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The Impact of Kuhn's *Structure of Scientific Revolutions* on Sociology of Science

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ABSTRAK

Para pakar sosiologi sains telah merujuk buku Kuhn, The Structure of Scientific Revolutions, *lebih banyak daripada buku-buku lain. Penulis mengkaji dampak buku Kuhn itu terhadap pakar-pakar sosiologi yang mengkaji norma, pencapaian, penilaian, pengkhususan di samping amalan makmal, perubahan kepercayaan, amalan dialog ekonomi dan kepentingan. Penulis menunjukkan bahawa wawasan Khun sememangnya pada suatu ketika memajukan sains sosiologi tetapi sekarang ia telah menghalang perkembangan itu.*

ABSTRACT

Kuhn's Structure of Scientific Revolutions *has been cited by sociologists of science more than any other book. In this article, the author examines the impact of Kuhn's* Structure of Scientific Revolutions *to sociologists studying norms, deviance, performance, evaluation, specialities and to those studying laboratory practice, belief-change, discursive practice, interests and economics. The author argues that Kuhn's theoretical framework and conceptual lexicon once enabled fruitful sociology of science but now impedes it.*

According to Merton (1973: 270), sociologists of science are interested, among other things, to study science as "an extension of certified knowledge". They are also interested to analyze the process of change in the scientific enterprise. Science as perceived by the 'positivists' is imbibed with several assumptions which underdetermined the process of scientific change, *the process from science at* time one to *science at time* two. The term 'positivists' is meant to be applicable to those who share these assumptions. The assumptions hold by the 'positivists', which I will explicate later, take for granted that this important process as uninteresting, to say the very least. In fact these assumptions stultify sociology of science. Kuhn's *Structure of Scientific Revolutions* (SSR) is so important because in one swift move, all of these assumptions that are promulgated to legitimize science are not only questioned but are also

shown to be inconsistent with the process of scientific change-that there are good ground to examine this change as sociologists of science suggest. In what follows, I will argue that Kuhn's SSR has been cited by sociologists of science more than any other book because of this very reason.

What are the assumptions of the 'positivists' that sociologists think truncate the set of questions that can be asked about the process of scientific change? First and foremost, it is the assumption that change is necessarily progressive. Another assumption is that the process of change can also be characterized in a rational fashion-that there is a logic undermining the process. Still another assumption is that the process is cumulative-each bit of scientific knowladge contributes to the greater whole. Not only the path is cumulative, say the 'positivists', it is also linear. Implicit in this linearity is the belief that scientific change is a process from total ignorant to total knowledge. In addition to that, the path to true knowledge is via science, more than anything else. Science is uniquely suited and it is better than any other form of investigation (including sociology of science). Put differently, science is superior to any other form of knowledge. Still another assumption is that the change in scientific knowledge is an objective process, that is, the transition from one set of belief to another can be described objectively. The 'positivists' also assume that the process is nomological. As a result, F=ma is true irrespective of time and place. The 'positivists' also maintain that the change is directly related to the notion of usefulness-it is not the case of 'more of the same'. Perhaps worst of all is the assumption that scientists discover-that it does not make sense to talk about constructed knowledge. In other words, to the 'positivists', truth is found and not made. The assumptions just mentioned are by no means exhaustive but I believe they are the major ones.

It is clear that these assumptions prohibit anyone from posing intelligent questions on the nature of scientific change. In science, posing the right questions can be more important, more revealing, than providing the answers. For example, Merton (1973: 453) argued that outstanding scientists lay great emphasis on the significance of problem-finding than problem-solving. Questions provide the impediments to a greater understanding on the nature of science itself. Is it really that scientific change is rational and not irrational? How can one be absolutely certain that observations are not theory-laden? And for that matter, is it not plausible that science develops in a haphazard fashion instead of the consequential, cumulative and linear way as claimed by the 'positivists'? These are legitimate questionss that anyone can ask about science. Just as scientists can perform experiments and examine their experimental objects in great detail, it is perfectly legitimate for anyone to

examine and scrutinize science in any systematic manner. The problem with those assumptions is that they permit the first mode of inquiry done by scientists but it prohibits the latter, the kinds of questions asked by sociologists of science. Clearly, to me, this is a double standard because it is not symmetric. Any sociological work on science more often than not will put those assumptions to trial and what better way to do it than to take into perspective an account given by a historian cum philosoper of science which, in its own right, demolished the 'positivists' assumptions? In a sense, sosiologists of science find the tool to demolish these assumptions in Kuhn's SSR.

In his SSR, Kuhn argues that science evolves when there is a consensus among scientists about basic ontological commitments, explanatory principles, general methodology, research priorities and guidelines which should be followed, in other words, when scientists share a paradigm. Scientists' sharing a paradigm are in the stage of normal science. Elements in a paradigm include the scientists' tacit knowledge. As a result, scientists cannot articulate what they believe nor can they easily envision alternative ways of doing science. According to Kuhn, most if not all scientists occupy the space of normal science. Normal science is characterized not only by a shared paradigm, but also by disciplinary matrix "...'diciplinary' because it refers to the common possessions of the practitioners of a particular discipline; 'matrix' because it is composed of elements of various sorts..." (Kuhn, 1970: 182). Other elements of normal science are examples which are established achievements serving as guides to solving new puzzles. Puzzles are problems arising in a paradigm within the terms set by the paradigm. Experiments are conducted not with the main purpose of challenging the established facts, they are performed in order to find more theories that are consistent with the established facts. For example, in the Ptolemaic astronomy, the main task of the astronomers is to develop another epicycle.

Moreover, if there is a contradiciton between the result of an experiment and the established so-called fact, the scientist is to be blamed. Kuhn's paradigm functions in a tacit way. The paradigm functions very well until scientists in their collaborative efforts have a puzzle that does not fit. This is where anomaly accurs. An abundance of anomaly, however, does not necessitate a scientific revolution. A crisis is what is needed. Scientists begin to question their basic assumptions and different paradigms emerged. This is followed by a clash of conflicting, incommensurable paradigms, with a final victory of a single paradigm. Thus a scientific revolution has occurred and scientists experience a gestalt switch. Following the revolution is again the normal science stage. Kuhn maintains that this cyclical process goes on continuously.

From Kuhn's account of the changes in scientific knowledge, it is clear that scientists do not adhere to a set of eternal assumptions precisely because the assumptions keep on changing. Kuhn's SSR also underscores the fact that the changes are not linear. It is also not obvious that the transition from one set of belief to another can be described objectively because Kuhn's model does not guarantee the continuity of one belief in another. Putting it differently, scientists subscribe to a particular set of belief at a point of time can have a totally contradicting set of belief at another. The superiority of science is questioned too. There is no solid ground to claim the unique superiority of science over any other form of knowledge since the objectivity of science is very much questioned. The content of science is no longer necessarily nomological because what is considered true previously can turn out to be false. So the scientists' claim that their experimental results are consistent with or correspond to the external world is no longer true. Truth is now made and not found. It is not the case that scientists discover but scientists construct. It is as if science is contingent on space and time-that it is locally defined. Furthermore, if paradigms are indeed incommensurable, how can each bit of knowledge is said to contribute to the greater whole? So one cannot say that the change is cumulative. Thus from just a single work, Kuhn has demolished all the basic and popular assumptions about science and opens the door for an inquiry into the very nature of change in scientific knowledge. It is an invitation especially to the sociologists of science because all of the assumptions that they detest are shown to be, at least, very questionable.

Now let us see the impact of Kuhn's SSR on sociologists studying norms, deviance, performance, evaluation, specialties, laboratory practice, belief change, discursive practice, economics and interests.

To sociologists studying norms, Kuhn argues that norms are organizationally contingent and have lives of their own. They are part and parcel of normal science. Kuhn's 'disciplinary matrix' refers to shared elements in a social group which include values (Kuhn 1970: 184). According to Cole and Cole:

> ... Kuhn acknowledges the important role that social variables play in the evolution and transition from one scientific paradigm to another. He implicitly suggests the importance of the values of scientists and the socialization process....(Cole and Cole 1973: 5).

The forms of injunctions can vary between one normal science and another. There are different appropriate ways scientific goals should be pursued. So the study of norms is more than a legitimate enterprise; it is an important endeavor in so far as we want to know more about scientists and their products.

He argues in a similar fashion to sociologists studying deviance or transgression. Kuhn maintains that since scientists are, to a certain extent, unsure whether the results of their experiments negates or nullifies the established facts within a paradigm, there is a tendency to suppress the results because whenever there is conflict it is the scientist who is to be blamed and not the exemplars. So there are secrecy in science. Likewise, there is dogmaticism, as an implication of the clash of paradigms and their incommensurability. For instance, Kuhn writes:

> Lifelong resistance, particularly from those whose productive careers have commited them to an older tradition of normal science, is not a violation of scientific standards but an index to the nature of scientific research itself (Kuhn 1970: 151).

Trimming data, for example, are expected since scientists use exemplars set by their leaders as their guides. Whenever they cannot reproduce the results, it is expected that something is wrong with their data.

To sociologists studying performance, Kuhn argues that the scientific community is akin to a social organization. It has organizational structure, hierarchy and stratification of members. One needs to consider the social environment of the scientists such as their graduate school environment. Thus echoing Kuhn, Fox writes: "...Most scientists do not significantly alter their ideas, approaches and commitments after graduate school" (Fox 1983: 291). There is the organizational ladder for the members to climb. So scientists must have ways of measuring their performance. Sociologists can give a valuable input on whether, for example, one method of measurement is better than the other. Sociologists of science, to give another example, can also give valuable input pertaining to the question of whether teaching accomplishments have equal value with puzzle-solving. One can find for example Hagstrom summarizing Kuhn (1970: 35-42), that "research is in many ways a kind of game, a puzzle solving in which the solution of the puzzle is its own reward" (Hagstrom 1982:25).

Kuhn contends that sociologist studying evaluation through the study of citation, can give us an insight into the scientists organizational structure because there *is* such structure among scientists. Crane, for example, writes:

> Kuhn (1962) has argued that groups of scientists develop shared definitions of their work, paradigms which interpret findings and guide new research. In other words, scientists adjust to the problems of dealing with knowledge in their fields by forming social organizations of various kinds, based upon shared interpretations of the situation (Crane 1969:335).

For instance, sociologists can study whether there is a strong correlation between recognition, diffusion and utilization (Cole and Cole 1973: 34-35).

Regarding the study of specialties, Kuhn maintains that the existence of collaborative effort among scientists indicates a preference of area of interest and confirms the heterogeneity of the scientific enterprise. There exists some kind of division between theorist, empiricist, phenomenologist *et cetera*. Through the study of citation, sociologists can identify the clumping of different scientific ideas. Sociologists need to explain the reason for such a coagulation of scientific activities, the occurrence of a sort of department within a department. Through specialty one can make a link to Kuhn's paradigm and normal science in the sense that those scientists within one specialty shared a common set of beliefs. According to Kuhn (1970: 49), "...substituting paradigms for rules should make the diversity of scientific fields and specialties easier to understand. Also, social and cognitive factors within the paradigm can indeed be separated. Thus one can find that Gilbert in his article on 'The Case of Radar Meteor Research' writes:

> ... it has become clear that a sociological study of academic science should pay attention to the relationship between the social institutions of science and the scientific knowledge which is produced...By considering in some detail the emergence of one area of research, this paper tries to isolate some of the social and intellectual factors which affect the direction of scientific progress. Particular attention is paid to the growth of knowledge, the web of social relationships in which researchers work, and the interaction between these two (Gilbert 1976:187).

Like norms, specialties have life cycles too- similar to Kuhn's normal science. When there is a new specialty formed, there is a development in science, signifying changes in scientific knowledge and this is parallel to the emergence of a normal science or a new paradigm. When sepcialists refuse to listen to others outside their circle, Kuhn explains that it is expected for them to behave in such a manner because two paradigms are indeed incommensurable. They do not share the same paradigm.

What are the assumptions of those studying laboratory practice? Perhaps the most important assumption is the inseparability of the socio-cognitive nature of scientific activity. Scientific activity should not be seen from its social and cognitive constituents on the further assumption that each part only makes sense within the vocabulary of the paradigm as a whole. In a revealing passage in the SSR, Kuhn writes:

> ... the process of learning a theory depends upon the study of applications, including practice problem solving both with a pencil and a paper and with instruments in the laboratory. If for example, the student of Newtonian dynamics

ever discovers the meaning of terms like "force", "mass", "space", and " time", he does so less from the incomplete though sometimes helpful definitions in his text than by observing and participating in the application of these concepts to problem-solving (Kuhn 1962: 47).

Sociologists studying laboratory practice assume that science is more of a process than an output. By putting science *in situ*, in the minutiae of the scientific laboratory bench, sociologists have direct access to the scientific process that needs to be described. Scientists' so-called facts, publications and technology, cannot be properly understood unless the processes by which observation and conclusion come to closure are studied. In fact, it is argued that reality is arti-factual because when scientists go into the lab, they don't see nature *de jure*-nature are made rather than given in the laboratory. Knor-Cetina, for example, writes:

> ... I have proposed that scientific inquiry displays itself in actual scientific work as constructive rather than descriptive, and I have specified constructivity in terms of the 'decision-character' of inquiry work (Knor-Cetina 1982: 123).

The set of decisions scientists' make are 'impregnated operation' so to speak. Scientific practice is transformational-there are various stages the data have to go through inside the laboratory before decisions can come to closure. Instead of fiddling with nature, it is shuffling of texts. And at the end of the process, scientists claim that the world is like what they have shown tc be. It is from this kind of process, scientists believe, that ultimate reality comes from. What does Kuhn have to say to all these assumptions? In the first place he will say that since there is always anomoly and crisis in the process of scientific change, so whatever scientists claim as facts, as corresponding to the real world, is not really what the scientists think the facts are. In other words, those so-called facts may very well be wrong. How then are we to explain this unfortunate conclusion? The ethnographer can provide a reasonable explanation. It is simply because those so-called facts are constructed; they are not discovered. They are made and not found, legitimized by the process of decision making inside the laboratory. In other words, the scientists' experimental results are context-determined. What comes as evidence is itself a question of paradigm. Furthermore, Kuhn's expression about the thing to blame when scientists fail to get results-like the "carpenter who blames his tools" (Kuhn 1970: 79), underscores the extremely close association between scientists and their instruments. Accordingly, the ethnographers' assumption that the scientists are not trying to discover but to manufacture knowledge by manipulating things and machines cannot be taken lightly. After all, what is a laboratory if it is not an instrument?

To those sociologists studying belief change, Kuhn will say that beliefs are contingent on paradigm. As a matter of fact, what constitutes belief can come only from paradigm. One should not differentiate between the social and cognitive parts of the paradigm. Sociologists can study the nature of the paradigm which is related to the set of beliefs. When crisis appears, scientists', conviction begin to be shaken. When there is a clash of paradigms, there is a clash of beliefs because of their incommensurabilities. The victory of one paradigm over the other denotes change of belief. So the process from one paradigm to another is a process of belief-change.

What are the assumptions of those sociologists studying interest? In a nutshell, they assume that scientists occupy certain social positions. These social positions give rise to interest-a certain set of social positions creates a certain set of interests. There is a congruence between interest and social position. These interests constraint the set of appropriate beliefs scientists have, ending with a particular scientific belief. And last but not least, the model works backward, that is, beginning with belief and terminating in social position. The model is not deterministic in the sense that there is no necessary connection between social position and belief. It is also not the case that interest depends on statistical evidence; it depends on choice. Furthermore, interest, unlike motivation, should be viewed from the social level. How are these assumptions related to Kuhn's SSR? First and foremost, since the model starts with belief, the sociologists have to take into account the paradigm the scientists is in, that is, the sociologists should pay heed to the models of reality the scientist is working with. As before, sociologists should not separate the social and cognitive part of the paradigm. These parts should be viewed as an integrated whole. In the study on eugenics, MacKenzie (1981: 28 - 31) argues that Pearson believes it is possible to generate smarter people by breeding because if eugenics is embraced, Pearson will rank high in the eugenically defined society. Eugenics enhance the power and resources of the middle class, to which Pearson belongs. Also the genesis of the new Pearson R theory, for example, can be construed as an intrusion of an idea that is contingent to the society writ large – one of the possible sources of Kuhn's anomaly.

Kuhn's notion of paradigm is likewise applicable to sociologists studying socio-economics. Let us consider the case of Furnivall, who espouses the concept of the plural society. Furnivall explains:

> In Burma, as in Java, probably the first thing that strikes the visitor is the medley of peoples European, Chinese, Indian and native. It is in the strictest sense a medley, for they mix but do not combine. Each group holds by its own religion, its own culture and language, its own ideas and ways. As individuals they meet, but only in the market place, in buying and selling. There is a plural society, with

different sections of the community living side by side, *but separately*, within the same political unit (Furnivall 1956: 304).

Religion, culture, ideas and ways are constituents of Kuhn's paradigm. True to Kuhn's belief that paradigms are incompatible, each group in the plural society adheres to a different set of constituents and as a consequence, the society "is broken up into groups of isolated individuals..." (Furnivall, 1956: 310) to the extent that "as individuals their social life is incomplete." (Furnivall 1956: 306).

Sociologists of science studying discursive practice hold a different set of assumptions than those studying interests. To begin with, the sociologists studying discursive practice claim that they are attempting to identify and describe the manner in which scientists' accounts are organized to reflect their actions and beliefs. For instance, Mulkay and Gilbert writes:

> [Discourse analysts] concern themselves ...with describing the interpretative methods which are used, not only by participants but also by traditional analysts to depict scientific action and belief in various different ways [but] does not seek to go beyond scientists' accounts in order to describe and explain actions and beliefs as such (Gilbert and Mulkay 1984: 14).

We need to recognize, the sociologists go on to say, the facts that actors (scientists) themselves are constantly engaged in doing sociology. There is no reasonable way to separate the actors' actions and beliefs from their accounts. The existence of multiple accounts precludes the traditional sociologists' accounts because in so doing, traditional sociologists are privileging one account over another. In elevating the actors as the touch stone, sociologists studying discursive practice are taking for granted that actors do provide a cultural repertoire whenever they give their accounts. Actors' accounts become the topics and not the resources, that is, the sociologists treat the actors' accounts as a phenomenon in its own right. Also particular accounts of actors are construed to be contextually contingent. Last but not least, the sociologists assume that actors' beliefs and actions are surrounded by interpretive reflex. Now, how is Kuhn's paradigm related to the program of, the sociologists of science studying discursive practice? Since actors' accounts are crucial to the sociologists' program and these accounts are contextually contingent, sociologists studying discursive practice need to identify the elements that constitute each paricular context or for that matter each particular paradigm. And to view beliefs and actions as parasitic to the actors' accounts is consistent with Kuhn's hypothesis about scientists operating under normal science-that scientists' worldviews depend on the set of assumptions, including belief, which scientists have.

Thus far, I have try to illustrate how Kuhn's theoretical framework assists, legitimizes and even paves the way for sociologists. Could the Kuhnian conceptual lexicon, in any way, impede fruitful sociology of science? Before I go further to answer this question, there are a few things that have to be made clear. One have to ask in what way, if indeed there is any, one can impede sociology of science. I can come up with only two ways to this in so far as Kuhn's work is concerned. The first way is by arguing that the process of change in scientific knowledge is only partially relative, that is, there are some objective criteria determining the change. The second way is to argue that even though there are some elements of subjectivity guiding the change, these elements are only significant at the individual level. I will argue that both of these methods bear negative consequences to the development of sociology of science.

The main problem with Kuhn's theoretical frame and lexicon, to me, lies with his vague notion of the incommensurability of paradigm and normal science. The interpretation of these concepts, to a certain extent, has positive effects to sociology of science as I have shown in the earlier paragraphs. There is, however, another way to interpret them. Let me take the notion of the incommensurability of paradigm to begin with. The question is, are paradigms, absolutely incommensurable to each other? One interpretation is to say that they really are. It is only from this perspective that it makes more sense to talk about 'the clash of paradigms' and scientific revolution. Another interpretation is that there are links between paradigms-scientists from two paradigms still have shared commitments. Furthermore, the commitments transcend scientific revolution. To elaborate this, Kuhn himself states:

> These five characteristics- accuracy, consistency, scope, simplicity, and fruitfulness- are all standard criteria for evaluating the adequacy of a theory....they play a vital role when scientists must choose between an established theory and an upstart competitor. Together with others of much the same sort, they provide *the* shared basis for theory choice. (That there are links between paradigm) (Kuhn 1977: 322).

It is the latter interpretation, to me, that can have negative effects on the sociology of science because it constraints the notion that the process of scientific change is relative. Consequently, it makes less interesting to ask whether scientific knowlegde is constructed, whether scientific theories are indeed nomological, whether scientific change is cumulative, whether norms vary with different scientific communities, whether the process of change could be irrational, whether observations are value laden *et cetera*. This interpretation pretends to have the answers to these kind of questions because of its contention that there *must* be some kind of objectivity in the process of change. What is more, this interpretation

clearly precludes the possibility that the change could be relative. This interpretation closes the door to investigate the possibility empirically.

Let us examine Kuhn's notion of normal science. The first interpretation is that individuals, by and large, share the same values. The second interpretation is to maintain that idiosyncratic factors contingent on individual biography and personality play an equally significant role. Says Kuhn:

> ... the choices scientists make between competing theories depend not only on shared criteria- those my critics call objective - but also on idiosyncratic factors dependent on individual biography and personality (Kuhn 1977: 330).

Accordingly the second interpretation necessarily relegates some of the values, if not all, to individual levels. Rather than viewing the scientist as part and parcel of the community doing normal science, this interpretation gives equal importance to the individual scientist. In other words, scientists are no longer strongly attached to the institutionalized norms. So why is it important to study the different appropriate ways institutional goals should be pursued, the standard which scientists use to guide their behavior and the forms of injunctions since all of these can be relegated to the individual scientist regardless of time and place? Clearly this interpretation of Kuhn's normal science have negative impact on the development of sociology of science, thus impeding its development.

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