosyncrasy 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 be called science, then science has included bodies of belief quite incompatible with the ones we hold today.(SSR, p.2)

In consequence, the cases of Dalton's belief in atoms, his discovery of a new belief of atoms, his realization of the phenomenon of the irreconcilable, and the incommensurable debate between him and Davy in the modern history of chemical science, should stirred the circle of philosophy of science, including the later Kuhn, to rethink the value of belief.

In connection to the above assessment, I also hope that philosophers of science could realize that all debates on Kuhn's approach in 1960s are, in a nutshell, ones of defending beliefs of debaters rather than ones of merely criticizing something from nowhere. In my opinion, the debate is obviously a battle in which every intellectual has his or her own belief in science or scientific views to be defended or controverted.[264]

Only by understanding the nature of the debate in terms of the belief crisis in 1960s, are we able to envision why so many joined the battle pro or against Kuhn's rebelling. In other words, the battle over Kuhn's SSR becomes, as Polanyi reveals, one of 'fiducial commitments' which is "intrinsic to the intellectual and social life of modern man".[265] That is, philosophers of science who are against kuhn's notion of belief not only combat for suppressing Kuhn's rebelling, but also, by doing so, justify their own beliefs, i.e., their "fiducial programme", in order to save their own beliefs they have acquired for a long time.

In general, Dalton's discovery of relative weight of atoms proves that scientific belief is not only a real issue in the history of science but also essential to our theoretical comprehension of science. But most philosophers of science still regard Kuhn's notion of scientific belief as the essential element of his relativist view of the development of science. Shapere

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[264] So it is understandable that when their belief was severely defied, the first thing for them is to face the painful belief conflict for which few of them are prepared.

[265] M. Polanyi, *Personal Knowledge*, Happer Torchbook, 1964, p.ix. In this book, Polanyi takes the task of justifying the holding of unproven traditional beliefs. Under the entry of "fiduciary program", the Index of the book lists more than 40 declarations of beliefs.

insists, for example,

> scientific development consists solely of the removal of superstition, prejudice, and other obstacles to scientific progress in the form of purely incremental advances toward final truth.[266]

But, as Dewey warns,

> We cannot keep connection on one side and throw it away on the other. We cannot preserve significance and decline the personal attitude in which it is inscribed and operative...[267]

In my opinion, it is Shapere who actually believes in an *Ideal* of science in which truth seems to be something so final and pure that it has less to do with history, society, culture, and personal idiosyncrasy etc. The image of Shapere's truth appears too absolute to be true. For the intention of Shapere' image, the required alienation of science from the indwelling of human being seems too extremely subjective on one side and too absolutely objective on the other side at the same time. In fact, this expectation of science is more ambitious than the Kuhnian approach in 1960s. What is more, the so-called anti-relativist image represented by Shapere as a matter of fact presupposes a not yet proved belief which,

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[266] D. Shapere, "The Structure of Scientific Revolutions", *Philosophical Review* 73, 1964, p.393. Unfortunately, Shapere might not be aware that what he insists is in fact one of the beliefs about history of science. What is more, his criticism of Kuhn is no more implied by historical facts than is the opposing view that we are still far from the stage of "purely incremental advances toward final truth".

[267] J. Dewey, "Beliefs and Existences", *The Middle Works: 1899-1924, Vol.3 (1903-1906)*, p.84.

therefore, has no legitimacy to bar Kuhn's belief as an alternative scientific standard of assessment. Kuhn's belief is built up, however, from his study of historical relics whereas Shapere's from his philosophical reasoning. That is, compared with the belief represented by Shapere which is actually itself from an intellectual's subjective ambition, the one declared by Kuhn in SSR is vividly from a practitioner's investigation about history of science. Given this, it might not be very difficult to notice the nature of the debate around SSR in 1960s, the nature that reveals what is important to the debate: more of fiducial commitments of certain groups of intellectuals than the real issue of advances toward 'final truth' of science.

In light of this, the debate can also be interpreted as involving commitments more to, as Polanyi points out, 'self-accrediting' which "is itself a fiduciary act of my own, which legitimates in its turn the transposition of all my ultimate assumptions into declarations of my own beliefs."[268] For, while defeating Kuhn's alternative belief in paradigms and incommensurability, philosophers like Shapere, Suppe, Davidson, Putnam and Kitcher etc. in fact themselves accredit their own judgement as the belief arbiter of all intellectual performances. Against their own wish, however, what they demonstrate unfortunately supports Polanyi's investigation of belief to such an extent that,

[268] M. Polanyi, *Personal Knowledge*, Happer Torchbook, 1964, p.265.

> We must now recognize belief more as the source of all knowledge. Tacit assent and intellectual passions, the sharing of an idiom and of a cultural heritage, affiliation to a like-minded community: such are the impulses which shape our vision of the nature of things on which we rely for our mastery of things. No intelligence, however critical or original, can operate outside such a fiducial framework.[269]

Since the analytical philosophy of science cannot deny that our mind lives in action, and any attempt to specify its presuppositions will produce a set of axioms which cannot, for some intellectuals, but can, for others, tell us why we should accept them.[270] Furthermore, I conceive that the key point of the discussion is not whether there are no beliefs in science but whether some beliefs in contemporary philosophy of science can be justified by reason in isolation from a certain indwelling.[271] As Polanyi remarks,

> All understanding is based on our dwelling in the

[269] Ibid., p.266.

[270] For Shapere, in particular, he cannot exactly, I am afraid, tell us why he in practice believes what he insists on as an axiom: scientific progress is bound to be in the form of purely incremental advances toward final truth. Whereas Kuhn in 1962 would not hesitate to inform us that what he believes in SSR is based on what he experienced by reading Aristotle's *Physics*. In this, I consider that it is not very hard to tell whose belief is groundless, given the fact that one does philosophy of science without one's personal discovery whereas the other freshly with the smell directly from practice.

[271] Here I differ from Polanyi's view of beliefs which assumes that one cannot tell us why we should accept certain belief while decline its opposite. I consider that Polanyi is not quite aware of the fact that there exist various kind of beliefs among which scientific ones have their characteristics which others do not entirely share. Otherwise all beliefs shall sink into the shadow of pure tacit knowledge and become wholly implicit.

> particulars of that which comprehend. Such indwelling is a participation of ours in the existence of that which we comprehend; ... Indwelling is also the instrument by which comprehensive entities are known throughout the world.[272]

Also, as Husserl's phenomenological approach reveals, *being-in-the-world* presupposes our understanding and it in turn builds up our understanding to the degree and content of beliefs, a process of *Bildung*.

But, as we have seen, not everyone in the process of *Bildung* acquires the same belief as the other. Here, only the different personal experiences count. And this aspect of personal encounter along the process of Bildung becomes the deeper requirement of building up a belief: to understand science, one has to convince oneself in direct experience, through dealing with the problems presented to it. This is also true, I believe, for those who are doing philosophy of science.

Now our understanding of the process of *Bildung* deepens: beliefs emerge not only from our indwelling in the particulars of that which we comprehend, ***Bildung***, but also from our personal activities in our dwelling in the world, ***Aufheben***. The former, in the process of *Bildung*, produces beliefs which we might not justify or wholly recognize, but the latter, in the process of *Aufheben*, could definitely provide our beliefs with a rationality deeply rooted in our experience which is not only partly articulated but also has workable consequences for the sciences. In this, although

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[272] Ibid., p.x.

the process in which scientific beliefs appear might not be fully articulated, the beliefs are not at all myths from nowhere and so it is possible to provide evidence for why they are supposed to be believed. For example, Dalton himself might have no idea how physiologically the atomic theory happened in his mind and the perplexities of metaphysical belief in atoms suddenly vanished, but he is definitely capable of telling people why he began to so believe with his experimental proof. Of course, belief which emerges in such a way is much more solid than those beliefs which are passively acquired in the process of *Bildung*, which has nothing connected with either special personal experience or unique scientific operations. This should be, therefore, counted as one of the important traits of scientific beliefs -- they are in certain degree different from other sorts of beliefs. For they are on the one side tacit knowledge, as Polanyi would say, for which we cannot clearly articulate how they come to us, in terms of *Bildung*; but on the other, they have profound proofs constructed through personal encounters and experimentations. Scientific beliefs are ones that are not only highly based on the process of *Bildung*, but also on objective justifications of unexpected personal experience in doing science, encountering the process of *Aufheben*. So, any scientific belief is subject to criticism, revision and even ultimate elimination through the development of its own implications by intelligently directed action. Belief itself is not the reason for believing. Rather, its reason is in the inevitable interaction between man and the life-world in which one lives, and in one's intention to know the world through personal experience, and to correct one's beliefs about the world.

This is the reason, I assume, why the contemporary thinker of American pragmatism, John Dewey, draws beliefs as "original Mr. Facing-both-ways".[273] On the one hand, "they form or judge-justify or condemn-the agents who entertain them and who insist upon them"; on the other hand, "they are of things whose immediate meanings form their content."[274] With this understanding, Dewey defines the notion of belief as such,

> To believe is to ascribe value, impute meaning, assign import. The collection and interaction of these appraisals and assessments is the world of the common man.[275]

In light of this definition, the world of the common man is the one in which the human being is not a pure intelligence but an organization of desires affected through reflection upon their own conditions and consequences. In other words, because to be the common man is to be thinking with desires, agreement does not come from oneness of intellectual conclusion, but from the sympathies of passion and the concords of action. Given this, we cannot but agree with Dewey's insights that the image of knowledge as being purely intellectual stands completely in opposition to the practice of common man who also thinks.

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[273] J. Dewey, *The Middle Works: 1899-1924*, Vol.3 (1903-1906), p.83.

[274] Ibid.

[275] Ibid.

So, the western intellectual professed ideal is, it seems to Dewey, rooted only in the ancient Stoic dogma that worships the belief of passionless imperturbality, absolute detachment, complete subjection to a ready-made and finished reality.[276] If developed to such a degree, the intellectual belief will become, as Feyerabend remarks, the source of a totalitarian tradition in the form of science.[277] This Stoic ideal of passionless Reason becomes the reason why analytic philosophy of science appears academically more and more remote from other traditions in western history of ideas, so that it can hardly teach the common man to notice that there exist incommensurable beliefs throughout the development of Western sciences and ideas.[278] This tradition is simply ignoring a basic fact that "reality" is experienced by human beings with certain kinds of belief with a certain history. Without belief, nothing in reality can be appraised and thus be managed as science, or even as thought, or "grid". That is, sciences are ultimately instrumental, in a certain sense, to human belief, in that they are disciplinary

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[276] Dewy remarks, "This ready-made reality, already including everything, must of course swallow and absorb belief, must produce it psychologically, mechanically or logically, according to its own nature"(ibid., p.85).

[277] Perhaps this is why, I guess, contemporary analytical philosophy of science tends to prohibit or correct any perceptions which stem from the tree of our fresh life experience: it simply stops hopes for beliefs they do not welcome while sanctioning only the one they believe.

[278] In Oriental history of sciences, there existed beliefs incommensurable with those of analytical philosophy of science in terms of its understanding of the world. In order to address the relevance, I provide a note of Chinese medical science in Appendix 4 as an antidote to comprehend incommensurability.

matrixes, paradigms, and also in that the matrix will change when belief changes.

The element of belief in science is real. For beliefs manifest their reality, as Dewey points out, in the usual way. That is, by experiencing the particulars, people modify and shape the image of the reality of those particulars; "they connect the bias, the preferences and affections, the needs and endeavour of personal lives with the values, the characters ascribed to things: -- the latter thus becoming worthy of human acquaintance and responsive to human intercourse."[279] I think that Dewey's account is much closer to the real story of the history of thought, indicating how beliefs and all they insinuate have been subjected to preconceived notions of knowledge and of reality. It is thus the story of that, as a matter of fact,

> practical human beings have in all ages worked out the implications of their beliefs, tested them and endeavoured in the interest of economy, efficiency, and freedom, to render them coherent with one another. Belief, sheer, direct, unmitigated belief, reappears as the working hypothesis; action that at once develops and tests belief reappears in experimentation, deduction, demonstration; while the machinery of universals, axioms, *a priori* truths, etc., becomes a systematization of the way in which men have always worked out, in anticipation of overt action, the implications of their beliefs with a view to revising them in the interest of obviating unfavourable, and securing welcome consequences.[280]

This is also Hegel's story: *Geist* to life in its own developing movement exposes its belief as such in its experience of

[279] Ibid., p.97.

[280] Ibid., p.94.

the movement.[281] In other words, only *within experience*, not *within knowledge of logic*, can the common man understand the world of immediate meanings which are empirically sustained as beliefs. And the opposing ideal of passionless knowledge seeks to purge science of all immediate personal reference, origin, and outlook. For the knowledge of the common man is rooted, Dewey considers, in the compact of the natural world to intelligence, of the moral and the spiritual world to belief, while the analytical school is in favour of absolute detachment, "dancing an idle attendance upon a reality complete without them [i.e. affections and aversions]."[282] From all these insights, Dewey unearths the disease of Western philosophy at that time:

> philosophy has dreamed the dream of a knowledge which is other than the propitious outgrowth of implications in order to recast them, to rectify their errors, cultivate their waste places, heal their diseases, fortify their feebleness: --- the dream of a knowledge that has to do with objects having no nature save to be known.[283]

Actually this is still the dream, I envision, of pure contemporary Reason which seems to have no intention to accord recognition to the experience of being-in-the-world. In terms of this, the dream that has been continuously dreamed against the

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[281] Dewey modestly claims that, at all events, he wishes to recognize his personal debt to Hegel for the view set forth in the paper, "Beliefs and Existences". And Dewey envisages that the paper implies that it represents Hegel's own intention which Anglo-American misinterprets as a Neo-Kantian, a Kantian enlarged and purified.

[282] Ibid., p.87.

[283] Ibid., p.86.

pragmatist movement ought to waken to notice its own indwelling in history. And the dream ought to be lessened in that one must live and experience before one wants to know. To believe means that, in consequence, a philosopher has to build up something through one's being-in-the world and not merely through one's being-educated-in universities. In this, philosophers have to welcome unexpected mental enlightenment and, as scientists always do, to transcend the limit of their usual belief.

In conclusion, I hold that nobody could escape the reality that one has to acquire certain beliefs in order to survive, a process to undergo ***Bildung*** as well as personal encounters and mental enlightenment, ***Aufheben***. And what I have been arguing is the following eleven points:

1. The process of *Bildung* denotes that before valuing the world one has to live in a *Dasein* of the world in which there exist a particular history, culture, society, and community all of which have been predetermined independently of one's choice.

2. Everyone is already in a certain kind of process to build up one's beliefs of surroundings, the process of *Bildung*. Thus the acquirement of a belief has in nature less to do with one's later education, although the latter is also the process of conventionally building up certain kind of belief with teachers.

3. However, the process of *Bildung* is not enough to build up new beliefs. For belief change, one has to personally encounter, struggle, and run through mental enlightenment, the process of

Aufheben, in a particular context of belief crisis.

4. It is time to research the process of acquiring new beliefs through mental enlightenments. It consists of gradual and sudden ones, which *Aufheben* one's personal experience to perceptions of objects as truth in a time. This is the philosophical topic I wish could be accorded more attention in the near future.

5. As Dewey points out, "truth is the union of abstract postulated meanings and of concrete brute facts in a way that circumvents the latter by judging them form a new standpoint, while it tests concepts by using them as methods in the same active experience. It all comes to experience personally conducted and personally consummated."[284]

6. Thus a belief is not only a philosophical notion, but also a practical way which, unlike the convictions of the common man and the hypotheses of sciences, finds its proud proof in the fact that it does not demean itself so unworthily as to work.

7. Given the reality that beliefs are produced in the process of building-up in certain *Dasein, Bildung* as well as in the process of *Aufheben*, they are all historically, culturally, and socially toned, sustained, and directed. They are evidences of the deep-rooted preconception that whatever concerns a particular conscious agent, a wanting, struggling, satisfied and dissatisfied being, must of course be only 'phenomenal' in import.

8. Therefore, any belief is subject to criticism, revision and even ultimate elimination through the development of its own

[284] Ibid., p.94.

implications by intelligently directed action.

9. Consequently, belief itself is not the reason for believing. Rather, its reason is in the inevitable interaction between man and the life-world in which one lives and intends to know through experience.

10. In this, sciences are ultimately instrumental to human beings, in that scientific beliefs are disciplinary matrixes, paradigms, and that they will change when beliefs change.

11. Philosophers who, after some struggle, only have one Reason for believing are those who live in a philosophical compartment, as the pragmatist points out, that has neither fresh experience of the world nor information from their colleagues in other departments. And they become those in academics who badly need the thrill of mental enlightenment to enlarge their vision of beliefs of the world.

Nothing is mysterious about belief. For everybody believes something, including scientists. Belief "is a cake that is had only by eating it, just as there is digestion only *for* life as well as *by* life."[285] The myth about belief is only in the mind of those philosophers who are infatuated with the *Domain* of one Reason, not with information, and who aspire after the dream of an enlightenment of, as Dewey draws, a universe of pure, cognitional objects, fixed elements in fixed relations, a hidden structure behind the phenomena. Of course, this eagerness to know the pure is

[285] Ibid., p.85.

admirable, but a modest attitude towards the world is much more needed, essential and workable. Otherwise, the following myth is always telling:

> The ancient myth of Tantalus and his effort to drink the water before him seems to be ingeniously prophetic of modern epistemology. The thirstier, the needier of truth the human mind, and the intenser the efforts put forth to slake itself in the ocean of being just beyond the edge of consciousness, the more surely the living water of truth recede![286]

For even at this level there still exists the value of beliefs. Tantalus' temptation to drink is itself enticed by the king's edificational belief that water is the medicine when one is thirsty. The thirstiness and the way of curing it, what is more, are of personal as well as physical experience; belief has no need to be valued only by Reason or language analysis; as a belief of living, it is already valued thousands of times in practice.

[286] Ibid., p.93.

13

The Consequences of Incommensurability

--- Paradigm Revisited

After the deconstruction of SSR by its architect, Kuhn's approach becomes, to some degree, incoherent. Yet the structure has not completely collapsed, for one of its central pillars, the notion of incommensurability, is still in its 'local' sense retained. As Kuhn observes:

> Since success in interpretation is generally achieved in large chunks ("breaking into the hermeneutic circle"),

> the historian's discovery of the past repeatedly involves the sudden recognition of new patterns or gestalts. It follows that the historian, at least, does experience revolutions. Those theses were at the heart of my original position, and on them I would still insist.(PSA, p.715)

That is, although Kuhn has, to a very large extent, localized the notion of incommensurability, he is aware, consciously or unconsciously, that holding the notion is crucial to his entire approach since 1947. What is more, the notion is so tangible to him since he himself unforgettably underwent the experience of "sudden recognition of new patterns or gestalts" in 1947, and it was this encounter of mental enlightenment that established his new way of viewing history of science.

On the other hand, Kuhn had been so deeply absorbed in the linguistic analysis of the notion that he has almost no option left but to disentangle the complex of incommensurability. It seems that he disregards the fact that he discovered the phenomenon of incommensurability in a totally different context. It also seems that, during years of his attempts after SSR, Kuhn becomes so haunted by the charge that his approach is of "irrationalism", "relativism", "unanalyzable assumptions" and "mysterious accounts", that he presumed that the flaw in his approach might lie in the fact that it lacked the required analytical skill with which he was not well equipped to deal with as a professional physicist.

In short, it seems that he never realized that the debate itself illustrates another example to sustain his notion of incommensurability. Kuhn did not notice that overcoming the incommensurability between his views and those of his opponents is not simply, as Shapere suggests, a question of a more careful scrutiny of one's tools of analysis, but rather of acquiring a good grasp of hermeneutics in terms of history. He losses sight of the fact that incommensurability also implies a preference for specific certain tools of demonstration. That is, tools in different traditions are often incommensurable. In this, employing opponent's tools while abandoning one's own amounts to a combat without one's weapons of choice: it is merely a way of surrender. In a word, throwing away those convenient tools of hermeneutics with which he built his account of the structure of scientific revolutions in 1962, Kuhn was in fact persuaded by mainstream philosophers to such a degree that he never sought to interpret incommensurability in a historical context. This is the misfortune of the Kuhnian approach to the history of science.

In consequence, the only thing he tried to defend in 1982 is his 1947 experience of how to read an out-of-date text in the present. Not only that, in 1982 Kuhn admitted that his view of revolutionary scientific change of science had been largely moderated to a degree that language change becomes more profound than the normal/revolutionary distinction had been. As I have discussed, from Chapter 9 to Chapter 12, Kuhn no longer conceives that the usage of a language is always changing according to conceptual changes or meaning changes in history.

In my view, language, as one of the instruments we use to exchange ideas, cannot change its meaning without its background changing, i.e. without changes in our experience in the life-world. It is therefore the process of *Bildung* that accounts more for meaning change in a language, rather than the local linguistic change itself; reading an out-of-date text is bound to face a gap between at least two different processes of *Bildung* in history. In this, the reading can never be a local event only concerning "no common language" in the present. To reduce the issue of *Bildung* into language change, it seems to me, Kuhn misleads not only his readers but also himself to such an extent that he has buried his own experience of sudden recognition of the incommensurable phenomenon, under the temptation of a prevailing analytical metaphor. This is, I believe, the inner reason led to Kuhn's tragedy: under the attack of analytical philosophy of science, he has moved further and further away from his own experience in 1947, while being tempted to construct some new linguistic metaphors in order to compromise with the mainstream philosopher of science.

Kuhn's vicissitudes thus prove that one can only talk about one's beliefs, as Polanyi points out, on the basis of one's own indwelling. In terms of this, I propose that Kuhn has betrayed his experience of 1947 and his beliefs of the 1960s, so that after twenty year's of "corrections" his SSR approach becomes so alien that it is not recognizable to the author of the 1960s writings. Kuhn seems no longer to understand why he viewed the history of science as he did in the 1960s.

The alienation happened in Kuhn's discourse is of course the result of a belief-change [without according experience]. In my opinion, Kuhn has largely converted to the view of his rivals at the expense of most his beliefs in SSR. He was not aware that employing opponents' methodology, i.e., methodology change, also has to do with the notion incommensurability he still holds. For Kuhn, as long as his way of reading out-of-date text is accepted, he seems not to mind going wherever his rivals request for analytical tools. This alienation, therefore, attests to that he fails to realize, in contrast to Feyerabend, that there exist at least two incommensurable philosophical traditions which have fought each other since the beginning of Western thought, a fight which includes disputes about methodologies of demonstration. So, he never sublimes his philosophic interest to the level that incommensurability as empirical phenomenon, which both Kuhn and Feyerabend proclaimed in 1962, exists in our experience of communication as long as human beings have tried to understand the world. Thus, Kuhn seems still remembering the experience of 1947, but fail to see the philosophical implication of it, *viz*, that different methodologies imply different beliefs which cannot be regarded as commensurable in understanding the history of sciences.

Speaking a language, as Kuhn once realized, is always speaking a language in history whose root is not, unlike what Kuhn said in 1982, other languages, but mainly the process of *Bildung* in which the usage of language is built up in certain history. In this, Kuhn's belief-change is Pickwickian, so willing is he to compromise in face of the charges brought by analytical philosophy, at the expense of all his views, with the exception of the one that is incommensurable to his belief-change, his experience of enlightenment in 1947.

This is why, at the same time of his conversion, Kuhn also stubbornly seeks for ways to defend a localized notion of incommensurability, without attempting to forge agreement with his rivals. He is willing to employ the 'language' that is familiar with his opponents so as to transport his notion of incommensurability into that 'language', in order to make that tradition of 'language' change accordingly. However, as Feyerabend has pointed out, incommensurability in his sense

> occurs only when the conditions of meaningfulness for the descriptive terms of one language (theory, point of view) do not permit the use of the descriptive terms of another language (theory, point of view, mere difference of meanings does not yet lead to incommensurability. (FR, p.272)

So, what Kuhn seeks is a one-sided deal with his opponents in the hope that he can introduce his conditions of meaningfulness at the expense of those of his opponents. Whereas this clever deal is too wishful to be accepted. For the deal itself is obviously a dream which is only dreamed by Kuhn himself. No philosopher in the analytical school would accept what Kuhn has proposed. On the contrary, they demand of Kuhn that he thoroughly abandon the notion of incommensurability; only then will his discourse, they maintain, be clear enough to be understood by mainstream philosophy. Indeed they request nothing to be changed in Kuhn's discourse but the story of enlightenment Kuhn experienced in 1947. And from this point on, they aim to dispense with the whole hermeneutic approach in Kuhn's philosophy of science in favour of "a particular approach to analyzing knowledge -- an approach which is in opposition to the dominant approach recently evidenced in epistemology."(SST, p.728)

That is why, at least for Suppe, the 1969 Illinois Symposium was held at a moment in which the philosophy of science was in an acute state of disarray, a chaos that eventually led it to find its new and supposed direction. The new direction, Suppe indicates, is both against positivistic philosophy of science and the work of Kuhn and Feyerabend. Suppe thus holds that this new direction has led to the emphatic belief that an adequate philosophy of science must embrace a "hard-nosed" metaphysical and epistemological realism, wherein how the world *is* plays a decisive role in the epistemic efforts and achievements of science. Doing so requires, Suppe believes, calling into question a number of deeply held traditional beliefs about knowledge, this leading to sustained efforts by philosophers of science to rethink many of the most basic issues concerning the nature of a posteriori knowledge and its metaphysical underpinnings.[287]

It is under the vision of this new direction, Suppe satisfactorily proclaims, that Kuhn's beliefs are collapsing:

> Thomas Kuhn publicly has taken criticism of his views seriously and has attempted to address himself to them in a number of recent articles... Often Kuhn's responses take the form that his critics have misinterpreted him or

[287] Cf. the second edition of SST which is edited with a "Critical Introduction" and an "Afterword" by Frederick Suppe, University of Illinois Press, 1977, p.649.

> else that, when in the critic's opinion particularly significant and telling objections are raised, he is incapable of seeing where his critics think there are disagreements between them other than in nuances; and he has been elective in which criticisms he chooses to address. However, he has taken a number of the many criticism of his position seriously and has made conscientious attempts to either clarify or else modify his position in response to them. (SST, pp.643-644)

Suppe is quite pleased also with the fact that, under the rational scrutiny, Kuhn's approach appears not as hopeless as Feyerabend's, although it too, he concludes, should be rejected as 'irredeemably flawed'.

But for me, it is rather the opposite, because what I am pleased to see is that philosophers of science, such as Suppe, after some struggle, have only a narcissist Reasons left for beliefs. With this image, Suppe reaffirmed a number of beliefs deeply held in Western philosophical tradition of knowledge, without any 'rational analysis' to his contemporary audience. While applauding Kuhn's conversion to 'the new direction', Suppe is unaware of the fact that the so-called 'new direction' is incommensurable with the historical fact of the separation of, as well as the developments of, modern philosophy and sciences. It seems to me, the so-called "epistemological realism wherein how the world *is* plays a decisive role in the epistemic efforts and achievements of science" is neither a central philosophical issue, and has not been since the birth of modern science, nor a epistemological issue for contemporary philosophy of science. For the issue is by all means a scientific inquiry which needs scientific communities to operate and experience only by doing science. In terms of this, Suppe's 'new direction' is not only beyond the domain of contemporary philosophy of science, but is also foreign to the academic reality we are facing. The proposal of this 'new direction' itself is rather a proof of incommensurability, given the fact that the 'new direction' is opposed to the hermeneutic philosophy of contemporary sciences once represented by Kuhn.

At this point, also, science hardly needs any help from Suppe's 'new direction'. Because, this narcissist attitude in the philosophy of science only reflects the obsolete intellectual desire, as Dewey pointed out at the beginning of the century, to idealize the principle of intelligence within its intellectual compartment. This ambitious belief, of course, has nothing common with those beliefs coming from our living experience of being-in-the-world and doing sciences.

It is true that Suppe and the 'new direction' have no intention of listening to the call from experience Kuhn has encountered in 1947. For they think that to rethink many of the most basic issues concerning the nature of a posteriori knowledge and its metaphysical underpinnings is not investigating how they happened but requiring to question a number of deeply held traditional beliefs about knowledge. As one of the representatives of the new direction, Shapere argues, in approaching the investigation of science, that the urgent thing we have to perform is not to see what really happens in our experience of practising science but sharpening analytical tools and locating an adequate vocabulary for talking about science.[288]

Given the nature of the 'new direction', however, it is quite bizarre that while Kuhn's historical approach has won the applause of many in the international academic world and already appeared as a distinct direction in the philosophy of science, which has nothing in common with Suppe's 'new direction', Kuhn imprudently hopes to catch up with the latter, in a form he already disowned as early as 1947.

What is more, as I depicted in Part One and discussed in Part Two, incommensurability is an empirical notion rather than a theoretical invention. That is, it is something that is experienced and not merely a theoretical construct. So, the problem of understanding incommensurability is not difficult for those who have an experience of mental enlightenment, sudden or gradual ones, but perhaps difficult for analytical philosophers of science who "represent 'the general will' among philosophers of science today and thus provides a reliable guide to future philosophical advances in understanding the scientific enterprise."(SST, p.730) Unfortunately, the 'general will' itself has not yet been justified on the basis of scientific practice from which its working belief is built up. The incommensurable phenomenon is not at all an issue of "general will" but merely of understanding and communication among our human beings; it is observed, at least, when human beings communicate or interpret a text to each other while exchanging

[288] Cf. Shapere's paper, "Meaning and Scientific Change", in *Mind and Cosmos*, (ed.) by R. G. Colodny, University of Pittsburgh Press, pp.80-81.

their views of the world in global transactions. Consequently, the phenomenon of incommensurability challenges the view that the most convincing interpretation of the world is one that originates with Suppe's 'general will'. On the contrary, the 'will' itself is questionable in that it has no colour of, as Suppe requires, a realist view of science. Given that scientific beliefs are built up on the basis of our experience in history, the incommensurable phenomenon cannot be merely understood with the help of an ahistorical 'general wills'. Also, the phenomenon of our belief encounters shows, when exchanging different views, that there exists something in understanding and communication which cannot be commensurable only with Suppe's 'general will'.

In connection to this discussion, I want to point out that even Feyerabend's definition of incommensurability falls short of the insight which Kuhn's 1947 enlightenment highlighted. For Feyerabend, incommensurability is only a rare event and it occurs only when the conditions of meaningfulness for the descriptive terms of one language (theory, point of view) do not permit the use of the descriptive terms of another.[289] However, he does not further specify what "the conditions" are and why they only apply to the meaningfulness of the descriptive terms of a language. Specifically, Feyerabend does not notice that the issue is by no means only of 'the descriptive terms of one language'. Something more than language is involved in this issue, as I have emphasized in Part Two.

[289] Cf. FR, p.272.

Given the fact that the conditions of meaningfulness can vary, as Feyerabend puts it, from analytical traditions to historical traditions, there are incommensurable traditions even about what constitutes the conditions of meaningfulness for a language. For example, in terms of the analytical tradition, Shapere holds that

> we must beware of analytical tools which, ... have never been adequately clarified. Such are the distinctions, between what is and what is not part of the meaning of term, and between stability and change of meaning.[290]

So, for this tradition, only the sharpening of the analytical tools can be regarded as providing conditions of meaningfulness. But it is a fact that this account of the analytical tools is not the only view concerning 'descriptive terms of a language', at least in terms of Feyerabend's historical interpretation of incommensurability.

For one thing, the conditions of meaningfulness of one language ought to be also understood in terms of beliefs which emerge from the primitive level of scientific practice or operations. These beliefs, considered by Kuhn in 1960s as an element of the scientific matrix, consist of a paradigm within which one "does not permit the use of the descriptive terms of another" freely. In other words, no matter how we define the conditions of meaningfulness, we still badly need Kuhn's 1962 notion of a paradigm to understand why conditions of meaningfulness

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[290] D. Shapere, "Meaning and Scientific Change", in *Mind and Cosmos*, (ed.) by R. G. Colodny, University of Pittsburgh Press, 1966, p.80.

differ among groups, the phenomenon of incommensurability, even while employing the same language to define those conditions.

In a nutshell, incommensurability is not, as both Feyerabend and Kuhn declared in 1962, an issue only of language, but mostly the one of *Bildung* as well as of *Aufhebung*, i.e. a powerful but latent historical process building up meaningfulness in people's mind in a context with its own history, culture, society, discipline, experience, and language etc. It is this diachronical process that mostly accounts for those conditions of meaningfulness of a language, not *vice versa*.

Therefore, I believe that incommensurability does not only cover a rare event as Feyerabend contends. To further enlarge our understanding of the phenomena of incommensurability as well as to discuss 'the conditions of meaningfulness', I shall review Kuhn's encounter with Aristotle's *Physics* as my last case study to reinforce the significance not only of Kuhn's notion of incommensurability, but also his controversial conception of paradigms. However, the story of Kuhn's enlightenment has been told many times before, but I plan to consider the historic discovery of conceptual change from a rather different perspective.

When James Conant called for attention to history in his *Tercentenary Oration* in 1936,[291] his target was the current dominant way of teaching American children, which was largely based

[291] Cf. James B. Conant, *My Several Lives*, Happer & Row, Publishers, 1970, p.656.

on the study of classics and mathematics. As a matter of fact, that is the way that, for more than fifty years, the mind and the art of the western world had been shaped, the way in which it was built up (*Bildung*), in forming thought by static, structural, ahistorical, abstract -- in many ways mathematical -- patterns and procedures. The dominant way, whose origin was Gottlob Frege's work in the 1880s, was against history, psychology and their affiliates, in favour of more abstract conceptions of "logic forms", which were considered to be intrinsically "significant".

For Frege, who denounces any tendency to confuse the true concerns of logic and mathematics, it was an illusion to suppose that either historical research into the origins of logic or psychological experiments on child development could teach us anything about the intellectual foundations of the principle of non-contradiction. On the contrary, the philosopher's prime duty, in Frege's view, is to strip away the historical and psychological accretions which veiled concepts in their "pure logic forms" from the eye of the mind. With this Platonist manifesto, Russell launched his programme for reforming philosophy, beginning with the purification of logic and conceptual analysis in western philosophy.

And it is known that by the 1950s, the logical positivist campaign of *Formalist Reformation*, through the efforts of philosophers of the Vienna Circle, had come to play a dominant part not only in the philosophy of physical science but also, in the

U.S.A. at least, in the methodology of the social sciences.[292] This was the academic atmosphere in which Kuhn was edified in the 1940s. Under this process of *Bildung*, Kuhn could only have acquired a discourse in which all subjective issues of taste and value could be set on one side, and the systematic analysis of "objective validity" could and should be pursued with the same purity and rigour as Frege's "pure logic forms" of mathematical concepts or Hilbert's "axiomatic systems". In this way, intellectuals at that time came to believe that the intellectual content of any truly scientific theory forms a *timeless* propositional system, like that of which Russell and Whitehead had given a prototype in *Principia Mathematica*. In due course, every other natural science worthy of the name would presumably achieve the axiomatic articulation that Hertz and Mach had demonstrated in the case of classical mechanics.

With this kind of *Bildung* in mind, it is no wonder that Kuhn was astonished by the content of Aristotle mechanics, when Conant requested him to undertake the role of lecturing on the origins of seventeenth century mechanics in 1947:

> Even at the apparently descriptive level, the Aristotlians had known little of mechanics; much of what they had had to say about it was simply wrong. No such tradition could have provided a foundation for the work of Galileo and his contemporaries. They necessarily rejected it and began the study of mechanics over again.(ET, p.xi)

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[292] It is interesting and fortunate that the "Reformation" never occupied the same part in the field of social sciences in Europe where it originally launched.

And Kuhn was certain, according to the way of thinking he had learned, that Aristotle was badly wrong:

> How could his characteristic talents have failed him so when applied to motion? How could he have said about it so many apparently absurd things? And, above all, why had his views been taken so seriously for so long a time by so many of his successors? The more I read, the more puzzled I became. Aristotle could, of course, have been wrong -- I had no doubt that he was -- but was it conceivable that his errors had been so blatant? (ET, p.xi)

It is clear that for Kuhn, at that time, mechanics ought to be seen as a *timeless* system or structure, i.e. a "pure form" in the sense of Newtonian mechanics. Or as Kuhn himself puts it, "being posed in a Newtonian vocabulary, those questions [i.e. those of Aristotle's mechanics] demanded answers in the same terms."(ET, p.xi) With this kind of image of mechanics, Kuhn was then forcing Aristotle to answer a Newtonian question:

> How much about mechanics was known within the Aristotelian tradition, and how much was left for seventeenth-century scientists to discover? (ET, p.xi)

So convinced at that time was Kuhn of the essential priority attaching to questions about the synchronic structure of mechanics, in terms of Frege's image of science, that he was not yet prepared to answer questions of diachronic issues in mechanics, i.e., questions of conceptual change. Apparently, for Kuhn before his 1947 enlightenment of incommensurability, history is above all a process of development-by-accumulation, a view which was still in the shadow of Harvard historian of science, George A. L. Sarton, and Conant.[293]

In other word, when encountering Aristotle's mechanics, Kuhn then envisaged that science is the constellation of facts, theories, and methods collected in current texts and held that scientists are people who, successfully or not, have striven to contribute one or another element to that particular constellation. Exactly in terms of what Kuhn criticized in 1962, what then he believed was that

> Scientific development becomes the piecemeal process by which these items have been added, singly and in combination, to the ever growing stockpile that constitutes scientific technique and knowledge. And history of science becomes the discipline that chronicles both these successive increments and the obstacles that have inhibited their accumulation.(SSR, pp.1-2)[294]

Therefore, there is no question about the fact that where Kuhn stood before his enlightenment of incommensurability in the summer of 1947 was a completely different "form" of thinking with which he

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[293] George Sarton, one of the founders of the discipline of history of science, was never a believer in conceptual innovation of sciences. He considers that it is only our superficial "first impression of scientific progress" that shows us science advancing by discontinuous giant steps, like a set of "gigantic stairs, each enormous step representing one of those essential discoveries which brought mankind almost suddenly up to a higher level". As we "pursue our analysis," he said, we find "the big steps... broken into smaller ones, and these into others still smaller, until finally the steps seem to vanish altogether".(Cf. Sarton's *The History of Science and the New Humanism*, Harvard University Press, 1937, pp.21-22. Likewise J. B. Conant regarded the history of science as the only truly cumulative and progressive activity of mankind.(Cf. Conant's *On Understanding Science: An Historical Approach*, Yale University Press, 1947, P.20.)

[294] These sentences are telling in that they were employed by Kuhn in 1962 to describe the view he held in the 1940s and also to depict the image he was to challenge.

had been justifying the world he encountered. And the "form" is no doubt a thinking framework, or a "conceptual scheme" as Kuhn defined in CR, or, more precisely, a "paradigm" as he proclaimed in 1962. What is more, it is the paradigm that once was shared by Kuhn's contemporaries, as well as the one that they denied when criticizing Kuhn's notion of a paradigm.[295]

It is therefore fortunate for us to have the Kuhnian experience of enlightenment in hand. And it is fortunate for Kuhn himself that he discovered a totally different paradigm in the western history of science through his experience of enlightenment.

The most interesting and surprising part of Kuhn's enlightenment is, as was detailed in Chapter 2, that with recourse to the principle of Frege's *Formalist Reformation*, i.e., the systematic analysis of objective validity confined to "pure forms" or "axiomatic systems", Kuhn surprisingly dispelled the rubric of the reformation. That is, when puzzled by the fact that Aristotle's interpretations of phenomena had often been both penetrating and profound, Kuhn badly sought *the* answer on how Aristotle's characteristic talents could have failed him so when applied to motion, and how he could have said about it so many apparently absurd things and, above all, why his views had been taken so seriously for so long a time by so many of his successors.

In posing these questions, Kuhn was searching for the

[295] To this extend, we can see that it is really interesting in that people who think in certain way vehemently deny the fact that there was and is, at least, a way or a paradigm of thought in history. For them, the real problem is perhaps that they never experience the thrill of enlightenment.

axiomatic "coherence" which made the Aristotelian system a scientific system. And they are questions that cannot be answered by recourse to Newtonian physics and to current prevailing image of science. However, with good will, Kuhn believed that there must be "coherence" somewhere in the Aristotelian system which could provide the answer of its coherency. Because, with the belief of the *Formalist Reformation*, Kuhn then believed that the father of logic, Aristotle, ought to have organized his system, at least, in logical terms.

Having suffered the pain of reading Aristotle and of reconstructing his image of science, Kuhn suddenly found the "coherence" of the Aristotelian system: the idea of change-of-quality in general. As a consequence, through the experience of enlightenment in a hot summer day in 1947, Kuhn not only discovered the coherence in the Aristotelian system, but also, what is more, disowned the formalist image of coherence in history. A conceptual change emerges. For Kuhn after the enlightenment, the continuity in history was no longer the tale formalists told; the perspective of discontinuity in history, conceptual change, was so fresh and so real in his interpretation of the history of science, in particular, his interpretation of the Aristotelian system.

Accordingly, Kuhn's way of thinking was completely changed to such an extend that he himself considered it a "revolution". What the Revolution largely changed was his view of history and the philosophy of science, i.e., his belief that conceptual change was a process of static historical continuity. Then the perspective of dynamic discontinuity in history, the concept of incommensurability, was born as the resolution of Kuhn's mental ***Aufhebung*** in the summer of 1947.

I have been emphasizing in this essay that the discovery of discontinuity in history itself, i.e. the perspective of incommensurability itself, is in nature not an issue of theoretical demonstration. It is in the first place an event of human experience, an event of mental enlightenment of which contemporary western philosophers have as yet little knowledge yet.[296] And I believe that the event itself is an experiential defence of Kuhn's insight about paradigms and incommensurability. This defence can do what a theoretical critique cannot. For the latter always presupposes the former. This explains why I recall Kuhn's experience of enlightenment, again, at the end of Part Two: the phenomenon of incommensurability and paradigm is not at first an issue of theory but of experience.

In view of the above mentioned facts, I conclude that there are sufficient reasons to believe in the existence of the phenomenon of incommensurability. I hope to have blunted, in consequence, the attacks on Kuhn's 1962 approach.

It is a shame to talk of the philosophy of science only by the standard of North America and to regard it as only the 'general will' of those who always want to make pure, theoretical and

[296] In this, the event cannot be fully covered only by the philosophy of science. It needs more cooperation with the other branches of the sciences.

universal claims about science and who have not yet had enough scientific experience of doing sciences. Philosophers have not perceived that we are living in a world in which there exists a large number of different paradigms with which we have to commune.

Thus to sum up Part Two, we can understand incommensurability and paradigms by considering the following points:

1. Incommensurable phenomena are not rare events. The phenomena reflect human beings' limitations, especially the limitation or *Dasein* of our mental constructs in history. In other words, Being for men is being-in-the-world-in-a-certain-time or indwelling-in-the-world. So any discourse reflecting an individual's indwelling is inevitably restricted by their being in the world. The way one is edified, the process of *Bildung*, beyond one's will, immerses the way one thinks. Given that no one can act and think without reference to one's indwelling, the issue of incommensurability is, as Hegel says, of being-in-itself in history.

2. It is the being-in-itself that builds up an environment of discourse, a paradigm, a limited way for mankind to act-in-the-world. Everybody behaves and thinks in a certain kind of way, or in a paradigm, no matter whether one accepts it or not. For nobody can understand the world without acquiring a built-up ***manner*** along the practice of one's being-in-a-certain-world.

3. It is the process of ***Bildung*** and ***Aufhebung*** that shapes our views and results in paradigms, consisting of unlearnable and

inimitable ***styles*** and ***tacts*** that affect our judgement and the mode of knowledge acquired in those processes. Given one's unique experience and knowledge in history, paradigms thus formed provide the basis for incommensurability in history. That is, nobody could posses and thereafter share a common ground to explain how paradigms were formed so and why they cannot be merely educated universally until one is built up by doing so. Only the history of their *Bildung*, therefore, supports the phenomena of incommensurability and explains why paradigms are incommensurable in history.

4. Epistemologically, as Chapter 9 emphasizes, the mental experience of enlightenment provides answers about why in the process of *Bildung* human beings can overcome the limit of *Bildung* to *Aufhebeng* into another. According to Kuhn's experience of enlightenment in 1947, personal commitment and experience can finally trigger the mental transformation that fulfils a process of *Aufhebeng*. *Through the transformation*, one overcomes problems of incommensurability and creates certain paradigms. This is, apparently, the epistemological area where contemporary western philosophers have as yet accord little attention. I hope that Kuhn's enlightenment can shed light to philosophy of science in that there is nothing mysterious to treat the personal enlightenment as irrational procedures.

5. Incommensurability opposes the method of philosophical analysis as the basic way of philosophizing, as noted in Chapter 10, for incommensurability also means unanalyzability in a certain

sense. Psychologically, as Kuhn once emphasizes, seeing similarity relationship plays a much more primary role in the cognitive process than in analytical demonstrations. The latter's axiom is in fact the upshot of *Aufheben*. This shows that philosophical analysis cannot analyze out where its axiom comes from and why it can only provide the answer of commensurability.[297]

6. In connection with this, I argued in Chapter 11 that the debate about incommensurability cannot be settled in the arena of philosophy. As Kuhn once wrote, conceptual change is illuminated in the context of social and cultural encounters. A paradigm is not only the product of education but also one of *Bildung*. Without sociological considerations, incommensurability is opaque for those who insist that the epistemology of science has nothing to do with the society in which science emerges. The sociological dimension of incommensurability reinforces the view that to understand incommensurability, one needs to be prepared with more than philosophical training and reading.

7. Pragmatically, as discussed in Chapter 12, incommensurability is above all relevant to the topic of belief-change, a change from intellectualism to pragmatism, a change from *Reason* to reasons in practice and experience. Those who always stay in the intellectual compartments, as Dewey points out, not only tend to extinguish information from 'common man', but also refuse

[297] It is conspicuous that philosophical analysis has tendency, due to its method of analysis, to create commensurable images which attempt to demonstrate that everything in history is only a product of an ahistorical framework.

to judge the information in its own right. The discussion of incommensurability in this essay, therefore, appears itself as an example that at least two disparate views, two paradigms, have no pragmatic consensus because one party in the discussion refuses to take the stand of another. In this, the issue of incommensurability and paradigms is not theoretical one at all, but experiential, even in the philosophical practice. For the answer to theoretical problems, as is always the case, is not decided by theoretical commitments: incommensurability and paradigms must be understood in terms of experiential encounters.

8. But there are different kinds of experiences of incommensurability and paradigms which Kuhn never properly distinguishes. The first one is synchronical, such as the debate I have shown in Part One. They are of incommensurable academic schools that hold different paradigms. This is the kind of incommensurability that can be resolved by a commitment to communication, dispute, and conversion etc.

9. The second one is diachronical, such as the example Kuhn experienced in 1947 when he was reading Aristotle, the one between the present and the past. That is, the incommensurable schools involved occupy different time frames. This area is the proper domain of hermeneutics, which does not involve communication and conversion, but understanding and interpretation.

10. The third one is cultural, such as my experience and

encounter with Western philosophy.[298] This sort of incommensurability occurs when people in two cultures try to understand each other. It is an encounter that happens not only in linguistic communication, but also in exchange of cultural and social background or traditions. This is, therefore, the area of cultural anthropology or comparative cultural studies, which are far beyond the reach of philosophy. However, this kind of incommensurability can be the topic of history of philosophies in a different context.

11. The fourth one is concerned with personal matters, such as ethical dilemmas which can occur frequently. This is covered by psychology and ethics, which deal with individual idiosyncrasies. For example, different responses that occur even in the defense of the historical approach to science between Kuhn and Feyerabend.

13. The incommensurable phenomena varies. It needs more scrutiny rather than simply denunciation, which is the attitude against any possible change in our view of the world. And we all know, this is the last attitude we need in conducting philosophy as well as in managing our daily life.

[298] In fact Feyerabend, in his recent book, *Farewell to Reason*, discusses this issue in considerable detail, in particular, in Chapter 1, "Notes on Relativism" (FR, Verso, 1987, pp.19-89).

PART III

Conclusion

14

Understanding and Communication

According to F. Suppe, the historic debate between Kuhn and his opponents in the 1960s was virtually settled at the 1969 Illinois Symposium on *the Structure of Scientific Theories*.[299] In

[299] Suppe remarks that the Symposium's timing was set at a time when philosophy of science fell into a state of acute intellectual disarray, and that the purpose of it was to bring together leading figures in philosophy of science and allied disciplines so as to sort out prevailing chaos and to search for new, productive intellectual directions to follow (cf. SST., p.4).

my view, however, the symposium in fact embarked on that debate in the United States. It was settled, if it was at all, only in the sense that Kuhn had never conducted a satisfactory defense of his position and, what is more, his opponents never suspended their preconceptions so as to be able to comprehend the consequences of Kuhn's experience of enlightenment in 1947.

That is, one side of the debate, even up to now, never attempts to answer the question: whether Kuhn's enlightenment in 1947 was real and thus whether issues like this sort should be legitimately taken into account of contemporary epistemology. At the same time, unfortunately, the other side of the debate, Kuhn himself, has never sufficiently defended the phenomena of incommensurability in terms of history of science and, in particular, of his experience of enlightenment that made the notions of a paradigm and incommensurability stand firm. Rather, to a very large extent Kuhn withdraws from what he achieved between 1947 to 1962. And even further, surreptitiously, he contrives to convert some of his views to those belonging to his opponents, the analytical school of philosophy of science, as we have shown in Part One. As a result, as I have discussed in Part Two, the most crucial issues in the debate remain untouched in their own right. They are supposed to be the issues of contemporary epistemology concerning such elements as *enlightenment, incommensurability, paradigm, Bildung, Aufheben, education, belief*, as well as the role of philosophical analysis itself.

With these unsolved questions in front of philosophers, the debate should be resumed, to my opinion, in its own terms which hitherto have not yet been seriously tackled in depth by contemporary western philosophy, especially by Anglo-American philosophies. In other words, the tradition in which Kuhn is *a contemporary* of its representatives cannot be valued in Kuhn's surreptitious conversion. Instead, this tradition ought to be deliberated in its issues raised from the relevant practice of everyday thinking and, as I insisted, from the entire legacy of the western tradition since Hellenic times.[300] At least, philosophers ought to confront these issues first in the area in which they appear rather than ignore their existence in the practical level of our thinking.

Suppe's claim about the defeat of an unanalytical philosopher like Kuhn[301] in the contemporary philosophical arena is a shame in that the mainstream philosophers never notice the value of Kuhn's proclamation about the phenomena of incommensurability when reading the out-of-date text of Aristotle. It is in this angle that I suggest in the thesis that the issues raised in Part One and Part Two be worth reconsidering in light of their practical merit in our way of knowing the world.

In the conclusion, therefore, my aim is to focus on a hope

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[300] Cf. my footnote 166 on page 176.

[301] Kuhn himself once remarks his role as a philosopher of science from other current schools of philosophy: "A consistent interest in historical problems and a willingness to engage in original historical research distinguishes the men he [Popper] has trained from the members of any other current school in philosophy of science. On these points I am an unrepentant Popperian."(CGK, p.236)

that we might persuade Kuhn and, especially, his opponents, to continue the debate in order to clarify and respond those questions I have addressed in Part Two. Namely, I propose, before dismissing Kuhn's position as one promoting irrationalism, relativism or even mythology, etc., analytical philosophers of science should be tolerant of Kuhn's 1947 experience. For it is the kind of experience that can sometime be experienced by "thinkers" of the sciences and, also, more importantly, by thoughtful common people. In this, a narcissist attitude, that is, an image of ivory tower philosophy, contributes nothing to help mankind rational.

In brief, Kuhn's approach after 1962 can be mainly divided into two directions: retreats and conversion.

In the direction of retreats, while complaining that he was always misinterpreted, Kuhn publicly has taken criticisms of his views seriously and has attempted to address himself to the criticism by retreating to his so-called 'earlier positions'. To be more precise, he retreats from the idea of "paradigm" to "exemplar", from "incommensurability" to "local incommensurability", and from "revolutions" to "language changes". These retreats initiated at the British Conference held at Bedford College, London, in 1965 and continued, at least, as late as the year 1982. It appears that the entire retreat is a shame for Kuhn in that, after the retreat, Kuhn has only one thing left to insist, i.e., the commencement of his career, the experience of reading Aristotle in 1947. Other than that, Kuhn has almost given up everything of his approach in 1962. What he committed himself to since 1965 seems a process of continuous modification of his positions in order to come to some compromises with his detractors.

However, in the other direction, the one of his conversion, Kuhn's modification appears not to retain the strength of his incommensurable experience of 1947. Rather, what Kuhn badly seeks is also a consensus with his opponents by converting into their methodological framework, the one of philosophical linguistic analysis. As a consequence, Kuhn's retreat becomes conversion that has said farewell to the historical hermeneutics which he once unconsciously but so brilliantly employed to interpret the history of science. In terms of this, his commitment to the structure of scientific revolutions is drastically watered down to the structure of scientific languages with which, he reports, there only exists 'local incommensurability'. To such an extent, Kuhn ceases talking about scientific revolutions, normal science, even exemplars, other than linguistic 'reference determinations'.

It does not matter, of course, whether Kuhn's conversion is reluctant or not, for it is certain that he no longer attempts to recapture the historical methodology he once employed in CR and SSR. What he contrives by employing the language of his opponents is merely in order to convince them: "I would", he says, "still discuss the special difficulties the sciences experience with holistic language change."(PSA, p.716)

The complex of Kuhn's retreat and conversion appeared as early as 1969 when he first employed Quine's concept of linguistic translation to establish his "therapeutic techniques" and thereby to solve the problems of incommensurability, the problems of communication breakdown. Introducing Quine's radical 'translation manual' to elucidate his notion of incommensurability, Kuhn viewed scientific communities as 'language communities' in 1969:

> What the participants in a communication breakdown have then found is, of course, a way to translate each other's theory into his own language and simultaneously to describe the world in which that theory or language applies. ... That sort of change, however, conversion, and the techniques which induce it may well be described as therapeutic, if only because, when they succeed, one learns one had been sick before. (CGK, p.277)

It seems that Kuhn realized that he himself had been in need of the same 'therapeutic techniques'. In 1977, he further moved in the direction of linguistic turn in order to cure his "sickness" through the identical process of therapeutic techniques. To an even larger extent, Kuhn states,

> One thing that binds the members of any scientific community together and simultaneously differentiates them from the members of other apparently similar groups is their possession of a common language or special dialect. ... new members acquire a set of cognitive commitments that are not, in principle, fully analyzable within that language itself. Such commitments are a consequence of the ways in which the term, phrases, and sentences of the language are applied to nature, and it is its relevance to the language-nature link that makes the original narrower sense of "paradigm" [exemplar] so important. (ET, p.xxii)

And he considers that this understanding of 'meaning change', being persuaded largely by the work of Quine, raises the importance of translation in his interpretation of science.

But, instead of reporting more on the progress of research on translation and its related project of computer programming, Kuhn in 1982 ended up by criticising the "Quinean Translation Manual". He took another shift to the stance of 'local incommensurability'. Seemingly along the line of hermeneutics, Kuhn conclusively claims that communication can only be established in the absence of Quine's translation manual.

Nevertheless, much of Kuhn's retreat as well as his conversion are unsuccessful. For example, his reasonable concession to 'local incommensurability' does not convince his opponents. As was discussed in Part One, Kuhn's linguistic and 'hermeneutic' incommensurability, the 'local ones', was hardly accepted by his opponents as possible alternative to their linguistic requirement.

In vain, both Kuhn's retreat and conversion are not only unwelcome by the mainstream of philosophy, but also score him a reputation that "often Kuhn's responses take the form that his critics have misinterpreted him"(SST, p.643) and that "Kuhn disowns most of the challenging ideas ascribed to him by his critics."(PR, p.51) Until 1982, Kuhn was in fact cornered into an awkward situation in which neither retreat nor conversion could sustain his approach to philosophy of science. It was a crisis of that approach. After that, his position becomes so opaque that nobody can discern precisely what he is up to. In other words, due to the fact that Kuhn has disowned almost all the insights of his 1962 achievement, nobody could distinctly tell what is the "paradigm" of his approach in and after 1982. Developed to this extent, his entire approach appears so loose that one would raise the same question Kuhn once posed to his opponents on the family resemblance: Your approach is now "similar with respect to what?"

Yet, there seems no clear answer for the question at least for now. For Kuhn himself, I guess, has not sorted everything out.[302]

Kuhn always contests that his presentations are drawn from work-in-progress. But it is obvious that they have not reached a conventional structure, and to such an extent that even he himself could not do 'normal science'. Thus, unfortunately, Kuhn presents us a Popperian example of continuous refutation or innovation without "normalcy". It is an example that has nothing but a ramshackle amendment to what cannot be reasonably figured out. This, in my judgment, is Kuhn's misfortune in his commitment to contribution to the contemporary philosophy of science.

However, as I have attempted to contest in Part Two, had Kuhn noticed the strength of his 1947 enlightenment, he would not have lost the debate so bitterly. That is one of the reasons why, in my conviction, Kuhn's retreat and conversion seem not only unnecessary but also avoidable, if remarking where is the real potency of incommensurability. In light of this, I assume that while Kuhn may be a successful historian of science, he is definitely not a successful philosopher of science. He presents a unique case of a philosopher who has been changing his ground for twenty years without any fixed ground. That is, we all know where his philosophy

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[302] Cf. Appendix 3. Kuhn is now working on a book mainly of incommensurability, and we hope he could have a good luck.

derived from, but few could grasp where he is standing now. Kuhn becomes a philosopher who deconstructs his own structure of discourse without a conclusion. Or put his own way in 1962, his approach appears to be in a process that has frequent small "revolutions", but lacks periods of "normal science".

Notwithstanding Kuhn is a philosopher who still insists on the notion of 'local' incommensurability in the domain of reading an out-of-date-text, he fails to retain and establish the theoretical meaning of this notion in its own right. Because, he has not clarified the philosophical implication of the notion. For he is not aware that, to confront incommensurability in communication or understanding, there are only limited options, that is, either undergoing conversion or staying on the line, or creating something different. All in all, one has to take a clear side to face the incommensurable properly.

In this connection, I presume that Kuhn's 1962 approach has been undermined not by his opponents but virtually by himself. This self-deconstruction is a pity for Kuhn's promising approach from 1947 to 1962 which has yielded one of the most important and most promising projects in the western contemporary philosophy.

Nevertheless, the entire process of this self-deconstruction has made me aware that paradigms and incommensurability are so closely related concepts in our philosophical thinking. In terms of this, the early Kuhn still holds the edge of contemporary philosophy, an edge on which few philosophers have done so much as he has synthesized on the epistemological issue of conceptual change in this century. It is with this understanding that the author of this essay stands firmly in the side of the early Kuhn against the later Kuhn, which consists of the essential stance of the dissertation.

Pragmatically, my view can also be supported by a set of random statistics on Kuhn's influence from 1962 to 1980:

WORKS ABOUT THOMAS KUHN[303]

Fields of Discipline	Number of Works
A. Philosophy of Science	102
B. History of Science	30
C. Sociology of Science	21
D. Sociology	28
E. Political Science	19
F. Economics	17
G. Psychology	8
H. History	7
I. Theology and Philosophy of Religion	7
J. Art and Literature	4

[303] All the data in the following chart are collected from the Bibliography to the book, *Paradigms and Revolutions*, edited by Gary Gutting, University of Notre Dame Press, 1980, pp.324-339.

This noninclusive chart demonstrate, as Gary Gutting notes, that "Thomas Kuhn's *Structure of Scientific Revolutions* (SSR) has had a wider academic influence than any other single book of the last twenty years."[304] Indeed, nobody could deny the reality that few books have been as widely read as SSR since it appeared: from 1970 to 1983, the University of Chicago Press sold more than 380,000 copies of its second edition (SSRE) alone. In comparison, the same publisher has printed a total of fewer than 18,000 copies of Rudolf Carnap's contribution to the *International Encyclopedia of Unified Science*.[305] It is obvious that, philosophers have to notice, without any help from the mainstream analytical philosophy, SSR gains its own philosophical life. For, reasonably, it provides the contemporary American mind with a vision which professional philosophers have not yet dispensed. Apparently, at least, it reflects a demand from the public that philosophy is supposed to be a pragmatic enterprise to meet the needs of all walks of life for improving our way of thinking.

And what is most interesting about the information provided in the chart is that intellectuals outside the departments of philosophy, not surprisingly, simply ignore the philosophical

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[304] Ibid., p.v.

[305] All figures referenced here is from D. G. Cedarbaum's article, "Paradigms", in *Studies of History and Philosophy of Science*, Vol.14, No. 3, 1983, p.174.

criticisms of the early Kuhn. Instead, they seek ways to transplant his ideas into their own disciplines to better understand as well as to better interpret what they cannot with the analytical tools offered by professional philosophers.[306] Although mainstream philosophers demean young Kuhn's commitment for the reason that it seems not worth the appraisal from outsiders, I deem that what Cedarbaum said in 1983 is pertinent to contemporary philosophical reasoning:

> That *Structure* is far more complex than it appears is the principal reason that no adequate appraisal of the book has been written.[307]

Now might be the right moment for mainstream philosophers of science to reexamine the shortcomings of its analytical tools, rather than to judge human experience with those tools without appropriate reason. And the shortcomings of analytical philosophy, as far as I can see through the debate, are threefold: its basic attitudes towards (1) experience, (2) history, and (3) western

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[306] It is an interesting fact of record, as I. B. Cohen points out, that whereas Kuhn's schema has been subject to considerable discussion, criticism, and approval by historians of science, the latter (including Kuhn himself) have tended not to make use of Kuhnian framework in their actual writings. Hence Kuhn's influence appears to be stronger among philosophers and sociologists of science (and scholars in wholly different areas such as political theory) than among scientists and practising historians of science. An exception, however, must be made for historians of the recent revolution in the earth sciences. (Cf. *Revolution in Science*, pp.xviii-xix.) Cohen is right, as a matter of fact, that in 1978 even Kuhn himself stopped employing his methodology, as I have pointed out in the thesis, in his book, *Black-Body Theory and the Quantum Discontinuity 1894-1912*.

[307] Ibid., p.175.

philosophies which grow out of academic branches and our way of everyday thinking other than of Anglo-American professional analytical philosophies. To be more precise, I consider:

I. The attitude of analytical philosophy of science toward *experience* is opposed to the pragmatic spirit which, I hold, ought to be an indispensable ingredient of contemporary epistemology. That is, in the opposite of American pragmatism that is initiated by questioning the universals, the contemporary analytical philosophy of science is in appeal to whether culture, history, society and idiosyncrasy etc. can be analyzed as elements of universals. But apparently each of these philosophical traditions arrives to different answers to the quest of universals.

For pragmatism, the quest of universal continuously concerns an attitude about whether philosophers of science can still put the philosophical cart before the experiential horse. In other words, given the numerous contemporary cases of defeating unanalytical philosophers of science such as Bridgman, Polanyi, and Kuhn, etc., philosophers outside analytical philosophy would, of course, doubt its attitude towards scientific conclusions which are derived from direct experience of science. They are conclusions that are perceived by those doing sciences. If analytical philosophers of science only choose those scientific representations they philosophically prefer and thereby rationalize them with analytical tools, people have reasons to question whether this kind of philosophy still intends to understand the first-hand experience from the point of views of practitioners of sciences. For example, in this context, analytical philosophers of science ought to answer why they can rule out the relevance of Kuhn's enlightenment in 1947 as an irrationalist element in developing scientific epistemology. If they can, then they might have to answer a further question: for what reasons? Because the experience of the practitioners is trivial or because it is defective? Analytical philosophers of science should also explain why they have the right to control philosophical presuppositions in their demonstrations in such a degree. Who delivers them the "rational right" to do so? If they could have the "right", however, it would involve a further appeal to challenge their rational requirement for analysis.[308]

The most consequential thing is, as Bridgman points out, that new kinds of experience are always possible, and therefore,

> we must be prepared for new facts, of an entirely different character from those of our former experience. ... It may perhaps turn out eventually that as a matter of fact nature can be embraced in a formula, but we must so organize our thinking as not to demand it as a necessity.[309]

This warning tells us nothing but that philosophy of science has to be ready to change with changes in scientific experience. To propose one philosophical method for all scientific experiences in

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[308] However, what I am interested in here is that attitudes like this attests to the existence of the phenomenon of incommensurability in the arena of philosophy. For here exist different attitudes towards scientific experience and, accordingly, different methods to tackle it.

[309] P. W. Bridgman, *The Logic of Modern Physics*, The Macmillan Company, 1927, p.2-3.

different dimensions will mislead young scientists into failing to understand the various practice of science.

Consequently, there seems no reason for Kuhn's experience of mental enlightenment not to be explained philosophically in epistemology. For, otherwise, the empirical appeal of contemporary analytical philosophy of science cannot be taken thoughtfully. Even granted that Kuhn did not conduct a successful defence of his approach, it by no means indicates that professional philosophers have provided alternative accounts of the experience to support their contentions. For the real story of Kuhn's discovery of how to read Aristotle remains significant. So, unlike what Suppe optimistically proclaimed, I suppose that the victory he declared was premature. Kuhn's experience of enlightenment has been knocking on the door to the analytical tradition.

Furthermore, if analytical philosophy of science takes experience as the real presupposition of its explanation of science, it should also seriously take elements of culture, history, society and personal idiosyncrasy into any account of epistemology of science. Only analytical tools are never enough to manage such an account of sciences. As Dewey notes,

> The history of science in its distinct emergence from religious, ceremonial and poetic arts is the record of a differentiation of arts, not a record of separation from art.[310]

That is, the history of science shows a collection and record of

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[310] John Dewey, *Experience and Nature*, Open Court Publishing Co., Chicago, 1925, p.388.

past experience. And to account for this part of experience, perspectives such as mental enlightenment, *Bildung*, and *Aufheben*, etc. should be paid more attention. In light of this, answers to rationality should be supplied only after philosophical research rather than labelling Kuhn's approach as irrationalism, relativism, and mythology beforehand only according to a presupposed universal rationality from nowhere.[311]

Also, I have to point out, philosophy can never be updated exclusively through philosophical training at departments of philosophy, the same as that science can never be developed solely through scientific education. For training, as I have stressed in Chapter 11, is always a prelude or preparation for young intellectuals to acquire basic aptitudes in listening to the call of experience, rather than in 'reasonably' tailoring fresh experience into the well-cut philosophical jackets.

II. With this understanding of the importance of experience, I surmise that contemporary Anglo-American philosophy of science has ignored its own ***philosophical traditions***, namely, its traditions which still have insights to offer to the contemporary mind. In certain degree, the contemporary philosophical mind, in

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[311] As a matter of fact, there is nothing wrong with so-called "irrationalism" as long as it can interpret the phenomena better than the "-isms" of rationalism. I take this as the spirit of contemporary pragmatism. And retaining this pragmatic attitude in the philosophy of science, I claim, is crucial. Also, this is the only way that the contributions of philosophers of science can be meaningful, not only to scientific communities but also to the human understanding of science.

particular the mind of analytical philosophy, seems too "formalized" to understand the importance of its history.

This negative philosophical attitude towards its tradition is typically, as I have discussed in Part Two, reflected in the ignorance of the humanitarian roots of Western thought. For, as far as we can trace its roots, philosophical discussions of cognitive 'enlightenment' dates from the days when Greek philosophers discovered a regularity in nature and concluded that its governing principle was the reasoning mind and when, prompted by Socrates, they turned to consider man and the high value of his intellectual powers. This tradition can also be found in the famous allegory of the cave in Plato's Book VII of *The Republic*, in which he notes that only through a painful process, which involves the rejection and overcoming of the familiar sensible world (very much like what Hegel described with the notion of *Aufheben*), the cave people illuminatingly begin an ascent out of the cave into reality.

For Aristotle, the philosopher of analysis, knowledge becomes something that a person has, which is in the activity of man, the activity of realizing a plan or "form" and of causing a material which has the proper potentiality, very like the way in building-up a house and in the making a statue, etc.

When this line is developed to the philosophy of St. Augustine, the concept of illumination is literally set in the centre of knowledge acquisition. This illuminationism is further emphasized by St. Thomas Aquinas through a demand of rethinking Aristotle's view of man, a rethinking in which anthropology and psychology form an important part of his philosophy. That is, in his psychology, there are two quite different "intellects" which are distinctive of higher human understanding: one is the abstraction of universal meanings from the individual presentations of sense experience; the other the synthesis of these abstracted meanings in the very act of cognition. And this activity is, for Aquinas, assigned to a different power, the possible intellect. Other than that, no special power is required for intellectual memory; the retention of understanding is thus explained by habit formation of human being in the possible intellect.

Down to the stage of Hegelian phenomenology, both abstraction and synthesis entangled as functions of *Aufheben* are, for the first time, put into a rational journey of "Notion" in history which heralds the aspects of contemporary hermeneutics to which Gadamer contributes the perspective of *Bildung* as its historical as well as philosophical basis.

It is obvious, however, that this legacy of Western thought has been somewhat neglected or lost, especially, in the arena of contemporary analytical philosophy. It is lost in the sense that this branch of philosophy is reluctant to admit the fact that sciences first are products of man and thus inevitably bond to be explained by the powers of human mind. Rather, analytical philosophy of science avoids tackling aspects of the real way of our everyday thinking in order to gain an reputation of "pure objective". No wonder analytical philosophers of science such as Suppe define that the philosophy of science is little more than an analysis of theories and of their roles in the scientific enterprise.[312]

But, without the knowledge of how those theories are established in the history of sciences, especially in the human mind, how could a philosopher of science justify a theory in its own merit? If philosophy of science only aims at analyzing theories without caring about how they are invented or interpreted, then in what way can scientists benefit from learning philosophy of science? And what is the distinction between philosophy and science? If philosophers of science cannot and do not intend to research the origins of scientific theories and their legacy in the history of philosophy, how can it offer students of science insights of how scientific theories were developed and what is their relation to scientific practice in historical terms?

Without sufficient and relevant information about how we really think, and also without a desire to learn from philosophical traditions, contemporary analytical philosophy cannot be one in need. That is, if a philosophy concerning science refuses to study its own legacy and to listen to professional scientists, who else can follow its approach to study history of science?

However, Kuhn in 1962 was an exception from those who ignored the traditions of philosophy. He is a pioneer of synthesizing not only the contemporary western views of scientific thinking, but also some western traditions of philosophic thinking (although he himself never realizes in the sense I have outlined). Thus the

[312] Cf. SST, p.viii.

young Kuhn's discourse was rich in the sense that it was rooted in learning from history and, what is more important, in willingly listening to science-in-action.

III. It is true that the early Kuhn's success largely benefited from his ***willingness to learn***, in particular, to learn from various European and Anglo-American traditions of thinking.

To the opposite, I deem that contemporary analytical philosophy of science lacks this willingness. Analytical philosophy of science seems so absorbed with its tools of analysis that it has no willingness to listen either to those who are doing science or to those who were philosophically developing philosophy of science with the help of the western traditions of philosophy.

It should be emphasized that being good at criticizing and defeating on other's views does not amount to the fact that the tools of analytical philosophy are the only acceptable tools of philosophical speculation. In view of this, I assume that a philosophy that can teach students of philosophy how to successfully criticize others and defeat opponents is not yet fully cultivated. Because, teaching them how to understand and how to communicate with opponents while confronting the incommensurable is a more substantial philosophical training. And this kind of training is the heritage of the Socratic dialectic method, a method that introduces an attitude of "willingness to learn" and thereby to synthesize unexpected truths. It seems to me, the kind of training is also of the basic philosophical manners, a manner in order to learn from others rather than simply to defeat others. Thus it is a training that seeks to keep conversation and communication going; it is a way of asking suggestive questions rather than making people think with the supposed "tools".

In my conviction, doing philosophy is never learning a basket of labels. Rather, learning philosophy, according to its traditional spirit in Hellenic times, is always to acquire a "good will", in the first place, in order to keep the conversation going so as to further synthesize new thoughts into one's own discipline. In other words, philosophers ought to be the most modest professionals in a society, because they always learn from other trades first and philosophically advise them later. Philosophy is supposed a trade that seeks to edify people by keeping conversations going with the subsequent benefit of this dialogue.

All in all, philosophy is not by any means a field in which only one voice can be heard. Of course, there was never only one voice in the history of philosophy. Hence, tools that enable one to criticize others are not the only philosophical means to equip the contemporary mind. In this, I contemplate that encouraging people to hear voices and to promote understandings by communications ought to be the basic intent of contemporary philosophy.

Of course, one does not have to agree with everything that Feyerabend and Michael Foucault say about institutionalized criteria of rationality with radical charges of capitalism, exploitation, and even with sexual repression. But they are probably not totally wrong in pointing out the fact that contemporary analytical philosophy of science has no curiosity to learn either from its own traditions nor from other cultures. However, I am not quite sure whether this attitude is directly because of sexual repression, exploitation, or capitalism, but one thing is certain for me that contemporary analytical philosophy of science has not yet prepared to loose itself in order to find its position in history.

Thus it is becoming an inquiry which talks about science, but almost without an audience in the sciences. To that extent, who else will care about what is going on within this inquiry except philosophers of science themselves? This situation encountered by analytical philosophy of science is crucial for the future of the inquiry. For the situation will doom the image of philosophies to such a degree that they will have little to offer to the real human thinking of sciences.

Therefore, defeating Kuhn's approach of 1962 is not the herald that contemporary analytical philosophy of science has won the audience of the scientific world. On the contrary, in certain sense, what the "defeat" sustains rather attests to Kuhn's "defeated" notions, paradigms and incommensurability, if one reflects the combat from a view outside the debate.

In sum, however, I envisage that there always exists hope to solve the conflict caused of incommensurability, for example, the one between the analytical philosophy of science and the historical philosophy of science represented by Hanson, Kuhn, Feyerabend, Foucault, etc.

But the essential condition, I am afraid, is a "good will" to listen in the first place, i.e. a willingness to communicate without any preconceptions. This willingness is the essential preparation to loose one's stand, for a while, so as to listen to and to learn each other from experience. For experience offers more to Suppe's "scientific realism" than the analytical tools which mainstream philosophy of science had supposed.

Why? As Johann Wolfgang Von Goethe once warns:

> ***All theory, dear friends, is grey, but the golden tree of actual life springs ever green.***

Appendix 1

Chuang Tong Li
Department of Philosophy
University of Ottawa
Ottawa, Ontario
Canada K1N 6N5

September 26, 1991

Dear Professor Thomas S. Kuhn,

I am the Chinese who wrote you of your presidential address at the PSA meeting last November. Here is the Chapter 2 (Chapter 1 is the introduction) of my thesis on your thought of incommensurability. It would be my pleasure if you could read the chapter and verify the facts about your attendance to the "shop club" meetings of "Unity of Science" around 1947 when you were in the Department of physics at Harvard. Also, it would be helpful if you could further provide more background information about your personal contacts or relations with Dr. Conant, Dr. Bridgman and Dr. Frank. This information would, I assume, should more light on your discovery of incommensurability (although you yourself never take the position as I understand). For me, your discovery of incommensurability is still, among others, one of the valuable contributions to contemporary philosophy of science. No matter you yourself accept this complement or not, I consider that you have responsibility to give us more details about the context of your experience of enlightenment in that hot summer day in 1947.

I am looking forward to hearing your answer soon. If you think it is convenient for me to personally visit you, please let me know the suitable time. Anyway, I am eager to get feedback from you on the impression of your reading my Chapter 2.

Best wishes.

Sincerely Yours

Chuang Tong Li

Appendix 2

20 November 1990

Mr. Chuang Tong Li
Department of Philosophy
University of Ottawa
Ottawa, Ontario
Canada KIN 8P5

Dear Mr. Li:

My presidential address at the PSA meeting in Minneapolis was made from my notes and is not available as a paper. In this form it is not available for distribution.

Sincerely,

Thomas S. Kuhn,
Laurance S. Rockefeller
Professor of Philosophy

Appendix 3

29 October 1991

Mr. Chuang Tong Li
Department of Philosophy
University of Ottawa
Ottawa, Ontario
Canada K1N 6N5

Dear Mr. Li:

I am late in answering your letter of 26 September because I hoped to find the time to write you at length about it. But that is clearly not going to be possible, and I must not delay any longer.

To the best of my recollection I was never a participant in the Unity of Science shop club that Gerald Holton speaks of. My relations with Dr. Conant were close: he introduced me to history of science, and I did my initial work in the field for the course he offered in the General Education program. I rapidly came to disagree with certain aspects of his philosophical position, but his role in my development was been crucial. With Bridgman I took an advanced thermodynamics course, and I ultimately read his book, but I never talked with him about his philosophical position. With Philip Frank I did talk from time to time, and I also read some of his work. Probably he contributed more than anyone else to my somewhat erroneous impression of the positivist movement. But there were many philosophers of science who influenced me more, even before I began to have thoughts of my own about the subject. I might add that of the three Conant assistants you mention -- myself, Nash, and Watson -- I alone was a graduate student, none of us was asked recruit a staff.

I enclose a copy of my talk to the PSA, which I've now written up for the proceedings. I do wish you good luck with my work.

Sincerely,

Thomas S. Kuhn,
Laurance S. Rockefeller
Professor of Philosophy, Emeritus

Appendix 4

Notes about the Chinese Traditional Medical Science

Although the Chinese traditional medical science has been practised mainly inside China and its culturally related oriental countries, its long history , over 2,000 years, has shown how successful it had been. It is an area entirely alien to the Western medical science, particularly in respects of its physiology, pathology, diagnosis and treatment. The elements of its theory are based on the early Chinese philosophy, the yin-yang doctrine, the theory of the five elements, the viscera-state doctrine, and the Jingluo doctrine.

1. The yin-yang doctrine takes the human body to be a unity of opposites. That is, it holds that the body is composed of two parts, the yin part and the yang part. The yang part includes the upper, exterior, back, outer side and the six hollow organs, while the interior, the abdomen, the inner side and the five solid organs belong to the yin part. There are also further divisions within individual organs such as the heart and the liver etc.

It is the balance between the yin and yang that ensures the normal function of the human body, and loss of this balance results in unhealthy evils which lead to disease. These unhealthy evils, too, are classified into yin evils and yang evils. Thus all diseases can be explained as the result of either yin-syndromes or yang-syndromes. Treatment is accordingly a matter or restoring the balance between the two.

2. The theory of the five elements holds that everything in the world is composed of five different elements: metal, wood, water, fire and earth. One of these elements can be found in each of the five solid organs of the human body: wood in the liver, fire in the heart, metal in the lung, earth in the spleen, water in the kidney. These organs are inter-related. The kidney essence nourishes the liver, the liver stores blood which is supplied to the heart, the heat generated by the heart warms the spleen, the spleen extracts vital substances from water and cereals to feed the lung, and the lung, in turn, assists the kidney by keeping the kidney fluid pure.

3. Pathological changes in one organ affect the other solid organs, the limbs, the bones, the five sensory organs, the nine orifices, the tendons and the blood vessels. These interactions are governed by the viscera-state doctrine. According to the doctrine, the eye is the orifice leading to the liver, the tongue leads to the heart, the ear to the kidney, the nose to the lung and the mouth to the spleen. As a result, eye disease is treated by clearing away the liver fire, kidney stone by compressing the ear, and so on.

4. The physiological and pathological concepts of Chinese medical theory are systematically different form those of Western medical theory. The Chinese theory allocates to a single organ functions which Western theory divides among different organs;

conversely, functions which, for the Chinese, are carried out by different organs the West attributes to a single organ. These differences arise because the Chinese theory of organs holds that they are as much physiological and pathological entities as they are anatomical entities.

As far as terminology is concerned, Chinese medicine has equivalents for certain Western anatomical terms, such as 'heart', 'liver', 'spleen', 'lung', 'kidney' (the five solid organs), and 'gallbladder', 'stomach', 'large intestine', 'small intestine', 'bladder', and 'triple warmer' (the six hollow organs). But when we come to the meanings of these terms, we find that they differ from Western anatomical terms, especially in their physiological and pathological connotations.

Take the heart. In Chinese theory, the heart is not only anatomical entity; it is part of the nervous system and can perform some of the functions which Western theory attributes to the cerebral cortex. The heart not only gives motive force to the circulation of the blood but also controls the mental and emotional faculties. So, the Chinese heart seems similar to the Western heart in respect of its cardiovascular function. But it substantially differs in its relationship to the cerebral cortex. One consequence of this difference is that Chinese pathology tends to be bodily holistic whereas Western theory tackles medical problems at the molecular and cellular level.

In 1973, it was suggested that the yin-yang doctrine might be explained by means of the Western theory of regulation, that is, in terms of C-AMP and C-GMP. But soon it became obvious that the Western theory of regulation does not match the Chinese one at all. For the holistic concepts of Chinese medicine should not be confused with atomistic Western concepts which only apply to particular physical structures. The kidney essence does not correspond to the DNA of the germ cells either. And the physical basis of the viscera state doctrine cannot be found in the workings of C-AMP and G-AMP, or nucleic acid.

The theoretical differences between Chinese and Western medicine explain the difference in the principles on which their diagnostic and clinical practice are based. For example, Chinese medicine holds that the spleen is responsible for transport and conversion: it effects the upward movement of vital substances, and controls the blood and the mouth is the spleen's orifice. What is more, the physiological state of the spleen is reflected in the lips and the muscles around the mouth. Therefore, the important symptoms of spleen disease are abdominal distention, loose stool, KX,!,, phlegm-retention, oedema, diarrhoea, blood in stool, purple macule, flaccidity of the muscles, general fatigue and the colour of the lips. All in all, the cure for these diseases lies in the nourishment of the spleen.

Generally speaking, all symptoms are classified into eight principle syndromes: the yin, yang, superficies, interior, cold, heat, asthenia and sthenic syndromes. Of the eight principal syndromes, the yin and the yang are the leading ones, with the former governing the superficies, the sthenic, and the heat, while the latter governs the interior, the asthenia, and the cold. Different principles of treatment are applied to the different syndromes, e.g. the cold-heat principle, the asthenia-sthenic principle, and the yin-yang principle. Accordingly, herbs with different properties are prescribed for the different diseases. And these herbs are unlike Western medicines in their mode of operation. For example, when an excess of yin leads to a weakness of yang, herbs are administered to restor the balance.

Chinese medical science as only one of paradigms in Chinese culture has been functioning efficiently in countries influenced by Chinese culture such as Japan, Singapore, Vietnam, Korea etc. Its practice shows that it has been a completely different world of science. Not only that, there is a fact that, without Western academic acknowledgement, Chinese medical science as booming business is gradually spreading all over the world. Whereas on the other hand, ironically, there is another fact that the main stream philosophers of western sciences are merely enjoying a narcissist dream which somewhat reflects the traditional Chinese image of the nation that ambitiously paints China as "the Central kingdom of the globe". Unfortunately, the narcissist dream is itself an incommensurable entity both to its own historical and Chinese traditions of sciences.

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