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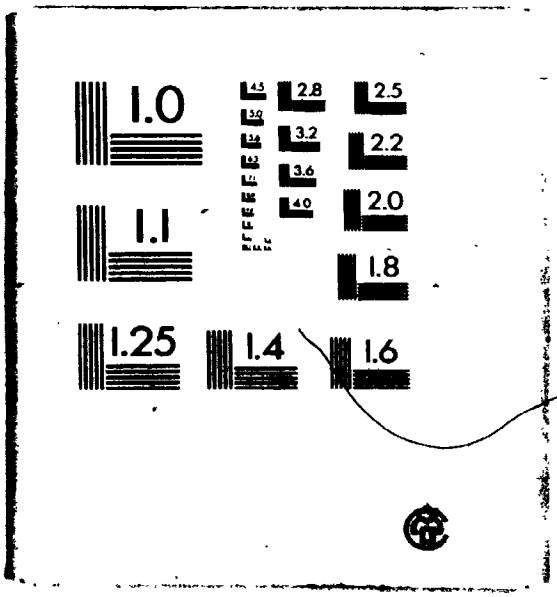

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TOWARDS A SOCIOLOGY OF SCIENCE: A COMPARATIVE ANALYSIS OF MERTON AND KUHN.

by

© Steven Ivor Salmond, B.A.

A thesis submitted to the Faculty of
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of the requirements for the degree of
Master of Arts

Department of Sociology and Anthropology

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Ottawa, Ontario

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The undersigned recommend to the Faculty of Graduate Studies acceptance of the thesis:

"Towards a Sociology of Science: A Comparative Analysis of Merton and Kuhn"

submitted by Steven Ivor Salmond, B.A., in partial fulfillment of the requirements for the degree of Master of Arts.

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ABSTRACT

The thesis outlines a research program for the sociology of science. The basis of this effort is a specification of certain conceptions of science which have emerged from the most influential works in the area, those of Robert Merton and Thomas Kuhn. A comparison of their respective works reveals that both analyze science as social activity, concentrating upon the structure of scientific groups to describe the development of science. Kuhn's analysis also involves conceptions of science as knowledge, and as rules. Combining his approach with that of Merton provides insight into the internal dimensions of scientific activity. External factors also influence that internal structure and therefore adding consideration of the various institutional settings within which scientific activity takes place to the combined Merton and Kuhn analyses will permit a more complete sociological analysis of science.

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Introduction

This thesis will specify a program for the sociology of science. The tenets of the program will be constructed by tracing the historical developments in this field through illustration of the two major schools of thought which have emerged - the first represented by Robert Merton, and the second by Thomas Kuhn. Merton's work in the sociology of science, which began in the 1930's and has continued to this day, stood as a virtually unchallenged exemplar for over twenty years. Kuhn's work, though originally intended as a contribution to the history of science, surfaced in the early 1960's as a major force in the sociological analysis of science - in the process becoming ammunition for critics of Merton and simultaneously representative of a new image of the nature of science. A debate quickly emerged, the established Mertonians, on the one hand, and the fledgling Kuhnians on the other. This helped to escalate interest in the area - volumes of literature were published extolling the virtues of one side over the other. Through all of this intense concern to have one triumph over the other, the battleworn opponents have until fairly recently failed to see that the two images of science may not be quite as distinct as they first thought, that in fact many similarities exist between the two schools. True, differences are still evident, and will always be so because the fundamental nature of the arguments are quite opposed to each other. However, if we examine Merton and Kuhn for their similarities as well as differences, then a pattern emerges which indicates how the sociology of science has developed over the years, and how the image of science presented by Kuhn is really an improved form of analysis built upon that of Merton. In a chronological sense, Merton can be seen as beginning the process of conceptual-

ization for the sociology of science, and Kuhn can be seen as extending that process towards maturity. It is the purpose of this thesis to extend this development even further.

To accomplish this task, the approaches of the sociologist Merton and physicist turned historian Kuhn will be examined in terms of a) an image of science; b) a comparison, intended to illustrate similarities and differences in the respective arguments; and c) the issues they address as a sociology of science. This should not only illustrate the role which the two have undertaken in the development of the sociology of science, but should also make evident their respective limitations in that regard. The crux of these limitations is that both Merton and Kuhn concentrate their arguments upon the internal dimensions of science, and give little heed to the influence of external factors - in other words, they view science as autonomous, instead of considering it as part of the wider society. Science is at the same time separate from and part of the overall society, and must be considered from both aspects. It is on this premise that the move towards a sociology of science will be made, which will involve a synthesis of existing approaches and the introduction of the role external factors play in the development of science.

Chapter One will discuss Merton's work. His approach focuses upon the social organization of scientists - a conception of science based on the premise that science is a social activity, and emphasizes the importance of analyzing the processes which account for scientific development. Identifying the normative structure of science is Merton's goal, which he attempts by systematically describing a set of norms that guide scientists in their pursuit of original knowledge. Motivation for expression of commitment to this system of norms stems from the promise of reward. Merton des-

cribes the operation of the reward system in science, and relates this to scientific achievement. Science develops through the incremental accumulation of original knowledge, and scientists are rewarded for their contributions. Adherence to a set of norms fosters the best climate for the creation of new knowledge - thus, Merton is able to link the normative structure and reward system, which together provide a basis for Merton's study of science as social activity.

The Mertonian school placed what many (notably the Kuhnians) considered an undue emphasis on the objective nature of science. Coupled with a description of the social organization of science as being free from damaging (from Merton's viewpoint) subjective influences (for example, science for profit, science for political power, and so on) led to a general confusion of the ideals of science with its actual operation. Merton could not overcome the obstacle (indeed, he did not see it as such) of reconciling the subjective side of science with its gallant attempts to be purely objective and thus removed from many of the problems that plague other institutions (witness the debates over what constitutes legitimized knowledge in religion, for example). Merton's sociology of science is therefore faced with a conceptual block.

Kuhn makes some effort to remove this obstacle, and it is to this effort we turn in Chapter Two. He attempts to explore the subjective aspect of knowledge development in science by relating it to social conditions. Focus is given to both the social organization of science and the role which bodies of knowledge play in its operation. Kuhn's approach relates the social dimensions of science to bodies of knowledge and rules of procedure. Like Merton, Kuhn's work is premised on the assumption that science is a social activity. Unlike Merton, however, Kuhn seeks to describe the charac-

teristics of knowledge and rules that lead to the development of science. In order to accomplish this, he breaks scientific activity into two types: normal and revolutionary. During periods of normal science, paradigms provide guidelines for the nature and content of activity in a given scientific community. Paradigms are bodies of knowledge, and serve to generate problems which scientists attempt to solve. When a particularly difficult problem emerges that cannot be solved within existing criteria, then a revolution may occur, whereupon one paradigm is replaced by another that permits resolution of the crisis. How this type of change comes about is a major concern for Kuhn, which he analyzes by describing the nature of the scientific group in relation to the characteristics of the bodies of knowledge utilized by that group.

Kuhn's image of science is the recipient of much criticism, the thrust of which is largely aimed at the concept of paradigm and the implied relativism of his argument. We will note these criticisms, and discuss Kuhn's efforts at response. These efforts change the original argument somewhat, and help to clarify the sociological approach of the work.

Chapter Three provides a comparison of Merton and Kuhn in terms of the normative dimensions of science. The comparison is intended to demonstrate that there are sufficient similarities to combine the two approaches in order to arrive at a more complete sociology of science than either provides individually. Several aspects of the normative dimensions of science will be examined.

The concept of <u>scientific community</u> is the basic unit of analysis for both Merton and Kuhn. Scientific communities are the social groups that produce knowledge. Each breaks the concept down into different levels, from the global community of scientists, to individual disciplines, to spec-

ialties, and finally to informal groups of scientists retaining common research interests - in order to analyze the relation of the structure of scientific activity to the advancement of science.

Both Merton and Kuhn attempt to describe the nature of scientific activity by identifying the normative structure of science. The social organization of science is structured by a system of norms and values shared by groups of scientists. Though the nature of the two arguments are similar in this respect, the means by which they describe the normative structure are quite different.

Merton's emphasis upon a normative framework is evident by his description of the 'ethos of science'. The norms he describes are solely concerned with the social aspect of scientific activity. Kuhn includes the dimension of knowledge in describing his version of the normative structure of science, which can be seen through his concept of paradigm.

There is a close parallel between a description of the normative structure and a demonstration of social control in science. Both Merton and Kuhn seek to illustrate the forces of social control within the operation of a normative structure. Whereas Merton locates the sources of control in the relation of the 'ethos' to the reward system of science, Kuhn goes beyond this by adding the role which knowledge and rules perform, placed in the context of a paradigm.

Kuhn and Merton present similar arguments concerning the role of competition in science. The development of knowledge can be viewed as a competitive activity, which greatly influences the structure of the social organization of science. Scientists compete for the solution of particular problems, both in groups and individually. They thus tend to concentrate research upon the solution of what are considered important problems, allow-

ing science to advance more rapidly than would occur if scientists conducted research in a more haphazard fashion.

Merton and Kuhn equate progress in science with scientific change. The manner in which each describes scientific progress is very different, however, stemming from the fundamental nature of their arguments. Kuhn describes two types of change - those minor changes that occur during the articulation process of normal science, and major changes that occur during a scientific revolution. Merton, on the other hand, describes progress in science as resulting from the cumulative character of scientific change.

Merton and Kuhn retain certain similarities concerning the notion of discovery. Each acknowledges that discoveries are produced by the social interaction of scientists using common methods of inquiry. Both stress the necessity of placing discoveries in a social and historical context. A fundamental difference concerning discovery stems from the basic nature of their arguments. Merton understands the development of science to result from the accumulation of discoveries. Kuhn does not accept this argument, claiming that discoveries cannot be isolated with any degree of exactitude, but in effect represent changes in the collective means of interpretation supplied by a paradigm.

Both Merton and Kuhn seek to establish a basis for demarcation criteria within the sociological nature of science. Scientists are portrayed as the only relevant audience for determining the scientific worth of statements of purported scientific value. Processes of validation stem from their commitment to a normative framework, which provides criteria for determining what is or is not to be accorded scientific status. For Merton and Kuhn, these criteria are defined by the normative structure of science.

Tradition plays a large part in determining the advancement of

science. Scientists' commitment to what presently exists often makes it difficult to achieve consensus on new ideas. Kuhn in particular demonstrates the major role which tradition plays in the conduct of research. He shows how tradition can facilitate the development of science by creating conditions for a scientific revolution.

Chapter Four specifies the move towards a sociology of science - essentially a synthesis of the Mertonian and Kuhnian approaches with one added dimension: consideration of the external influences on the development of science. Constructed in this manner, sociological analysis would involve the following conceptions of science: 1) science as social activity; 2) science as knowledge; 3) science as rules; and 4) the relationship between science and other societal institutions.

The basic starting point for the sociology of science is that science is a social activity. This is an intrinsic aspect of sociological analysis. The unit in the analysis of this activity is scientific groups. Study of the structure of these groups reveals a great deal about how scientific knowledge is produced, a factor which has been the traditional focus of sociologists of science such as Merton.

Kuhn offers a basis for points two and three. His work, when treated as a sociology of science, drastically alters the traditional mode of inquiry in this area. Kuhn accomplishes this by emphasizing the importance of attaching a problematic status to the characteristics of knowledge and the significance of these in bringing about change in science. Science can be defined as a body of knowledge, scientific activity as the agent of production. The creation of new knowledge is the goal of science, a process which is in part dependent upon existing knowledge.

The basic assumption of this approach is that the characteristics

of scientific knowledge to some extent determine the development of science. This raises the question of determining the relation between knowledge and the social factors of its production. By relating the intellectual and social dimensions, a more complete study of science may be obtained - for science cannot simply be defined in terms of its features as social activity.

Science is also comprised of rules, that is, dictates for procedure in the conduct of research. Rules are connected with the internal organization of science, providing guidelines and defining the boundaries of scientific research. Science as rules is related to science as social activity, for a consensus must exist on the nature and content of rules. In this sense, the rules of science are not abstract conceptions, but are constructed according to the needs of a given scientific community. Science as rules is also related to science as knowledge, for ostensibly rules provide criteria for the creation of knowledge.

These first three aspects of the sociology of science can largely be derived from the combined works of Merton and Kuhn. Their efforts, however, lack one very important ingredient, and that is consideration of the extra-scientific influences on the development of science. If one examines the historical development of science, it becomes evident that a very complex series of relationships has arisen with, in particular, the economic and political institutions. The extent of these relationships is a major factor in determining the nature of research activity in science. Through study of the various institutional settings within which science takes place, it is possible to understand the nature of research strategy in relation to scientists and their work. From a sociological standpoint, this facilitates study of what influences scientists to practice the types of science they do, providing a clearer picture of the determinants of problem-choice.

The sociological analysis of science must include all four of these conceptions in order to arrive at a clearer understanding of all facets of science. It has been common practice for sociologists of science to focus only on science as social activity. Ignoring the other conceptions will not permit an understanding of the role they play in the development of science. Kuhn devises a means of linking knowledge and rules to science as social activity. By adding the factor of the relationship between science and society, and combining with existing work concerning science as social activity, a more complete sociology of science may be achieved.

Extending the boundaries to the sociology of science even more than the respective Mertonian and Kuhnian approaches will permit, should enable us to ask more questions. Merton and Kuhn provide us with a useful picture of the internal dimensions of science - adding consideration of the external dimensions not only provides an analysis of science as an institution within the wider society, but also serves to illustrate the influence of these external factors upon the internal organization of science. In the process, the notion of science as an autonomous, objective activity is placed in a somewhat different context.

It must be noted that the emphasis here upon Merton and Kuhn purposely excludes consideration of other theoretical perspectives in this area of study. This is not to suggest that approaches such as Marxist analysis and Systems theory, for example, are not useful in examining the social institution of science. Rather, my intention is to limit the parameters of the thesis in the particular fashion I describe, and this precludes the possibility of incorporating all the various approaches. To do so would make the entire effort far too cumbersome. Therefore, the discussion will be restricted to Merton and Kuhn.

Chapter One

Robert Merton's Sociology of Science

The development of the sociology of science is in large part the result of the work of Robert Merton. His analysis of science[1] evolved over a period of thirty-five years. Merton focuses specifically upon the social processes of scientific activity, on the organization and behaviour of scientists. He employs a functionalist theoretical approach to examine the social structure of science. The construction of Merton's sociology of science developed in two basic phases. The first phase involves the identification of the normative structure of science, which Merton attempts by describing a system of norms and values known as the 'ethos of science'. Through adherence to an ethos scientists are able to realize the extension of certified knowledge, which Merton recognizes as the goal of science. At this point in the development of his exemplar, Merton fails to account for the motivating force behind scientists' commitment to an ethos - that is, what it is that encourages scientists to abide by such a system of norms and values. Many critics thus seize the opportunity to point out that widespread deviation to the norms in fact takes place, leading many to question their existence at all. Merton accomplishes the difficult task of defining the source of scientists' motivation for adherence to an ethos, and this comprises the second and final phase of his sociology of

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1. Here and throughout this thesis, I am referring solely to the natural and physical sciences, as opposed to the social sciences.

science. Through description of a reward system in science, Merton links an institutionally derived need of scientists with scientific achievement. Scientists are rewarded in various ways for contributing to the advancement of scientific knowledge. Achievement of that goal is best realized through commitment to the ethos of science. Hence the normative structure and reward system complement each other, and together provide the basis for Merton's study of science as a social institution. We will here briefly outline each of the phases which comprise Merton's sociology of science.

First Phase

Merton (1968:628-660) locates the source of the institutionalization of modern science in Puritanism[2]. This involves identifying the social conditions under which the study of nature became an end in itself, in contrast to earlier times when this type of study served religious ends. Merton's (1970) work suggests that these conditions are associated with the rise of modern capitalism, wherein, as Weber (1958) demonstrates, the development of Protestantism plays a major role[3]. Merton notes that Puritanism provided a legitimizing role for science, but that it was not a necessary condition for the growth of science. Rather, Puritanism simply provided major support for science in that historical time and place. Merton (1973b) then proceeds to demonstrate, through his conception of an ethos, how science developed as a distinct institution, providing a sketch of the structure of science in the process. A more detailed understanding of the components which make up this structure is then offered, which we shall here outline.

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2. It must be noted that the question concerning the source of the institutionalization of modern science stems from Max Weber's efforts in the study of society.

3. The basis of Weber's thesis is largely drawn from the writings of the English Puritans during the latter part of the seventeenth century.

The Normative Structure of Science:

Merton's (1968:604-615) functional analysis of science is codified by describing a set of norms and values known as the 'ethos of science'. These norms are prescriptions of behaviour linked to institutional values. They comprise the normative structure of science, commitment on the part of scientists to them performs an integrating function for scientific activity, permitting the extension of knowledge. The ethos is inculcated into the value structure of the individual scientist through a process of transmission by example and reinforcement by sanction. Once internalized, these sanctions form the scientist's 'scientific conscience'. The ethos is a derivation of the goals and methods of science, comprised not only of technical, but also moral norms. The entire complex of technical and moral norms serves to fulfill the objective of science. Examples are: 1) the necessity of developing accurate and reliable empirical evidence, and 2) being logically consistent in one's work in order to enhance the validity of predictions. The ethos possesses a methodological rationale which is binding upon the scientist not only because it instigates procedural efficiency, but because it is considered, in Merton's words, to be 'right and good'. Hence his claim that these are moral as well as technical norms.

Merton's paper 'Science and Democratic Social Structure', written in 1942 and published several times since, defines the ethos of science to consist of four norms. These are: universalism, communism, disinterestedness, and organized skepticism.

Universalism:

The norm of 'universalism' compels scientists to maintain a strict degree of objectivity concerning the evaluation and subsequent acceptance or rejection of statements of purported scientific value. Regardless of

source, claims to the extension of knowledge are to be evaluated on the basis of previously established criteria, which must be consistently applied. Here Merton is suggesting that the validity of a scientific statement must be judged independently of the characteristics (that is, race, religion, personality, and so on) of its author. Adherence to this principle facilitates the possibility of obtaining objectivity. Thus, objectivity is considered to be inherent in the methodology of science - operating to prevent particularism. The conditions surrounding the contention that certified scientific formulations are the result of an objective process tends to weigh against efforts at the imposition of particularistic criteria of evaluation.

Merton recognizes that the norm of universalism operates fine in theory, but is not widely advocated in practice. Regardless of how inadequately universalism may be practiced, he says, it is included as a principle of operation for democratic societies. Merton (1968:609) defines democracy as the societal condition which seeks to progressively eliminate restraints upon exercising and developing activities considered to be socially valuable. Democratic societies are characterized by impersonal criteria of achievement, as expressed in the norm of universalism. Merton admits that democracy practiced in laissez-faire form does lead to a differential accumulation of advantage for various segments of the population. This forces the introduction of ever-improving forms of organization (for example, political) which act to preserve some semblance of equality of opportunity. These forms of organization may vary, but universalistic standards remain (in principle). Merton's point is this: universalistic criteria for science are much more easily fostered in a democratic society than in a non-democratic one.

Though this norm demands that the worth of scientific statements

be determined by application of the technical norms of science, independent of particularistic characteristics, it is apparent that considerations outside the realm of technical norms to some extent determine the direction of science. For example, scientists may only read journal articles written by individuals accorded high status in their field, or avoid the work of those designated as incompetent. Merton rightly notes that scientists employ indicators of truth like all social groups. In part, these indicators are based upon the reputation of the scientist(s) in question, and also the strength of those supporting a particular claim. These forms of truth indicators, though retaining a certain amount of unreliability, may at times be utilized instead of alternatives such as race and nationality.

Communism:

The norm of 'communism[4]' obligates scientists to establish and maintain free channels of communication with other scientists regarding the findings of their research activity. Communism thus refers to a common sharing of findings. The findings of science are the result of social collaboration and therefore should be available for the use of all scientists. The individual who makes a discovery does not retain sole rights to it. Rather, the discovery becomes the common property of science - the scientist's claim to a particular discovery is limited to recognition for the feat - something which is roughly 'commensurate' with the significance of the discovery in terms of the established fund of knowledge. The pressure for dissemination of information is reinforced by the goal of extending knowledge and the incentive for recognition within science. Those who fail to commun-

4. During the conservative times of Merton's original writings, the word 'communism' retained certain inappropriate connotations. In an effort to overcome this problem, Bernard Barber changed the word to 'communality'. We will, however, recognize Merton's original usage.

icate their findings - Merton gives the example of Henry Cavendish - become the target of sanctions which serve notice that an institutionalized norm is being violated.

Science is further characterized by a communal nature because scientists recognize that their work depends upon a cultural heritage developed over hundreds of years. Merton (1968:612) notes that "Newton's remark - "If I have seen farther it is by standing on the shoulders of giants" - expresses at once a sense of indebtedness to the common heritage and a recognition of the essentially cooperative and cumulative quality of scientific achievement."

The recognition that individual scientists play a relatively small role in the advancement of science places emphasis upon the cumulative nature of scientific activity. That is, scientific advancement is realized as scientists build upon the work of their predecessors, a process which continues through generation after generation.

Disinterestedness:

Merton clarifies the motivational basis of research activity through the norm of 'disinterestedness', which obligates the scientist to pursue scientific activity on the basis of an internalized desire to further the goal of science, rather than being motivated through personal benefit. It is improper for a scientist to seek personal recognition on the basis of his work. Rather, the scientist should only approach his work on the basis of the institutional norms sanctioned by science. He should not be primarily concerned with the fame or wealth that may accrue as a result of his findings.

Disinterestedness, Merton claims, is not to be equated with altruistic behaviour. The individual scientist might be motivated by various concerns such as altruism, desire for knowledge, curiosity, and so on. However, focus upon these will confuse motivational and institutional levels of

analysis. Rather than seeking specific motives, one should instead look for the institutional controls which govern the various motives[5].

Merton notes that incidents of fraud are rare in science, something that stands out significantly when compared to other types of activity and which is often attributed to the claim that scientists generally possess high personal qualities. However, there is no real evidence to indicate that scientists possess superior qualities of integrity, so Merton suggests that the explanation must lie within the character of science itself. As science involves verification of results, one's research is constantly under the scrutinization of fellow scientists. Scientific activity is therefore subject to a strict form of policing in a manner perhaps unequaled in any other sphere of activity. The attitude of disinterestedness is based in this characteristic of science - and thus contributes to the creation of an image of integrity for the practitioners of science. In the competitive world of science there are incentives for getting ahead of rivals by illicit means. Merton claims that such impulses can find little opportunity for fulfillment in the conduct of scientific activity because the practice of disinterestedness effectively makes scientists accountable to their peers, thus contributing to the institutional stability of science.

Organized Skepticism:

This norm is both a methodological practice and an institutional requirement. It demands that evaluation be [illegible]ded until all the facts are presented and that these facts be impersonally scrutinized in terms of pre-established criteria. It would be foolish for scientists to judge work

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5. Wunderlich (1974) makes the important point that sociologists of science often define disinterestedness as referring to the subjective or individual motive, when in fact Merton is referring to an institutional motive.

without possessing all the facts, for then scientific knowledge would be based upon very dubious grounds indeed. In essence, organized skepticism provides science with criteria for distinguishing between science and pseudo-science. The scientist is compelled to always be skeptical when presented with new claims to scientific knowledge. This attitude of skepticism is organized in the sense that all scientists must express it - hence it becomes an institutionalized form of behaviour.

Merton's outline of the normative structure of science suggests that the combination of these norms serves to accomplish the task of regulating science in such a manner as to enhance the development of scientific knowledge. In this sense, the ethos integrates scientific activity in accordance with the institutionalized goal of science. Any conflict which occurs is viewed as deviant because it threatens to disturb the normative order. Many writers note that deviance to the Mertonian normative structure is widespread, a factor which can be readily demonstrated by comparing the norms he describes with actual cases of scientific behaviour (Barnes and Dolby, 1970;Mulkay, 1972;Rothman, 1972). Merton, however, never intended that scientists should so invariably adhere to these norms as his critics would believe. In large part, these critics misconception stems from Merton's failure to describe the motivational basis for commitment to a normative ethos. He finally arrives at the missing link through an analysis of the reward system in science. Consideration of this aspect permits an understanding of both conformity and deviance to the ethos of science.

Second Phase

The Reward System of Science:

The social organization of science is based upon an institutionalized reward system that serves to facilitate the extension of scientific knowledge.

Scientists are encouraged to develop new knowledge in return for recognition - the latter acts as a reward, the allocation of which is imbued within the social processes of scientific activity. Scientists can best achieve the development of new knowledge through commitment to the 'ethos of science' - hence, they are motivated to do so by the institutional promise of reward through recognition.

In order for recognition to be given where it is due, scientists must establish their priority in a particular discovery (Merton,1957). Scientists thus come to value originality in research, which serves to foster competition within science. This competitive situation produces many disputes over priority, and these have characterized the history of science. Galileo, Newton, Cavendish, Faraday and most of the other great scientists were at some point in their careers embroiled in such disputes. Merton contends that the explanation for these disputes is to be found in the sociological factors associated with 'intellectual property', that is, in the institutional norms of science - which act to exert pressure upon scientists to communicate their findings. Every aspect of science reminds the scientist of the duty of his role: to advance knowledge. Thus originality is placed at a premium - for originality makes the advancement of knowledge possible. Scientists are motivated by an obligation to this institutional mandate. Recognition for originality is a motive which derives from the emphasis on institutional norms - becoming a testimony that the scientist is successfully fulfilling his role.

The rewards offered in science take various forms. At the head of the list is eponymy, that is, the act of attaching a scientists's name to a discovery - for example, Newton's Law, Halley's comet, Planck's constant, and so on. In this manner, the contributions of certain scientists are

indelibly etched in the annals of the history of science. Another type of reward comes in the form of a symbolic prize. One example is the Nobel Prize, which most consider to be the highest token of recognition in science. There are also various medals honouring both famous scientists and those who receive the award. As well, there are honourary memberships in scientific societies and academies. On some countries, as in Britain, scientists are often accorded titles in recognition of their work. A final example of recognition is that supplied by the historians of science. Historians pay great attention to priority in discovery - in part, it is often their task to isolate the scientist responsible for a particular discovery, and thus resolve a dispute which may have lasted many years.

A scientist's right to his 'intellectual property' contrasts sharply with other types of owners rights. Once a scientist makes an original contribution to the advancement of knowledge, he no longer retains exclusive access to it. The contribution becomes the common property of science, to which all its practitioners acquire access. Scientists do not have the right to withhold their findings contingent upon the allocation of appropriate recognition. Basically, the only property right in science is the recognition due for one's achievement.

A scientist should only realize credit for his role in a particular discovery, and ensure that others upon whom the work is built also receive recognition. Merton notes that if science placed supreme value only upon originality, then scientists would be inclined to attach more importance to recognizing priority than presently takes place. It is therefore essential that scientists express the value of 'humility', to ensure that they not become overly concerned with originality and priority. Humility receives expression in the form of an acknowledgement of indebtedness to the findings

developed by predecessors. In another form, humility finds expression in the modesty of scientists, who must be aware of their own limitations as well as the limitations of science.

Merton recognizes that the value of originality, which causes scientists to seek recognition for priority in discovery, and the value of humility, which causes them to express their debt to others as well as the limitations of their own work, are 'potentially incompatible'. He insists, however, that this does not result in a contradictory situation because these potential incompatibilities can be blended into a single orientation - the conflict between the two values results in feelings of ambivalence towards the establishment of priorities.

The components of this ambivalence are quite clear, according to Merton (1973c). He uses the historical example of Darwin and the theory of evolution to illustrate the point (1957). Darwin did not immediately publish his theory of evolution at the time of its discovery. In letters to his friend Lyell, Darwin expressed hesitation in publishing solely for priority, yet also indicated that he would be considerably upset if someone else published his 'doctrines' before he had the opportunity to do so. Merton contends this to denote an expression of 'uncontrollable ambivalence'. As it turns out, Darwin was pre-empted by an individual named Wallace. In another letter to Lyell, Darwin again registers his ambivalence, stating that his loss of priority does not alter the 'justice of the case'. He refuses to publish merely in an effort to establish priority. Torn between a concern for priority and the feeling that Wallace's work was totally independent of his own, and therefore deserving proper acknowledgement, Darwin is stalled in a situation of ambivalence. Other scientists take the matter in hand for Darwin, presenting both his works and those of Wallace.

This historical example is probably an exception, as Merton recognizes. Scientists, in their quest for originality, are usually not as concerned with modesty or humility as they are with establishing priority. Originality is readily observable in science and receives recognition in the form of concrete rewards, while humility receives recognition only in the form of respect from other scientists. As well, after a scientist publishes a finding, it is extremely difficult to show clearly that one independently came to the same result. It is thus an unequal contest between the values of originality and humility.

Means of Evaluation and Communication:

As priority became important in science, so did means of evaluation and communication (Merton,1973d). These means were institutionalized in response to the emerging social organization of science during the seventeenth-century. At that time, scientific academies and societies (such as the Royal Society in Britain) developed the scientific journal, which became a mechanism both for recording and communicating the findings of scientists. A concomitant factor was the emergence of the referee system, where scientists acted as judges to determine the acceptability of manuscripts submitted for publication. Both the referee system and journal were designed to achieve improvement and diffusion of scientific knowledge. They also operated to motivate scientists to adhere to the norm of communism by 1) prompt publication, 2) providing a means of establishing priority by recording date communications were received, and 3) permanently securing scientists' work in the archives of science. As well, scientists were provided with a means of determining the legitimacy of their work as scientific, enabling them to build on the work of others with some degree of confidence.

Changing Social Organization of Science:

Since the seventeenth-century, however, the social organization of science has changed considerably. The race for priority is presently so intense that it is common for entire research teams to work on a project, rather than the solitary scientist of past times. This often involves great financial cost, introducing new factors which serve to strain the norms of science somewhat. For example, scientists would be hesitant to communicate the progress of their work when losing out to competitors might result in the loss of financial resources as well as prestige. These changes in the social structure of science have counteracting effects on competition (Merton,1973e:330-333). Firstly, differences in the social structure of scientific specialties affects the intensity of competition within them. For example, some specialties may have a large number of scientists working on a small range of problems. Thus, the competition is likely to be very intense. Secondly, the degree of competition differs among the various strata of scientists in each specialty. Those at the top levels have readier access to needed resources, including information about competitors. Those in the middle and lower ranks work at a more impersonal level of competition, lacking easy access to information about competitors. At these levels competition is more diffuse. Thirdly, the reward of recognition ensures that scientists will not work in anonymity. Fourthly, the introduction of team research complicates the evaluation of individual work. Finally, becoming known in science is increasingly difficult due to the exponential growth rate in the amount of scientists and publications (Price,1963).

Merton further examines the changing characteristics of the structure of science as they affect priority in his paper 'Singletons and Multiples in

Science'. Here he studies the development of science through single and multiple discoveries (the latter refers to the 'independent appearance of the same scientific discovery', and will hereafter be referred to as 'multiples') (1973f:352). Merton pieces together a sociological theory of discovery, based upon the following tenets[6].

Basically, Merton considers science to develop by an incremental accumulation of knowledge. Discoveries, in effect, have their basis in existing knowledge. Scientific progression is best realized through social interaction. 'If all scientists worked alone, development would be very slow. By working together as social groups (which serves to create a division of labour in science), and utilizing common methods of inquiry, scientists facilitate the progression of scientific knowledge. Therefore, the socialization process is vital, in order that young scientists may acquire proper knowledge of existing methods of inquiry. This serves to reinforce the notion that common procedures are essential in the conduct of scientific work. Thus, discoveries are generally products of their own time, stemming from the cumulative nature of scientific knowledge, the interaction of scientists, and the employment of systematic methods. Further, the same discovery is often made by more than one scientist. This becomes inevitable as scientists, working in a common social milieu which determines the importance of particular problems, compete for solution and the reward of recognition. In fact, "the pattern of independent multiple discoveries in science is in principle the dominant pattern rather than a subsidiary one. It is the singletons - discoveries made only once in the history of science -

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6. Merton's paper was a commemorative lecture on Francis Bacon.

that are the residual cases requiring special explanation" (Merton,1973f:356).

Having laid the basis for the social determination of scientific discovery, Merton (1973f) proceeds to supply evidence on the 'hypothesis of multiples'. Multiple discovery occurs as a result of the social structure of science, and poses ramifications for the issue of priority and the allocation of rewards.

Merton bases his hypothesis on the following: Scientists often discontinue work on a problem because the results have been anticipated by a competitor. In such cases of being forestalled, the scientist may still report his original work. However, reported instances of forestalled multiples are minute compared to the vast number of unreported cases. In some cases, discoveries become identified as rediscoveries of previously unpublished work. The scientist may lose interest in recording his version of a discovery if he learns it has been accomplished elsewhere. In fact, forestalled multiples are often communicated in the form of oral lectures, rather than in written form. Scientists are at times diverted from research on a particular problem, which is then successfully completed by others. A potential multiple therefore becomes a singleton. The race for priority further creates a situation whereby if one scientist or research team does not make the discovery, another will. Scientists who become involved in potential multiples may be hesitant to come forward and defend their version of the discovery. The case of Darwin mentioned previously stands as a prime example. 'Institutional expedients' exist which ensure a scientist's right to claim for priority. One example is the scientific journal, another is the practice of depositing manuscripts with scientific academies and societies.

Merton combines this social theory of discovery with one that places emphasis upon the role of 'genius' in scientific development. Scientists who fit this category, for example, an Einstein or a Newton, will generally produce many discoveries, based both upon their superior intellectual ability and privileged access to scientific resources. Each of these discoveries is a potential multiple, so individually, the genius is not indispensable to science, since his discoveries will at some point be made by others. The genius will, however, contribute to science what a number of lesser scientists will combined. Furthermore, the genius serves a charismatic function, a source of inspiration to others both in ideas and by example. Thus, Merton achieves the task of considering the role of genius sociologically by accounting for the social determination of discovery and allowing for variability in the intellectual ability of scientists[7].

Merton makes a further point concerning the status of individual scientists that is of some importance. His studies indicate that once scientists achieve a high status, they never go below that level - "Once a Nobel laureate, always a Nobel laureate" (1973g:439). Merton (1973g:446) refers to this as the 'Matthew Effect' in science, which consists of "the

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7. In his commemorative lecture, Merton took from Bacon two basic ideas: 1) the notion of genius in science, and 2) the social determination of scientific discovery. Merton's task was to determine which of the two offered the best explanation of discovery. Bacon claimed to find a disjunction between the two points. Merton argues that this is wrong, based on his understanding that once science becomes institutionalized, patterns of multiple discoveries will predominate. He thus reformulates the notion of genius to offer what he considers the correct explanation of the social determination of discovery.

accruing of greater increments of recognition for particular scientific contributions to scientists of considerable repute and the withholding of such recognition from scientists who have not yet made their mark".

This forces Merton to conclude that one's evaluation of the worth of particular contributions is influenced by the author's status. Therefore, when a multiple discovery is involved, the higher status scientist will inevitably receive more recognition than his fellow discoverers of lower status. In this sense, the Matthew Effect serves to disproportionately allocate scientific resources - in this case, the communication device of journals. In another sense, the Matthew Effect operates as a social mechanism that inhibits the efforts of lower status contributors to science, making it difficult for their work to receive appropriate recognition.

For the final section of his exemplar, Merton (1973h:375) attempts to justify the importance of studying multiples in order to better understand the social processes in the development of science. He notes that the systematic study of multiples and priorities in discovery provides a basis for analyzing social relations between scientists. Scientists are organized into 'communities', which are dispersed collectivities rather than being geographically compact. Thus, the structure of these communities cannot be properly understood by focusing upon them in isolation. The fact that the same discovery occurs independently signifies that scientists respond to common social and intellectual forces which affect them all. Furthermore, it becomes possible to identify the similarities and differences between the various communities, and to question the traditional notion that scientists exist in one unified body. Finally, the study of multiples suggests how these communities extend in both time and space. Scientists

are bound to the past by building upon an existing body of knowledge; they are bound to the present through the social interaction required of their work, which provides a social mechanism for determining common problems; and, they are bound to the future through an obligation to fulfill their social role by advancing knowledge.

Summary

Merton's sociology of science understands the development of scientific knowledge to be dependent upon scientists' commitment to a normative framework - the motivation for such commitment stems from an institutionalized reward system. His major premise is that knowledge is generated by social means. Scientists work in groups, or communities, utilizing a common stockpile of knowledge and common methods of inquiry. These factors create a situation whereby scientists respond to similar intellectual and social forces, thus creating a common perception of what problems must be solved at any given time. Further adding to this is the necessity of communicating research findings. Science will not advance if scientists work in isolation, the promise of recognition serves to ensure that they will respond to the social requirements of their role.

Many writers overemphasize the criticism that Merton's conception of a normative 'ethos' amounts to nothing more than an idealistic interpretation of what goes on in science. Merton time and again admits that the norms and values which he claims to characterize scientific activity may not actually describe what occurs, in fact, he often provides examples to illustrate. Through analysis of priority in discovery and the reward system of science, Merton introduces factors which account for both situations of conformity and deviance to the ethos of science. It is recognized that scientists can

best advance knowledge by adhering to these norms, however, in their quest for originality, they may deviate somewhat in order to accomplish their task - for example, by conducting research under conditions of secrecy. Once a solution to a research problem is realized, however, it must be evaluated in order to establish whether or not a contribution to the advancement of knowledge has been achieved. Therefore, the conditions of secrecy must be relaxed at some point, and the findings submitted to social forms of scrutinization. A further example of how the social structure of science can effect the normative order concerns the notion of stratification. Based upon the institutionalized reward system, scientists are accorded differential status, which in turn determines their access to the resources of science - in effect, determines their ability to exploit the nature of the social structure of science. Those of high status will find it easier to acquire, for example, financial resources for research, and acceptance of their findings as a scientific contribution because of the means of communication at their disposal. Scientists of lower status, on the other hand, will find it more difficult to achieve recognized contributions to scientific advancement, regardless of their commitment to a normative order.

Merton's work showed that science is sociologically significant - he laid the groundwork for analysis in the area. His approach is what we refer to throughout this thesis as the traditional sociology of science, if for no other reason than to establish the original boundaries of the field.

What questions can the sociology of science usefully ask with Merton's exemplar? The range of these illustrates both the strengths and limitations of his position. Questions that are typically posed include, for

example: What are the nature and extent of institutional factors in science? How did these emerge? How have they changed over the years? By what social control mechanisms are they maintained? How are research communities organized? How does scientific development take place? This list is by no means exhaustive, of course, but is indicative of the focus of Merton's work. Certainly he was on the right track by placing emphasis upon institutional developments in science, for this provided a useful approach to understanding the characteristics of its social organization - and hence an answer to the question of how scientists fulfill the goal of extending certified knowledge.

The limitations of Merton's analysis are evident, however, perhaps more now than during the time of his most influential writings. Firstly, his approach cannot be usefully extended beyond the internal dimensions of science, as only a marginal effort is made to place it within a broader social context. Kuhn, whom we shall discuss in the following chapter, does not really make any improvements in this direction. Secondly, Merton examines social factors at the expense of what is being produced, namely scientific knowledge. It is quite proper to be critical of this, as many are, but we must be fair in our assessment of the matter by asking whether or not this is important for Merton, given his sociological premise and his understanding of what this form of analysis should comprise. Perhaps Merton saw scientific knowledge as being of a special type, and therefore removed from the purview of sociological analysis. This argument could be made with some force, given Merton's faith in the objective nature of science.

This second limitation of Merton's work has given rise to a great debate between his proponents and those of Kuhn, who are interested in

the relation between scientific knowledge and its development. In fact, much of the impetus for the sociology of science derived from Kuhn's work is a highly critical attitude towards that represented by Merton. In the effort to establish the superiority of their position, the followers of each have become blinded to the possibility that many similarities exist between the two approaches - that taken in conjunction, they illustrate how the fledgling sociology of science is developing parameters for analysis. The Kuhnian argument becomes, in effect, a response to that of Merton (rather than a reaction), serving to extend the range of study originated by the traditional approach. In order to understand this, it will be necessary to outline the basis of Kuhn's work, which will be the task of the following chapter, and then compare his position with that of Merton, which will be the task of the third chapter.

Chapter Two

Thomas Kuhn's Implicit Sociology of Science

The work of Thomas Kuhn (1970a) is an account of the development of science which challenges the prevailing image of scientific change as an accumulation of knowledge. If Merton's work can be seen as focusing upon science as social activity, Kuhn's can be seen as adding to this dimension by stressing the importance of science as bodies of knowledge and rules of procedure. This chapter will provide a brief overview of Kuhn's image of science, and will then proceed to a more detailed discussion of the concepts which he employs. These concepts provide at the same time the appeal of his perspective as well as the focal point for criticism. They represent an acute problem with Kuhn's work, due to both his lack of clarity in defining them, and the lack of agreement amongst critics over the definitions. Kuhn recognizes this problem, and a large part of his later work is an attempt to rectify the dilemma.

Kuhn claims that the image of science set forth by Merton and others is misleading. This image views the development of science as a 'piecemeal process' by which new facts, laws, and theories are added to an ever increasing stockpile of knowledge. Kuhn rejects the notion that scientific development is continuous and cumulative. Instead of claiming that ideas of the present are merely improvements upon ideas of the past, Kuhn suggests that these ideas should be viewed within their social context and time. Science does not always develop in a continuous fashion, but rather is punctuated with periods of discontinuity. It is the distinction between these two types of development that comprises Kuhn's work and separates it from that of Merton.

Kuhn: an overview

The development of science takes place among groups of scientists, or scientific communities. In this manner, scientific knowledge is socially constructed. But Kuhn is not merely concerned with the social processes involved in this construction, he is also interested in trying to understand what it is that is being produced - why certain forms of knowledge come to be produced from certain social conditions and not others. To accomplish this, he creates an analytical framework to relate knowledge, the techniques used by scientists, and social factors.

Kuhn begins by describing how a scientific problem emerges. When scientists are faced with the explanation of a set of phenomena, they must begin by making an effort to systematize it. Attempts are made to place the phenomena within some theoretical structure, so that explanation may be achieved. During this, what Kuhn refers to as the pre-paradigmatic stage, little or no progress will be made in this regard. However, the efforts at systematization will eventually produce a paradigm, which serves as a model to define the techniques and procedures used in further investigation of the phenomena. Kuhn provides several examples, of which are included Aristotelian cosmology, atomic theory, and heliocentric theory. The emergence of a paradigm signals a period of normal science, whereupon scientists proceed to investigate the phenomena in accordance with the methodological procedures supplied by the paradigm. Like a judicial decision in common law, a paradigm can be articulated and extended to different or more rigorous conditions. In fact, periods of normal science are characterized by this process of articulation and extension. Incremental pieces are added to the fund of knowledge - only during periods of normal science is the development of knowledge cumulative and continuous. The knowledge which results from the

activity of normal science is accepted as correct explanation of the phenomena, and is used to generate new problems. For Kuhn, the development of knowledge is never complete. When a new piece of knowledge is produced, it must be questioned, refined, and elaborated - all in accordance with the guiding paradigm. Should this process encounter serious obstacles, scientists must begin to question the utility of the paradigm. If the crisis cannot be resolved by means of existing criteria, they must seek to develop a new paradigm, one which will permit an appropriate resolution. The only way to achieve this is to overthrow the existing paradigm, although all crisis situations in science will not necessarily lead to such a revolution. However, when a revolution is necessary and does occur, science can once again proceed in reasonable harmony, solving the problems generated by the new paradigm. It is this normal science - scientific revolution dichotomy that characterizes the discontinuous nature of the development of science for Kuhn.

Like Merton, Kuhn's argument developed in two stages. The first laid the basic framework, and the second attempted to deal with problems that had been created by the first stage. During the following discussion of the Kuhnian framework, attention will be given to the criticisms of the original work, and the changes that Kuhn later made in response.

Paradigm:

The concept of paradigm, which is central to Kuhn's image of science, is ambiguously formulated and comprises a major difficulty with his argument. Kuhn (1970a:10) originally defined paradigm as referring to "accepted examples of actual scientific practice - examples which include law, theory, application and instrumentation together - (and) provide models from which spring particular coherent traditions of scientific research." Paradigms

serve to control the content of science, regulating standards of procedure. A paradigm constitutes a shared set of rules, practices, and body of knowledge - and requires commitment on the part of groups of scientists. The commitment and consensus a paradigm produces are prerequisites for normal science, that is, for the maintenance of a particular research tradition.

Kuhn's concept is the recipient of much criticism (Shapere,1964; Masterman,1970)[8], which has forced him to reformulate the definition. Briefly, these criticisms amount to: the lack of a clear distinction between paradigm and scientific community, overstating the unanimity of scientists' allegiance to a paradigm, and inconsistent usage of the concept. Kuhn's (1970b;1970c;1970d) response to his critics appears in several articles, whereupon an attempt at refining the concept may be found. We will here note the problems, and Kuhn's efforts to arrive at some solution.

The problem of distinguishing between paradigm and scientific community stems from the intrinsically circular nature of the former. For a

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8. Masterman's content analysis of the concept reveals that Kuhn employs it in twenty-one ways, which she then reduces to three basic categories: (1) metaphysical paradigms, which includes sets of beliefs, a new way of seeing, a metaphysical speculation; (2) sociological paradigms, which include universally recognized scientific achievements; and (3) artefact or construct paradigms, in which the paradigm supplies tools or instrumentation - it is, in effect, a problem-solving device. Masterman contends that the definition of artefact or construct paradigms is the most suitable to Kuhn's understanding of the development of science. In other words, scientific development is realized because problem-solving devices are created that can be applied to problems posed by the paradigm, producing what Kuhn refers to as normal science. By this definition, a paradigm cannot be equated with a theory or a set of methods. The paradigm precedes the theory, and involves more than a particular methodology. A problem-solving device is very specific, focusing upon one set of problems. Only when it is applied to problems beyond its range do anomalies result, according to Kuhn, leading to the potential development of a new paradigm.

paradigm is shared by members of a scientific community, and conversely, a scientific community is comprised of scientists who share a paradigm. Kuhn's later work attempts to circumvent the difficulties this creates by isolating scientific communities from paradigms. He realizes the problems in precisely defining 'scientific community', noting however, that most scientists do have some understanding about their 'community affiliations' (1970b:176-178). Until a systematic means of identifying a scientific community is developed, he suggests that it will suffice to define them as consisting of the "practitioners of a scientific specialty" (1970b:177).

Kuhn identifies different levels of these communities. At the global level is the community of all scientists; at the next level are individual disciplines - for example, physics, chemistry, biology, and so on; and then the specialties - for example, solid-state physics, radio astronomy, and so on. Kuhn claims that it is possible to identify the boundaries for the latter two levels through membership in professional societies, and journals read by those members. Below these levels, however, it is difficult to empirically isolate scientific communities, and Kuhn leaves that task to others.

These communities are the groups who produce and validate scientific knowledge, whereas paradigms are something shared by members of these groups. Paradigms are not discipline-wide. One cannot make a claim for a paradigm of physics or biology. Therefore, each scientific discipline may contain numerous paradigms. There is really no limit to how small a scientific community may be. Its size is often directly proportional to the degree of difficulty involved in establishing formal and informal communication networks, as well as attaining consensus on the maintenance of the ruling paradigm.

Kuhn is also criticized for exaggerating scientists' unanimity in commitment to a paradigm. Though Kuhn still insists upon some degree of consensus for a scientific community to operate, this condition becomes less rigidly required by the dissolution of the scientific community into different levels. Entire disciplines are no longer bound by single paradigms - for several communities can exist within a discipline, and compete with each other over a paradigm. Kuhn thus allows for competition between and within communities, citing (1970b:180;1970d:255) the theories of matter which his critics offer as a counterexample to his stipulation of consensus governing normal science. The theories of matter were not used exclusively by one scientific community, but by many. Kuhn's original contention that fundamental controversy ceases during normal periods of research is no longer a necessary requisite for a scientific community. Now, members of the same community must only agree upon common 'research tools', although they may disagree on the use to which they are put.

Kuhn agrees with the criticism concerning the inconsistent usage of 'paradigm'. Much of this inconsistency is attributed to stylistic problems, which, once eliminated, would leave two different usages of the concept. One, "it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community", and two, "it denotes one sort of element in that constellation, the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science" (Kuhn 1970b:175). The first sense Kuhn (1970b:182) labels 'disciplinary matrix' - "disciplinary because it refers to the common possession of the practitioners of a particular discipline", while matrix refers to the composition of "ordered elements of various sorts, each requiring further specification."

Disciplinary matrix involves shared elements such as symbolic generalizations ($f = ma$, $I = V/R$), beliefs in particular models (atoms, fields of force), and values (accuracy of prediction, evaluation of theories). The second sense Kuhn labels 'exemplars', which is also the final component of disciplinary matrix. Exemplars involve the application of shared generalizations to the concrete puzzle-solutions required in scientific research. During their educational process, science students encounter exemplars through textbook and laboratory work. For example, all physicists begin by acquiring the same exemplars. Kuhn cites problems such as the inclined plane, Keplerian orbits, and the conical pendulum. As their training proceeds, "the symbolic generalizations they share are increasingly illustrated by different exemplars" which Kuhn (1970b:187) maintains, provide the "community-fine structure of science".

Kuhn's later work is a vast improvement on his original conception of 'paradigm'. Many of the confusions surrounding the meaning of the concept are eliminated. Concomitantly, Kuhn's conception of the community structure of science is clarified somewhat. This structure is attached to scientific communities, which involve groups of scientists that share research tools - and are guided by common symbolic generalizations, beliefs, values, and exemplary problem-solutions. These communities exist at different levels, thus allowing Kuhn to distinguish between paradigms and scientific communities, something which he failed to do in his earlier work. For communities can be broken down into: the global group of scientists, entire disciplines, and specialties. It is at the last level to which Kuhn attaches most importance. Whereas Kuhn originally implied that a very high degree of unanimity in commitment to a paradigm was essential, he now relaxes the restriction somewhat by allowing for fundamental controversy during normal periods of

science, competition between and within paradigms, and stipulating that members of a community must only agree upon common research tools. The concept of paradigm is now not so rigid. Kuhn's first edition suggested that paradigms dictated scientific activity, he now suggests that paradigms merely guide scientific activity.

Normal Science:

For Kuhn, normal science is that activity which takes place at all times except during the occurrence of a scientific revolution. Normal science is research within a framework, its boundaries being defined by a paradigm. It involves the articulation and extension of problems posed by the paradigm. These problems are of two types: fact gathering and theoretical activity (Kuhn, 1970a:25). The former involves the experiments and observations commonly listed in scientific journals; the latter the theoretical problems encountered in normal science.

The activity of normal science centers around puzzle-solving - the analogy being that a puzzle, made up of scattered bits and pieces, possesses a framework within which those pieces may be fitted - science operates within a framework in the form of a paradigm into which research activity is fitted. Both the puzzle and the paradigm presuppose a solution. Kuhn suggests that the solution is facilitated by certain rules, which place limitations on the nature of acceptable solutions, and define the steps by which a solution is to be obtained.

These rules fall into four main categories (Kuhn,1970a:40-42). Firstly, statements about scientific laws and theories. These statements help to set problems and to limit acceptable solutions. Secondly, at a somewhat lower level than laws and theories, are commitments to certain types of instrumentation and techniques. Thirdly, the influence of particular scien-

tific writers. In a metaphysical sense, such writings inform scientists on what sorts of things exist in the universe. In a methodological sense, such writings inform them on the requirements of laws and fundamental explanations. More importantly, these influential works instruct scientists on what research problems they should attempt. Fourthly, is a commitment to science itself, to solve the problems posed by a paradigm.

Kuhn is careful to point out that rules alone are not sufficient to describe the common features of a scientific community. Normal science is not entirely determined by a set of rules. This is why Kuhn refers to 'shared paradigms' as opposed to 'shared rules' providing coherence for periods of normal science. Rules derive from paradigms, and should rules be absent, paradigms can still guide research.

Kuhn provides several reasons for suggesting that paradigms take priority over rules. The first involves the difficulty of discerning the rules which guide normal science, that is, the problem of discovering what all periods of normal science have in common. The second, a corollary of the first, concerns the nature of a scientist's education. Laws and theories are never learned in the abstract, instead, they are encountered in a manner that relates them to their specific applications. The ability to abstract laws and theories becomes evident in the demonstration of successful research, an ability which is not necessarily dependent upon an understanding of a set of rules. The third reason is the converse of the second - research requires the use of models (what Kuhn later refers to as exemplars) as well as abstracted rules. Normal science can only proceed without rules if the scientific community accepts existing solutions to problems. Only during periods when the paradigm is losing its usefulness and agreement over problem-solutions is diminishing do rules become a concern. The pre-paradigm period (or the transition from one paradigm to another) is marked by controversy over

problems and solutions. A fourth reason involves the diversity of scientific fields. Kuhn contends that the substitution of paradigms for rules makes this diversity more understandable. A single paradigm may be exclusive to a very small group of scientists. However, it is unlikely that a set of rules would be applicable only to such a small group, rather, commonly understood rules are usually attached to a very broad scientific group.

Kuhn extends this discussion of rules in his Postscript. He maintains his position on the priority of paradigms over rules. Using a refined sense of paradigm, he suggests that exemplars are more important to solving problems than are rules. Borrowing a phrase from Polanyi, Kuhn claims that the 'tacit knowledge' which scientists acquire is the result of doing science rather than learning rules to do it. One of the fundamental ways in which scientists learn is by being shown exemplars, which allows them to come to an understanding of the similarities and differences between the various exemplars employed by a scientific community. Knowledge is embedded in these shared exemplars, which as a form of knowing is as systematic and analyzable as knowledge embedded in rules. It is only after scientists have learned the relation of a particular exemplar to others that they engage in interpretation, a process that requires the application of rules. Rules come into play only after the tacit process of knowing is completed.

Many critics argue against Kuhn's postulation of a normal science - revolutionary science dichotomy, questioning the existence of either type (Watkins,1970;Toulmin,1970;Williams,1970;Popper,1970). Kuhn (1970d:247) suggests that these criticisms stem from an "apparent inability to see in historical examples the detailed functions of the breakdown of normal science in setting the stage for revolution." Here the interpretation of historical evidence is important - Kuhn's critics simply do not reach the same conclusion about the historical data he supplies. They attempt to isolate normal

scientific units in history, something which is impossible, according to Kuhn. Normal science is merely one of two types of change, revolutions are the other - the former is in this sense a cumulative process of articulation.

Science nearly always involves some novel element of interpretation, leaving the continual possibility of incomprehension - for scientists who took part in the development of earlier ideas may find new innovations difficult to accept. During periods of normal science, communication breakdowns can occur which are similar in nature to revolutionary change. This implies that normal science is in some sense revolutionary, a premise which Toulmin (1967: 85) claims will collapse Kuhn's distinction. In response, Kuhn contends that he never suggested the possibility of complete mutual incomprehension, but only 'partial communication'. To deny the possibility of more than one type of scientific change is to mistake the nature of scientific progress, he states (1970d:250).

Having first claimed that the major focus of scientists' commitment is to paradigms, Kuhn's later work emphasizes a more basic commitment to certain paramount values, which underwrites their adoption of particular paradigms, helping them through periods of crisis when the usefulness of the paradigm is being questioned. These values (for example, group unanimity, simplicity, precision, and congruence with existing theories) contribute much to the form of normal science (Kuhn,1970c:21). Kuhn later describes these values as one element of the disciplinary matrix. He thus relates the notion of normal science to scientific community - for the values which guide the former provide a sense of structure for the latter.

Some critics (Lakatos,1970b, for example) accuse Kuhn of subordinating scientific progress to 'mob psychology' - scientists are groups whose collective psychology generates a respect for order - which makes it difficult to conceive of them as 'revolutionaries'. As King (1971:30) argues, if scien-

tists are not committed to any specific form of normal science, but rather to normality per se, "then the problem of explaining changes in the mode of practice and the social structure of science virtually disappears; for what is made to strike the eye is not the changes but the continuity in paramount values".

Kuhn (1970d:261-263) vehemently denies the charge that decisions in science are based upon some form of mob psychology. He acknowledges that shared values are used in making choices between competing theories or paradigms. However, scientists who share the same values may nevertheless make different choices in the same situation. Two factors are at issue. Firstly, different values can result in different choices. Where values conflict (for example, "one theory is simpler but the other is more accurate"), the amount of importance attached to different values by particular scientists plays a major role in individual choice. Secondly, shared values are not always applied in the same manner. Variability of judgment does exist, without necessarily violating any accepted value. These factors can be essential to the advancement of science, states Kuhn, something which would be inhibited were scientific progress characterized by mob psychology. If such were the case science would cease, for mobs tend to renounce normally shared values rather than reinforce them.

Kuhn's response to his critics serves to clarify the distinction between normal science and revolutionary science. The former is merely conceived as one form of change, as opposed to an isolable unit which occurs from time to time in the history of science - precisely the reason why critics have difficulty locating periods of normal science. Kuhn's present conception of normal science also reinforces his understanding of the community structure of science - the primary commitment of scientists is now to 'paramount values' instead of whole paradigms. Shared values provide struc-

ture, commitment to them reinforces a sense of community. Kuhn manages to escape the claim that decisions in science emanate from some form of mob psychology by allowing for the conflict of values, whereupon decisions will be based upon the importance attached to each value. Furthermore, values will not always be applied in the same way. Individual choices (for example, between competing theories) therefore do not stem from the whims of a mob-like scientific group.

Anomaly and Crisis:

Essentially, normal science aims to extend the precision of existing scientific knowledge. During this process, various obstacles may emerge which prevent the solution of a particular puzzle. Kuhn refers to such an obstacle as an 'anomaly'. That is, science comes to recognize "that nature has somehow violated the paradigm-induced expectations that govern normal science". (Kuhn,1970a:52-53) Research activity then proceeds in an attempt to solve the anomaly - "and it closes only when the paradigm . . . (is) adjusted so that the anomalous become(s) the expected' (Kuhn,1970a:52). When the awareness of an anomaly persists over a long period of time and results in a profound effect upon the capability of normal science activity, then one can, according to Kuhn, describe the situation as a state of 'crisis'. He cites historical evidence to support this contention.[9]

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9. "The state of Ptolemaic astronomy was a scandal before Copernicus' announcement. Galileo's contributions to the study of motion depended closely upon difficulties discovered in Aristotle's theory by scholastic critics. Newton's new theory of light and colour originated in the discovery that none of the existing pre-paradigm theories would account for the length of the spectrum, and the new wave theory that replaced Newton's was announced in the midst of growing concern about the anomalies in the relation of diffraction and polarization effects to Newton's theory. Thermodynamics was born from the collision of two existing nineteenth-century physical theories, and quantum mechanics from a variety of difficulties surrounding black-body radiation, specific heats, and the photoelectric effect" (Kuhn,1970a:67).

Kuhn (1970a:84) describes two effects which are universal regarding a state of crisis: "all crises begin with the blurring of a paradigm, and the consequent loosening of the rules of normal research". Similarly, all crises end in one of three ways: one, normal science is able to resolve the crisis-provoking problem; two, the problem is labelled as unsolvable and is set aside for the time when research procedures are more adequately developed; or three, the crisis results in the emergence of a new paradigm, and a battle ensues over its acceptance or rejection.

The charge is often made that Kuhn exaggerates the extent to which crisis so invariably precedes revolution. He (1970b:181) replies that crises do not invariably lead to a revolution, rather, "they need only be the usual prelude, supplying . . . a self-correcting mechanism which ensures that the rigidity of normal science will not forever go unchallenged". He admits that a lack of discussion of the community structure of science in his work obscures the relation of crisis to revolution. Kuhn's change in position concerning the invariability of crisis preceding revolution should satisfy some critics, however, he still fails to adequately discuss the relation between the two. If crisis is the usual prelude, there must be others. The nature of these is not described. One must also be hesitant to accept the notion that crisis is a 'self-correcting mechanism' - this sounds vaguely like functional analysis.

Scientific Revolutions:

Kuhn claims that paradigms undergo transformations - that is, the laws, theories, and so on, upon which they are based are improved to the extent that the original notions are rendered unacceptable for further scientific development. Kuhn refers to this transformation as a scientific revolution, and this is an essential element in his analysis of change in science. Science develops when a transition occurs from one paradigm to

another by a process of revolution - previously accepted criteria are overthrown and replaced by new criteria which offer greater potential in the quest for scientific knowledge. In this manner, Kuhn's notion of 'revolutions' in science replaces the traditional view of accumulation.

Kuhn explains that in order for a new paradigm to be accepted by the scientists in a given community, it must seem better than its competitors. It is not necessary that the paradigm be able to solve all the problems with which it can be confronted, for this would clearly be an impossible demand. When a new paradigm achieves prominence, the older schools of thought gradually disappear, as more and more scientists accept the new way of thinking. In essence, the new paradigm redefines the field of research. When it is able to guide the whole groups research in a particular field, it is considered to have achieved maturity.

The new paradigm will usually retain some of the language and methods of the previous paradigm. However, the new paradigm "seldom employ(s) those borrowed elements in quite the traditional way. Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with another" (Kuhn,1970a:149). Scientists will be hesitant to accept a new paradigm, according to Kuhn, unless two important conditions are being met. Firstly, the new paradigm must demonstrate an ability to solve some outstanding problems which the old paradigm could not. Secondly, the new paradigm must be able to preserve most of the problem-solving ability of its predecessor.

Kuhn acknowledges the effect which the emergence of a new paradigm causes on the structure of a scientific community. In part, he views change in the way of thinking of scientists as a conversion process. As more and more members of a scientific community become converted to the new paradigm, social and professional pressure is brought to bear on those who cling to

the old views. Those scientists who for some reason or another cannot accept the new paradigm have two alternatives. They can proceed with research activity in isolation, or attach themselves to another group whose ideas and methods are more closely allied with their own.

Kuhn (1970a:136-137) suggests that most of what he takes to be scientific revolutions are customarily viewed as additions to the stockpile of scientific knowledge, thus reinforcing belief in the cumulative nature of scientific growth. He accounts for this with the claim that revolutions in science are characteristically 'invisible'. This claim stems from Kuhn's (1970a:136) contention that scientists acquire their image of scientific activity from an "authoritative source that systematically disguises . . . the existence and significance of scientific revolutions". The authoritative source to which Kuhn is referring are the science textbooks, as well as popularizations and philosophical works based on them. These all focus on an "already articulated body of problems, data, and theory, most of the particular set of paradigms to which the scientific community is committed at the time they are written" (Kuhn,1970a:136). As well, they portray the outcome of previous revolutions as stable and relate these to the present normal-scientific tradition. Kuhn maintains that textbooks in particular require re-writing whenever a change occurs in science. They are always rewritten after the occurence of a revolution, and as a result, "they inevitably disguise not only the role but the very existence of the revolutions that produced them" (Kuhn,1970a:137). These reconstructions of history are always related to the existing scheme of things - scientists of the past are portrayed as having worked on a fixed set of problems using a fixed set of methodological procedures that fit in with the most recent scientific revolution. Science is thus seen as developing linearly toward its present state. No wonder, Kuhn says, that science comes to be viewed largely as a cumula-

tive enterprise.

The notion of scientific revolution is the focal point of much interest on the part of Kuhn's critics. Many believe that Kuhn is primarily concerned with revolutions of major proportions, a factor which stems from his admitted inability to clarify the nature and size of scientific communities in terms of the historical examples chosen. Kuhn's (1970b:181) later efforts at describing community structure provides a clearer understanding of revolutions - which is for him "a special sort of change involving a certain sort of reconstruction of group commitments". The change need not be considered large, nor does it necessarily need to seem revolutionary to any other than those within a particular community.

While the preceding Kuhnian concepts attracted considerable criticism, other aspects of Kuhn's work also created problems. Two of the more important of these - the incommensurability of competing paradigms and the relativist nature of his work - will now be discussed.

Incommensurability

A crucial factor of Kuhn's thesis is the contention that scientists are compelled to view their world solely on the basis of the paradigm they support. This stance poses ramifications for the times when paradigms are in competition with each other for the allegiance of a particular scientific community. At these times the proponents of competing paradigms fail to fully understand each other's perspective, a process Kuhn describes as 'incommensurability'. He advances this view because proponents of competing paradigms often disagree about the type of problems that a new paradigm must solve due to their differing understanding of what constitutes the problems. Communication is also inhibited by the fact that the competing paradigms present such differing views. Introduction of the problem of incommensurability stresses the difficulty of obtaining rational discourse and crit-

icism between competing paradigms. Kuhn (1970a:150) writes:

> . . . before (competing sides) can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift. Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt-switch, it may occur all at once (though not necessarily in an instant) or not at all.

Kuhn's position on incommensurability raised immediate criticism (Scheffler,1967;Meiland,1974). How can competing paradigms, based in different worlds (or perspectives), address different problems by criteria of different standards? Without some common framework, in what sense could they be considered to be in competition? Comparative criteria cannot be supplied by the paradigms involved. Paradigms must therefore be compared by external factors. These are related to the social and psychological aspects of scientific activity, a point which Kuhn (1970a:158) makes in his mention of the gestalt-switch, which involves the conversion of scientists from one paradigm to another largely on the basis of their faith in the ability of the new paradigm. Therefore, it is possible that different criteria are involved in the comparison and evaluation, thus inhibiting a rational solution to the conflict between two paradigms. If there is no basis for comparison, is there a basis for conflict? This must be resolved by the proponents of the respective paradigms in relation to the particular requirements of their scientific activity, otherwise science would be locked in a continuous stalemate.

Advancement of the incommensurability thesis has led many critics to postulate Kuhn's insistence upon the irrational nature of paradigm choice, or, at another level, of theory choice. Kuhn (1970d:266-277) seeks to refute this claim, suggesting that his readers have misinterpreted his work.[10] The comparison of theories, Kuhn maintains, requires a language

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10. Here Kuhn is speaking of choice between competing theories, rather than whole paradigms.

into which both can be translated. Translation necessarily involves compromises, which can alter meaning. A decision must then be made on the acceptability of certain alterations. In order to accomplish this, the translator needs to know what of the original should be preserved and also something about those who will read the translation. For Kuhn, the act of translation is a recourse for those holding incommensurable theories.

Scientific communities can, in effect, be regarded as language communities. Scientists learn a language by a process of ostension, the "direct matching of whole words or phrases to nature" (Kuhn,1970d:271). Scientists acquire knowledge by ostension through the use of exemplars. Exemplars pose problems which involve learning the language of a theory and hence the knowledge of nature embedded in that language. Through exposure to a number of exemplary problem-solutions, scientists begin to see the similarity of different situations. Once this ability is acquired, it becomes easier to understand the differences between situations as they arise.

> These learned similarity-dissimilarity relationships are ones that we all deploy every day, unproblematically, yet without being able to name the characteristics by which we make the identifications and discriminations. They are prior, that is, to a list of criteria which, joined in a symbolic generalization, would enable us to define our terms. Rather they are parts of a language-conditioned or language-correlated way of seeing the world. Until we have acquired them, we do not see a world at all (Kuhn,1970d:274).

Exemplars serve a cognitive function and thus help to remove the notion of irrationality of theory choice. The scientist, provided with exemplary problem-solutions, and bound by shared values to science, does not require a separate set of criteria to make a decision when conflict arises.

Kuhn suggests that normal scientific research requires an ability to group objects and situations in 'similarity classes'. During a revolution, some of these similarity relations change, whereupon objects can be grouped differently. Kuhn gives the historical examples of the sun, moon,

planets, and earth before and after Copernicus; of free fall, pendular, and planetary motion before and after Galileo. The redistribution of objects can often render previously similar descriptions or generalizations incompatible, resulting in a communication breakdown. Kuhn insists that there is recourse for the restoration of communication, which stems from the basis of what scientists share (history, language, or the ability to communicate, an everyday world, and so on). These shared elements permit a determination of the differences that exist. What this involves is a translation of each other's theory into one's own language in order to describe the phenomena to which that theory or language applies. This is not simply an act of 'arbitrary conversion', but requires a specific decision to adopt a new language and deploy it accordingly. For Kuhn, then, a basis for comparison, a common framework, always exists. The transition from one theory or paradigm to another is thus a rational process.

As scientific communities exist at different levels, there is the possibility that several very different languages can exist within a single discipline. Kuhn's solution to the problem of communication this could create is to impute a cognitive function to exemplary problem-solutions - exposure to several exemplars permits the scientist to acquire knowledge of several languages, which helps to facilitate comparisons. Combined with shared values, exemplars provide a means of decision concerning theory choice. The nature of this choice is thus connoted a sense of rationality. Combined with other means of theory or paradigm choice, such as persuasion, acceptance by faith, and so on, Kuhn can account for both the rationality and irrationality of comparison in science.

The notion of incommensurability remains, for the proponents of competing theories and paradigms will always operate somewhat at cross-

purposes. Kuhn (1970a:147-148) notes that if "there were but one set of standards for their solution, paradigm competition might be settled more or less routinely". The important point to be made concerning incommensurability is that science does not progress by an accumulative process. Rather, incommensurability forces change through a process of revolution.

Relativism

Another major difficulty with Kuhn's original work is the relativism to which it leads (Shapere,1964;Phillips,1973). The charge of relativism is based upon the following interpretation of Kuhn's position: scientific knowledge develops because scientists view the world from a succession of perspectives - each perspective containing a self-justification for the tradition it represents. That is, rules for the conduct of science and standards of evaluation are relative to a paradigm. According to Phillips (1973:15), Kuhn apparently denies the existence of independent standards in making comparisons between paradigms. Factors involved in comparison are paradigm-dependent, rendering rational comparison impossible.

Kuhn (1970b:205-207;1970d:264-266) vigorously denies the charge of relativism. He claims that his view of scientific development is 'fundamentally evolutionary'. If one considers two theories on this evolutionary scale, Kuhn believes it would be possible to distinguish one from the other by means of certain criteria - for example, accuracy of predictions, degree of specialization, number of concrete problem-solutions, and so on. This would allow an evaluation of the two theories, as one theory cannot be as good as another in every respect. Present theories are better than earlier ones for solving problems because of their higher state of advancement on the evolutionary scale. In this sense, Kuhn claims not to be a relativist.

One of the main reasons Kuhn is labelled a relativist involves his

treatment of the concept of truth. His original work designates 'truth' as paradigm-dependent, and therefore relative. Kuhn now states that for purposes of comparing theories, the notion of truth is non-problematic. Scientists in a given community can generally agree on what is or is not valid knowledge. Kuhn suggests that theories are true as long as they are useful at solving puzzles, but can later be abandoned as false - a position which is allied with scientific development as evolutionary, but does nothing to destroy the traditional conception of the accumulative nature of science.

While Kuhn will admit that a theory can be demonstrated to have better problem-solving ability than its predecessors, he refuses to accept the contention that a theory can be presented as a closer approximation to the truth. These types of judgments do not refer to the theories' problem-solutions or concrete predictions, but rather to the match between what the theory is trying to explain and what is really there - in other words, it's ontology. Kuhn (1970b:206) maintains that there is no 'theory-independent' way to construct the match between the "ontology of a theory and its real counterpart in nature". Furthermore, he suggests that nothing is lost because of this, least of all the explanation of scientific progress.

Kuhn's contention that the charge of relativism is simply the result of a misinterpretation of his original work is quite convincing. The fact is he never subscribed to incommensurability to the extent that it implies relativism. Kuhn does allow for theory-independent standards by which a new theory may be deemed to constitute progress over the old. Scientists using different exemplars will see the world differently, but this does not mean that communication is impossible. Shared knowledge from learning various exemplars helps to facilitate communication.

Summary

Kuhn is attempting to transform the traditional image of science by developing a new approach to the interpretation of historical data - one which emphasizes the coherence of knowledge and practice in relation to groups of scientists. To effect this new image, Kuhn constructs a series of concepts by which scientific activity is divided into periods of normal and revolutionary science. It is the resolution of the conflict between these two types of activity that allows scientific knowledge to develop in evolutionary fashion. From this perspective, the development of science stems from the discontinuous nature of scientific activity - existing traditions are overthrown at various points in the history of science - a viewpoint which is in opposition to the traditional conception of the cumulative nature of scientific development.

Certain problems arise with Kuhn's argument (which are inevitable whenever a new way of thinking about a particular area of study is introduced) that he attempts to deal with in later work. Most notably, problems with the concept of paradigm receive clarification, helping to make a distinction between it and scientific community. In the process, we are presented with a clearer understanding of the sociological nature of Kuhn's work. Kuhn further seeks to deny the commonly made charges of relativism and incommensurability, two points which lead many critics to claim that he conceives of scientific change as inherently irrational. Kuhn quite convincingly deals with these critics, although in the process he softens his original position. He now, however, accounts for both the rationality and irrationality of change in science.

Nevertheless, other more general problems remain with Kuhn's analysis. While he does provide some insight into the internal processes of scientific

activity, one would have to argue that he suggests little about those social processes involved in the conformity of scientists to particular paradigms. While Kuhn does speak of the socialization process which scientists undergo during their educational training, he does not adequately account for the changing perspectives that scientists experience during the course of their careers. By the implications of his argument, the beliefs of scientists would have to change just as rapidly as theories do in science. In fact, it is more likely that theories change with more rapidity than do the beliefs of individual scientists.

As another example, Kuhn obscures the distinction between a descriptive history of ideas and a methodological prescription for the conduct of scientific activity. He (1970b:207-210) acknowledges this ambiguity in his Postscript. Kuhn's original text repeatedly commits the error of interchanging the descriptive and normative modes, which violates the basic philosophical theorem that 'Is' cannot imply 'Ought'. In another paper, Kuhn (1970d) suggests that his work must be considered as simultaneously descriptive and normative. He (1970d:237) states: "If I have a theory about how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish."

Kuhn therefore unequivocally contends that he is offering a methodological prescription for science. He claims that the descriptive generalizations set forth are evidence for the validity of his analysis because they can be derived from it. Despite this claim, it would be more plausible to suggest that Kuhn is mainly offering a history of ideas because his study traces the transference of ideas in disciplines and sub-disciplines between periods of normal and revolutionary science. In the process, Kuhn implies the importance of the sociological nature of the factors involved in the

transition.

Kuhn employs a political vocabulary in his description of scientific activity - words such as 'crisis' and 'revolution' suggest that the development of science consists of a series of victories and defeats. This imposes a rigidity on science which renders problematic the issue of innovation in research - scientists must follow the guidelines of a paradigm or risk being excluded from the mainstream of activity in their field. The implication is stronger than Kuhn intended, although it does illustrate his argument as emphasizing the conservative nature of scientific development.

Kuhn's major contribution to a sociology of science is that he extends the analysis of the organization and behaviour of scientists to include the role of knowledge in scientific development. In various respects, Kuhn's analysis of the structure of the social organization of science is quite similar to Merton's, as we shall see in the following chapter. Both examine the social organization of science in the terms of scientific communities structured by a normative framework comprised of commonly - shared values. The parameters of Merton's approach essentially focus on these aspects, which for at least two decades defined the boundaries of the sociology of science. Kuhn's work is an effort to expand these boundaries by relating bodies of knowledge to the social organization of science. Further, he gives explicit illustrations of how rules are part of this relation, which we shall discuss in the final chapter. Whereas Merton described the development of scientific knowledge to be an inherently objective process, (and this in part explains why he did not consider study of the relations between knowledge and the social organization of science to be important), Kuhn shifts the emphasis to subjective factors. This sets out a whole new series of problems for the sociology of science. As with Merton's work, we must

ask what questions can be answered by adopting Kuhn's view? Certainly all the questions mentioned in the previous chapter concerning Merton's perspective would still apply to that of Kuhn, although some of the answers would be quite different. For example, the explanation of scientific change posited by Merton as an accumulation of knowledge is in direct contrast to the explanation offered by Kuhn of the discontinuous nature of scientific activity, which he defines as the interplay of normal and revolutionary periods of science. Given these types of differences, Kuhn's work still permits us to ask questions that we could not with Merton's. What is the nature of the subjective factors in the development of knowledge? How do bodies of knowledge relate to the social organization of science? How is knowledge legitimized as scientific? How do scientists acquire the knowledge necessary to operate in their field? What rules exist in the practice of science? How can knowledge be placed within its social and historical context? The list goes on - these are very general questions from which more specific ones may be gleaned.

The sociological analysis of science therefore had its popular origins in Merton, whose focus on institutional factors laid a groundwork for understanding how scientists conduct their work. Kuhn furthers the development in this area by rendering problematic the relation between knowledge and social/historical conditions. In spite of his contribution, we cannot in any way suggest that the sociology of science is now a completely adequate tool for studying scientific activity. Taken in combination, the approaches of Merton and Kuhn contain a very significant limitation - that being an almost exclusive concern with the internal dimensions of science, negating consideration of the external factors which influence the operation of that internal structure. We will examine the latter in the final chapter,

but for now our concern is to place Merton and Kuhn in the context of the historical development of the sociology of science.

The contention is here made that the combined approaches serve to provide a highly useful understanding of the internal dimensions of science. This statement would go in the face of the debate which has been waged between the two camps, but as in all such endeavours, the opposing forces often fail to see the fact that their supposedly separate arguments retain many common premises. A major aspect in this regard is the efforts towards obtaining some measure of analytical usefulness in the sociology of science. This would seem to be a common objective of both sides, although each has a tendency to view this task as requiring the establishment of the superiority of one position over the other - which may or may not be the appropriate course of action. We will rule out its utility here, because our concern is to establish a program for the sociology of science - a synthesis of the Mertonian and Kuhnian perspectives is to be used as a basis for this task. In order to arrive at a synthesis, it will be necessary to compare the two arguments with the purpose of looking for both the similarities and differences. The importance of the similarities is to illustrate common factors in the historical development of the sociology of science - in other words, for example Merton concentrates on the social organization of scientists, as does Kuhn, so that premise would be adopted as a useful starting point. The importance of the differences is equally important, for they can spell out the manner in which certain aspects of one approach may be seen as an improvement upon certain aspects of the other. Kuhn's emphasis upon the subjective factors of knowledge development in science would be one example of an improvement over Merton's assumption that objective factors remove any subjective interference on the part of the scientists. Therefore, by

conducting a comparison, we not only see what are the most useful aspects of each in terms of specifying a program for the sociology of science but also how the conceptualization of the field has developed since Merton's initial work.

A synthesis will not end our task, largely because of the inherent limitation of both approaches to focus on the internal dimensions of science. It will also be necessary to outline the external factors. However, the synthesis will provide a basis for specifying the move towards a sociology of science. To begin this move, we will compare the Mertonian and Kuhnian positions in the next chapter.

Chapter Three

Merton and Kuhn: the social structure of science

The Mertonian and Kuhnian analyses will be compared along the normative dimensions of science. This form of comparison, while establishing the similarities and differences between the two arguments, also helps to demonstrate the implicit sociological nature of Kuhn's analysis. Whereas Kuhn's original work was intended largely as an exercise in the history and philosophy of science, his (1970d:238) later work expresses the importance of accounting "for scientific progress by examining the nature of the scientific group".[11] Many similarities exist between the two arguments, even though Kuhn's is intended as a challenge to the image of science presented by Merton. These similarities are especially evident at the level of analysis to which Merton is chiefly concerned, namely the behavioural and organizational aspects of science. Kuhn goes beyond this level by attempting to relate the effect of bodies of knowledge and methodological techniques to those social aspects discussed by Merton. Comparison of the two should illustrate the contributions which Kuhn's approach can make to the traditional sociology of science, and provide a basis for the next chapter, which will examine what improvements could be made to the sociological analysis of science in general.

We will begin by examining the basic unit of analysis employed by Merton and Kuhn, that being the community structure of science, considered in terms of a normative framework. Discussion will then focus on the social control mechanisms operating within that framework, the competitive nature of scientific activity, how the two account for the progress of science, and

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11. See also (Kuhn,1970b:173) - "If this book were being rewritten it would therefore open with a discussion of the community structure of science . . .".

the role of discovery. Finally, consideration will be given to the factors involved in the determination of the scientific status of knowledge, and the role which tradition plays in the development of science.

Scientific Community

Kuhn and Merton both employ the concept of scientific community as the unit of analysis in the study of the social structure of science. Scientific communities are groups of scientists who utilize common bodies of knowledge and methodological techniques. These groups are the social units that produce knowledge, a point which is central to both arguments. Within scientific communities the substantive processes of decision-making and problem-solving occur, pointing to the social factors involved in the advancement of science. Kuhn and Merton seek to explain these sociological factors, the former implicitly and the latter explicitly, however, they differ in method of analysis. Merton is more concerned with the behavioural and organizational dimensions of the scientific community, detached from what it produces. Kuhn's analysis renders problematic the relation of what is produced to these units of production.

Kuhn and Merton develop remarkably similar definitions of scientific community. Each breaks the concept down into different levels, which possess varying degrees of institutionalization. At the general level is the entire community of scientists. This form of scientific community retains a high degree of institutionalization, expressed in a stratification system based upon the allocation of rewards for the development of original knowledge. Scientists are formally differentiated on the basis of status, which ultimately effects the evaluation of individual scientific work. As Merton demonstrates, mobility within science is largely controlled by those in the upper level strata, that is, those who have made important extensions to certified

knowledge. Therefore, the distribution of research grants, editorships, professorships, honourary awards, and so on, is determined by these higher status scientists. The effects of a stratification system is interpreted as functional by Merton's sociology of science, helping to ensure the progress of scientific activity. At the next level is the individual discipline, which also reflects a fairly high degree of institutionalization. For example, the discipline of physics, on which Kuhn in large part bases his work, has developed an internal structure which facilitates: the institution of professorships, university curricula that provide for the socialization of young physicists, the publication of journals and the formation of professional associations - both of which are involved in the evaluation of work conducted within the discipline. These forms of institutionalization can be found in each scientific discipline. Below this level is the specialty, or subdiscipline. These forms of communities have come about as a result of the increased specialization of science, and the demands which this places upon the performance of scientists. No longer is it possible for scientists to work within a broad range of interests, rather, the dictates of science require concentration upon restricted problem areas. Specialties feature well-established communication networks, necessitated by the fact that often only those working within the specialty can understand the nature of the problems under research. These communication networks are the essential means of institutionalization at this level of community, serving to define the boundaries of a specialty. At the lowest level would be groups of scientists working in particular research areas. Communication at this level is very informal, requiring only interaction with regard to common problems. These groups, lacking the institutionalized means found in higher levels of communities, are quite flexible regarding innovation and resistance to change.

As the level of institutionalization increases up the various levels, the degree of flexibility decreases.

Having established that groups of scientists are organized at different levels, Kuhn and Merton proceed to express similar ideas on the level to which most importance should be attached in analyzing the social structure of science. Merton considers both the community of scientists and the level of specialty to be of prime importance for sociological study. His emphasis on the macro-community is an effort to illustrate that scientists respond to common social forces, evidence of which is provided by the independent occurrence of the same discovery in remotely different places. Multiples characterize the history of science, the same discovery often taking place in several scientific communities within the same discipline. Were scientists working in isolation from one another, multiples would only rarely be realized - a premise attesting to the significance of the social character of science in the advancement of knowledge. As the social organization of science is presently oriented towards collaboration and research teams, the level of specialty becomes for Merton (1973e:328) the major focus for identifying what goes on in science. Certain specialties generate a great deal of research activity - Merton (1973e:331) refers to these as 'hot-fields'. These are highly competitive areas, characterized by a high rate of significant discoveries, the results of which have implications extending beyond the specialty. Research teams operate at a feverish pitch in these hot-fields - the quest for priority is intense. There is a lot of immigration into these fields, until they show signs of 'cooling off'. At this level, the size of scientific community would be comprised of the number of research teams working in a particular area.

Kuhn (1970b:177;1970d:253) also considers specialties to be the most

important unit of analysis. Members of these communities undergo similar education and initiation procedures, and absorb much of the same technical literature. They become familiar with certain common exemplary problem-solutions that are important to their area of study. These groups are characterized by a strong communication network, and a high degree of unanimity in the evaluation of scientific work. Kuhn suggests that a typical community at this level may consist of one hundred or fewer practitioners. Owing to the small size and high number of these groups, it is possible for individual scientists to be members of several communities, either simultaneously, or in succession.

Merton and Kuhn's focus upon communities at the micro level illustrates the difficulties that have come about as a result of the ever-increasing specialization of science. For one, the issue of communication across communities becomes problematic, simply because more communities are coming into existence all the time. The content of the research areas of each community differs to some extent - techniques used in one might not be understood in another - complicating effective interaction between members of separate groups. Yet, as Kuhn points out, communication across communities does take place, a combination of shared values and knowledge of different exemplars permit scientists in separate communities to have some ability at translating the others language into their own everday language. Shared values transcend community boundaries, and as both Merton and Kuhn demonstrate are common to the global community of scientists - thus helping to remedy the difficulties of communication. Shared values therefore provide a common basis which makes it easier for separate scientific communities to relate to each other. To be otherwise would jeopardize science, for several potentially conflicting interpretations of the purpose and operation of

science might emerge. By retaining a common outlook upon their role. scientists ensure a more rapid development of knowledge, unhindered by crises over the purpose of science.[12] Merton's notion of the entire scientific community permits an understanding of the social factors that effect all scientists, regardless of specialty or discipline affiliation. Kuhn's emphasis upon specialties stems from a contention that knowledge will develop more rapidly if scientists concentrate upon a small range of problems at a given time. The ever-increasing complexity of science helps to ensure that each specialty will be "its own exclusive audience and judge" (Kuhn,1970d:254). Further, the development of knowledge in a particular community will often pose ramifications for all of science. The theory of relativity developed by Einstein serves as an example.

From a sociological perspective, it would seem that the individual specialty is not the only relevant group for the scientist, for they are also part of the entire community of science. For a more complete study of the concept of scientific community, the contribution of Merton could be combined with that of Kuhn. This would permit an understanding of social factors common to all of science, as well as those factors which are peculiar to various scientific groups.

Normative Structure

The concept of scientific community implies some form of cohesion must exist within groups of scientists. Both Kuhn and Merton share a commitment to locating this cohesion by describing the structure of science in terms of a normative framework. Scientific communities are portrayed as

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12. Here I am referring to a philosophy of science, and its relation to the progress of knowledge. Should one consider the relationship between the institution of science and the wider society, it becomes evident that factors emerge which serve to disrupt the image of science presented. These factors are discussed in the final chapter.

exercising normative control over the conduct of members. The norms, beliefs, and values which are shared by groups of scientists are identified by analyzing their behaviour in the production of knowledge. While the nature of the Kuhnian and Mertonian analyses are similar, their method of explanation is somewhat different.

Merton's commitment to a framework of norms is readily apparent - the four norms which comprise the 'ethos of science' perform the function of integrating scientific activity towards the extension of knowledge. The motivation for commitment to these norms stems from an institutionalized reward system, which allocates recognition for scientific achievement. Merton's studies of priority in discovery demonstrate the basis for this commitment to a system of norms.

The norms which Merton describes are solely concerned with the social behaviour of scientists, and not with the content of scientific knowledge. A relation between the two exists in the sense that scientific knowledge results from scientists' commitment to a prescribed set of norms. Merton does account for the effect of existing knowledge upon the production of new knowledge by postulating the accumulative nature of science. New knowledge is, in effect, an improvement of old knowledge. Science realizes advancement in incremental fashion, a process which requires a combined effort on the part of scientists committed to a normative framework. Knowledge is, by these criteria, socially produced.

Essential to the maintenance of this normative structure are mechanisms for the diffusion and evaluation of knowledge. These mechanisms, coming in the form of the scientific journal and referee system, provide a source of constant reinforcement for the normative structure. Scientists are able to fulfill an obligation to communicate findings (communism), and to submit

them to objective scrutinization and determination of scientific worth (universalism, organized skepticism). These mechanisms also operate as a means of establishing priority, which forges a link between communism and disinterestedness. The scientific community places a premium value upon original knowledge, something which ensures that each scientist will publish swiftly in order to secure priority. Merton (1957:648) therefore relates priority and publication to the normative structure of science, a point which is, by his own admission, sociologically crucial - ". . . scientific knowledge is not the richer or the poorer for having credit given where credit is due: it is the social institution of science and individual men of science that would suffer from repeated failures to allocate credit justly."

The emphasis upon the institutional source of norms in science is a product of the functional analysis which informs Merton's work. For example, disputes over priority function to confirm the value given to original knowledge. The incentive to seek priority explains the relation of the normative structure of science to the allocation of credit (Sklair,1972:50). By this view, science would cease as an institution were scientists not sufficiently rewarded for their efforts. A number of sociologists (Cole and Cole,1973; Gaston,1973;Blume and Sinclair,1973) have followed up on this theoretical basis by attempting to empirically determine the distribution of rewards in science.[13]

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13. The Coles conclude from a series of studies that evaluation in the physical sciences closely resembles Merton's ideal of universalism. Just about all significant work is published, regardless of what characteristics its author might possess. However, the Coles find that work of lesser significance is more likely to receive attention if its author is well-known. Gaston reports similar findings in a study of high energy physicists, and Blume and Sinclair for chemists.

Kuhn's commitment to a normative framework can be seen through his concept of paradigm in the sense of disciplinary matrix. A paradigm represents a set of norms and values that provide a framework by which the behaviour of scientists can be understood. The paradigm establishes a normative cohesion for the members of a scientific community by specifying the nature of both: 1) norms and values, and 2) standards regarding the content of research in a particular area, and the methodological techniques to be employed. Scientists are socialized into the paradigm, and are thus clear about the nature of their role. Most of the time they are engaged in normal science, working towards the solution of problems posed by the paradigm. During these periods, the norms and values change very little - the dominant picture of science at these times is one of relative stability.

The essential feature of the normative framework described by Kuhn is that it involves shared elements, and is, therefore, inherently social. From a sociological standpoint, a similarity exists between values, norms and paradigms because they are shared by groups and are identifiable through common intellectual and social factors. Kuhn's notion of 'paramount values' (examples include the desire for consensus in a scientific community, accuracy of predictions, achieving precision in research; etc.) are instrumental in structuring scientific development and are therefore similar in purpose to Merton's 'ethos of science'. Most of Kuhn's examples of shared values are related to the more substantive aspects of science (laws, theories, hypotheses, and so on), whereas Merton's 'ethos' is strictly concerned with the behaviour of scientists.

Kuhn notes that shared values are useful in making choices between competing theories. When such a situation arises, values that guide scientific research will influence the decision that is ultimately made. The

manner in which values are applied may differ somewhat from instance to instance, creating the possibility of conflict. Merton also allows for the conflict of values, although he is more comprehensive on this matter than Kuhn - specifying both internal conflict (the need to establish priority and the desire to express humility about one's accomplishments), and external conflict (the findings of science contradicting the teachings of religious doctrine). Kuhn's work accounts for only internal conflict, and neglects consideration of the relationship between science and other institutions. His analysis as a sociology of science could possibly be improved by incorporating a set of norms and values as conceived by Merton with further specification of the norms and values related to the substantive aspects of science. Once achieved, this would provide a more complete study of the structure of scientific development.

In part, Kuhn accomplishes the task of delineating the norms and values tied in with the substantive aspects of science through description of the other shared elements that comprise a paradigm: symbolic generalizations, beliefs in particular models, and exemplary problem-solutions. These elements supply scientists with criteria that makes communication possible, a basic contributing factor to the social construction of knowledge. If every scientist were forced to employ an individually-constructed set of symbols, communication would be very difficult - requiring constant translation and exacerbating the problem of evaluating and establishing knowledge as scientific. Thus, without this social basis, the advancement of science would be a very slow process.

By Kuhn's view, scientists are committed to these elements that structure a paradigm, and therefore, to a normative framework. During periods of normal science, scientists work towards the articulation of the

paradigm that guides research in their area. Motivation to do so stems from the necessity to seek solutions to problems posed by the paradigm. In order to realize this goal, scientists must express commitment to: 1) concepts, laws, theories, 2) preferred instruments, 3) particular sets of writings to provide a source of inspiration, and 4) to science itself.

Kuhn allows for the periodical disruption of these commitments, coming in the form of scientific revolutions. When this occurs, a reconstruction of group commitments takes place. The content of certain commitments may change, for example, the replacement of one theory by another, but the form will remain the same. In effect, one normative structure is replaced by another, the content of which is redefined by the new paradigm.

Social Control

There is a close parallel between a description of the normative structure and a demonstration of social control in science. It is important to show that social control exists in science, which otherwise might be construed as a disarray of intellectual activity. Both Kuhn and Merton agree that science is a social activity, and that a scientific community exercises control over the behaviour of its members. They seek to illustrate the forces of control within the operation of the normative structure, which further points out the differences in the two arguments.

Merton's 'ethos of science' is claimed to be both intellectually and morally binding. If one accepts this contention, the question of social control virtually solves itself. However, as noted previously, widespread deviation to this system of norms and values takes place. Merton attempts to overcome this difficulty by suggesting that the locus of control cannot be situated within the 'ethos', but rather can only be understood through consideration of the link between it and the reward system in science.

Merton's account of priority disputes indicates that scientists place a high value upon the development of original knowledge. The commitment of scientists to this value functions as a control mechanism that serves to reinforce the goal of extending certified knowledge. As the reward system is an integral part of the normative structure of science, Merton's functional analysis is able to describe the parallel between social control and that structure.

Kuhn adds another dimension to the issue of social control in science. He maintains that the forces of control may be identified through the concept of paradigm - which guides the content of knowledge and conduct of activity for a scientific community, determining what problems should be solved, what methods used, and what are to be considered acceptable solutions. There may exist disagreement about the work which the scientific community is doing during these periods of normal science, however, outright failure to conform to the guidelines supplied by the paradigm will result in exclusion from the community. Should this occur, one would be forced to form another group or to work in isolation.

Mulkay (1975) claims that the Kuhnian analysis of the social structure of science suggests that the reward of recognition is controlled by the members of a scientific community. Essentially, recognition is distributed in accordance with the maintenance of normal science. A paradigm provides problems for scientists to work on, and therefore offers opportunity for recognition and mobility within the scientific hierarchy. Some problems will be considered more important to the advancement of science than others, and reward for their solution will thus be greater. As these major problems are solved, the opportunity for reward declines. In their effort to secure recognition, scientists may now be less likely to conform to existing trad-

ition, thus creating the optimal environment for revolution - which can basically be equated with a breakdown of social control.

Kuhn challenges the conception of the cumulative nature of scientific knowledge development with the notion of a normal-revolutionary science dichotomy. He shows that normal scientific activity is not simply an unrestricted quest for new knowledge, but is limited by the paradigm which the scientific community is using at any given time. The basis of Kuhn's argument is that such arbitrary limitations are beneficial to the development of science, as they force scientists to concentrate upon a small range of problems, enhancing the likelihood of periods of rapid accumulation of knowledge. From a sociological perspective, the important point to be drawn from Kuhn's argument is that the degree of consensus is related to the strength of social control within a scientific community. Scientific communities are able to achieve knowledge development because of a firm agreement about the internal structure of scientific activity.

Whereas Merton locates the sources of control within the social norms of science, Kuhn goes beyond this by adding the role which the more substantive aspects of science perform - for example, exemplary problem-solutions, symbolic generalizations, and so on, all of which are placed in the context of a paradigm. The paradigm exercises for Kuhn a form of control over a scientific community, and is more important than the more readily visible mechanisms of control such as journal editing and refereeing. Similar to the Mertonian analysis, Kuhn's implicit account of social control can be related to the processes of rewarding contributions of original knowledge. Both Kuhn and Merton thus locate the control mechanisms in the social character of science.

Competition

One of the distinctive features of science is its competitive nature. The degree of competition is directly related to the social structure of a given scientific community. Merton's study of priority disputes is an illustration - scientists engage in competition for the socially-valued product of original knowledge. Those who are successful are rewarded in various forms for their efforts. Within a scientific community at the level of specialty, the intensity of competition is related to the number of scientists working on particular problems. Competition will be severe if a large number of scientists or research teams are attempting to solve the same problem. As a result, the solution should be achieved in much shorter time than if only a few scientists were working on that problem.

As scientific communities are stratified, scientists have unequal access to the resources required for their work - those on the upper levels will have greater access than those at the lower levels. This poses ramifications for the individual scientists' ability to compete. Those at the lower levels are continually operating under a disadvantage, a factor pointed to by Merton's study of the 'Matthew Effect' in science. For these scientists the problem is compounded by the fact that the upper-level scientists control in large part access to needed resources, and hence the rate of mobility within science. As the Coles (1973:81) remark:

> (the latter) as members of the most prestigious departments, decide who among the younger scientists can receive continuing appointments at the major institutions; they nominate new men to positions and memberships in honorary societies; as editors of journals they decide what should and should not be published . . . (as) administrators of foundations, laboratories, and government agencies, (they) are influential in setting policy on what sciences or scientific specialties should be heavily funded. They can determine what specific research areas are to receive priority, and what individuals are to receive support for their research programs.

As long as original knowledge is valued, the only alternative to competition is cooperation. Scientists form research teams in an attempt to solve the particularly difficult problems, and once solution is achieved, divide the rewards among themselves. The emergence of research teams during this century exemplifies a situation of cooperation. However, such cooperation does not eliminate the competitive nature of scientific activity, for often entire research teams compete against each other. As well, individual scientists can cooperate in the initial search for solution to a problem, and then compete for completion of the discovery. Cooperation further does not eliminate the stratified structure of science - those members of research teams with higher status will receive a greater share of the rewards.

Merton's 'ethos of science' and Kuhn's 'normal science' suggests that scientists do cooperate in the production of knowledge. By communicating their findings, scientists provide one another with information that is potentially helpful in furthering research - 'invisible colleges' of scientists exist, comprised of scientists who work cooperatively (de Solla Price, 1963;Crane,1972). Furthermore, by concentrating research efforts on a specific area (for example, solving the problems in a given paradigm), scientists produce more and improved knowledge than they would by operating in a haphazard fashion, selecting problems at random. Perhaps the most fundamental motivation for scientists to cooperate is its basic necessity when dealing with a highly complex body of knowledge. The more complex knowledge becomes, the more difficult it is for scientists to work in isolation.

Conditions of cooperation directly effect the social organization of science, a factor which can be demonstrated by viewing science as a competitive activity. For example, within a scientific community clusters of scientists engage in competition for the solution of a particular problem.

These groups utilize certain research tools that they consider to offer some advantage for the competition. The standards by which these tools are chosen can be very arbitrary, as Kuhn notes. Competitive conditions force the selection of those research tools that are deemed to best permit achievement of the solution to a problem. Consequently, the use and development of research tools becomes a more intense procedure than it would if science did not retain a competitive nature.

Kuhn and Merton thus present very similar arguments concerning the role of competition in science. Both suggest that the prime environment for scientific advancement is created by concentrating research upon the solution of specific, central problems. This allows knowledge to advance in a more useful way than would occur if scientists spent an inordinate amount of time working on peripheral problems. The intellectual consensus necessary for the pursuit of solutions to scientific problems is therefore a derivative of the social nature of science. In relation to the organization of scientific communities, conditions of competition and cooperation are quite compatible.

Progress in Science

Both Kuhn and Merton consider the development of science to be synonomous with scientific progress.[14] For Kuhn, progress in science occurs through the interplay of a normal science - scientific revolution dichotomy. For Merton, progress in science stems from the cumulative character of scientific development. We will here discuss the various aspects of change as they relate to progress in science, paying particular attention to how Kuhn's views compare with those of Merton.

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14. 'Progress' implies a qualitative form of change, the change from a to b means that b is in some sense considered to be an improvement upon a.

Kuhn's work is essentially a study of the dynamics of scientific change. His breakdown of scientific activity into categories of normal and revolutionary divides the types of changes that can occur. Only minor changes will take place in periods of normal science, related to the achievement of specific problem-solutions. Accumulation of knowledge results from the successful solutions of problems, but is confined to the paradigm and is not in any sense universally continuous, as under the traditional scheme represented by Merton. Kuhn denies the continuity of science, and therefore a conception of cumulation, through a holistic view of paradigm change occurring via revolution. It is here that the major changes in science take place, permitting scientific progress. This is very much an evolutionary model of scientific growth, as opposed to the additive character of Merton's conception.

Merton conceives of science as an inherently stable enterprise, this stability being necessary for its growth. Kuhn, in contrast, posits the instability of science, claiming that this condition is necessary for its evolutionary development. For Merton, change means accumulation, and occurs as a result of the normative structure of science. Failure on the part of scientists to adhere to the various norms and values is simply labelled as deviance. From Merton's position, incidents of deviance in science present little problem - they are merely passed off as failure to conform, and not as issues which deserve consideration apart from the dictates of the normative structure. Witness the case of Velikovsky.[15]

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15. In 1950, Dr. Immanuel Velikovsky published Worlds in Collision. The work challenged many of the accepted scientific theories of the time. Velikovsky's ideas depended almost entirely upon his interpretation of evidence derived from ancient legends. Legends are not considered to be scientific material, so Velikovsky was labelled a pseudo-scientist because he deviated from 'acceptable'

Kuhn, on the other hand, is more concerned with deviance from the substantive aspects of science, in other words, the paradigm. He claims that the variability of importance each scientist will attach to shared values is perfectly acceptable and does not serve as a disruptive force. Deviating from the guidelines of the paradigm will create a situation of conflict, which must be resolved either by sub-dividing the specialty or excluding the deviants from the mainstream of activity (Kuhn,1970c:21).

Kuhn (1970b:206-207) illustrates under what conditions scientific development may be viewed as progressive. Here it must be noted that he is speaking of theories only, and not entire paradigms. Theories represent part of the methodology and knowledge content of a paradigm, and it is important to remember that the two are not strictly equatable. Kuhn states that scientific theories can be demonstrated as improvements upon previous theories, their measure of worth dependent upon a superior ability to solve the problems posed within a particular paradigm. A choice between theories stems from the judgment of the scientific community. The movement from one theory to a better theory for Kuhn represents scientific progress. Progress therefore means an increase in problem-solving ability.

There is, however, an essential element which distinguishes Kuhn's notion of progress with the image of science defended by Merton. The latter's viewpoint considers a scientific theory "to be better than its predecessors not only in the sense that it is a better instrument for

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methods of research. The Mertonian analysis would be forced to reach this conclusion, negating consideration of the fact that the 'deviance' in this case might have resulted in a new approach to research. As it turns out, several of Velikovsky's ideas were later verified by other scientists.

discovering and solving puzzles but also because it is somehow a better representation of what nature is really like. One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth" (Kuhn,1970b:206). As Kuhn suggests, this perspective does not refer to the concrete aspects of the purpose of theory, that is, to provide a means of explanation and prediction, but to the ontological level of relating a theory to the reality of nature. In contrast, Kuhn would suggest that the progressiveness of science is rooted in the coherence supplied by a paradigm. The purpose of a paradigm is not simply directed at the pursuit of some end such as the 'truth', because truth cannot be treated as an absolute. What is considered 'true' today may be demonstrated as 'false' tomorrow. The chief purpose of a paradigm is to provide a scientific community with a common understanding of their activity. One attains an understanding of the development of science by comprehending the (paradigmatic) confines within which it takes place.

Merton would suggest that a high degree of cooperation amongst scientists is a necessary prerequisite for the progression of science. Kuhn, on the other hand, would suggest that cooperation is essential during periods of normal science, but that it is not a continuous feature of science - otherwise scientific revolutions would never take place, and science would not experience major change. Conflict, the antithesis of cooperation, is a basic requirement for the occurrence of a revolutionary upheaval - which, by Kuhnian criteria, is essential for the production of new knowledge in science, here equated with scientific progress. Merton overemphasizes the cooperative aspects of science, a problem which stems from his functional theoretical analysis. Kuhn manages the difficult task of accounting for both the persistence of cooperation and conflict

in accord with the nature of the development of science.

Discovery

The Kuhnian and Mertonian conceptions of scientific discovery retain certain similarities, yet also differ on some fundamental aspects. Neither Kuhn nor Merton provides an explicit definition of 'discovery'. Presumably for the former, the term refers to small-scale innovations produced in periods of normal science, and innovations of major proportions occurring as a result of revolution. For the latter, the term refers to a new 'fact' which is the recipient of empirical verification. Traditionally, the development of scientific knowledge is associated with the concept of discovery, a viewpoint which is adopted by Merton and in part questioned by Kuhn.

The similarities which exist concerning a conception of discovery expresses the sociological nature of both arguments. Each acknowledges that discoveries are produced by social interaction, the result of the operation of scientific communities which utilize common methods of inquiry, or in Kuhnian terminology, 'research tools'. These shared methods serve to minimize the intellectual-ability differences of scientists - hence the socialization process is crucial so that young scientists may acquire a proper understanding of the relevant methods of inquiry. Both Kuhn and Merton stress the importance of the socialization process.

Kuhn and Merton emphasize the necessity of placing discoveries within a social and historical context. Merton accomplishes this point by suggesting that discoveries are products of their own time, and are not random occurrences. Kuhn covers this point through his concept of paradigm, each of which represents a social and historical context. A paradigm defines the nature of both problems and solutions, mitigating against the

possibility of accidental discoveries, although these do occur. Merton's study of multiples reinforces the necessity for discoveries to be placed within a social context. The independent occurrence of the same discovery suggests that common forces are impinging upon all members of the global scientific community. Within the Kuhnian scheme, multiples "occur as a result of the convergent expectations of normal science, by which anomalies will be recognized by independent (yet paradigm-sharing) scientists" (Martins,1972:50).

Part of the social conditions that permit discoveries to take place are the criteria scientists employ for choosing the types of problems they will work on. For Merton, the major criterion is the extrinsic reward of recognition. Discoveries vary in importance insofar as a contribution to scientific development is concerned, therefore, scientists are more likely to work in areas that offer possibility for the greatest reward. Similarly, the Kuhnian analysis of the nature of science can be set out in much the same way. Though the paradigm determines what problems should be solved, it is ultimately the members of a scientific community that determine the importance of individual problems. Those of greatest importance will receive a larger share of attention than problems deemed to be peripheral to the development of science.

Where Kuhn and Merton differ concerning discovery stems from the basic nature of their respective positions. Merton contends that scientific development proceeds in a unidirectional fashion through the accumulation of discoveries. Discoveries are by this view readily identifiable acts or events, and can be ascribed to a particular individual or individuals. In effect, this represents the traditional image of science as employed by social scientists and historians.

Kuhn does not accept this argument, based upon his contention that the conception of the cumulative nature of scientific development is in error. He maintains that because the scientific community attaches so much importance to recognition contributes to the popular image of discoveries as being isolated, single events, the efforts of particular individuals. Kuhn claims that discoveries are not so tangible as to be ascribed to a particular place, person, and time. Rather, discoveries must be understood in a social and historical context, which is supplied by a paradigm. Discoveries in effect represent changes in collective means of interpretation represented by the paradigm. During these 'revolutionary' periods of scientific development, discoveries cannot be isolated with any degree of exactitude. To do so would presuppose that paradigm change is a fairly common and rapid occurrence, a claim which would vitiate the presentation of Kuhn's image of the history of science.

Consider the case of the discovery of oxygen, for example. Kuhn (1970a:53-55) notes that at least three individuals could legitimately lay claim to the discovery. Scheele first isolated the gas, however, he did not publish his findings in time, and therefore we should ignore his work, says Kuhn. The second claimant would be Priestly, who in 1774 collected gas from the red oxide of mercury and mistakingly labelled it as nitrous oxide (more commonly known as laughing gas). With the recognition of his mistake, Priestly conducted further tests and came to the conclusion that the gas could be nothing more than dephlogisticated air. Lavoisier, the final claimant, recognized Priestly's mistake, first took the gas to be pure air, and finally as a distinct species, one of the two main gases in the atmosphere.

Based on this recount of the incidents which led to the isolation

of oxygen, who could properly be claimed as the discoverer? Certainly Priestly's priority in the matter is important, based upon his isolation of a gas later identified as a distinct species. However, Priestly did not have a pure sample, "and, if holding impure oxygen in one's hands is to discover it, that had been done by everyone who ever bottled atmospheric air" (Kuhn,1970a:54). Lavoisier could lay a stronger claim to the discovery, yet to the end of his life he insisted oxygen to be a "principle of acidity" - oxygen resulted "only when that principle united with caloric, the matter of heat". Both the principle of acidity and the caloric were banished from chemistry by the 1860's. By then, oxygen was firmly established as a standard chemical substance.

This example illustrates the difficulties in precisely locating the time when oxygen assumed its present identity. Theories of valence, diatomicity and specific heats had to be developed before the 'fact' of oxygen's discovery could be recognized. The recognition of oxygen which exists at present depended on the development of knowledge which would permit the correct identification to take place.

Both Kuhn and Merton recognize that occasionally discoveries occur that were unanticipated yet are compatible with existing scientific knowledge. Merton (1968:157-162) terms this discovery by chance the "serendipity component of research". He defines the serendipity pattern as the "fairly common experience of observing an <u>unanticipated, anomalous, and strategic</u> datum which becomes the occasion for developing a new theory or for extending an existing theory". One might question whether or not this type of discovery is a fairly common experience - nevertheless, it is a fair assumption to make that it does occur. The unanticipated aspect is self-explanatory, however, Merton uses 'anomalous' in a sense which differs

from Kuhn's understanding of 'anomaly'. For Merton, anomalous refers to an observation made by a researcher which involves some element of surprise, because some inconsistency is discovered with the "prevailing theory, or with the established facts". This inconsistency calls for some solution; in effect the researcher must "make sense of the datum, to fit it into a broader frame of knowledge". And finally, the 'strategic' aspect means that the unexpected fact . . . must permit of implications which bear upon generalized theory . Here Merton is referring to "what the observer brings to the datum than to the datum itself". The serendipity pattern exerts pressure on the researcher to change the direction of inquiry in an effort to extend theory.

For Kuhn, serendipity can be very much a part of revolution. During times of extraordinary research when a group is attempting to redefine its area of work, often a new paradigm will emerge that permits an understanding of phenomena that had been entirely unsuspected under the old paradigm.[16]

Through his account of the normal science - scientific revolution dichotomy, Kuhn examines the social processes by which scientific discoveries take place. What is discovered during periods of normal science is relatively unimportant to the progression of science. Only those discoveries which initiate a crisis situation - and in turn instigate a scientific revolution - significantly effect the development of science. During the occurrence of these processes, the nature of what is discovered is related to the existing methods and knowledge supplied by a given paradigm. What

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16. Kuhn (1970a:154-155) provides the example of Einstein, who "seems not to have anticipated that general relativity would account with precision for the well-known anomaly in the motion of Mercury's perihelion, and he experienced a corresponding triumph when it did so".

is discovered and how it is discovered are factors of equal importance - for the methods and content of each paradigm will vary, or are, in Kuhn's words, incommensurate.

Kuhn's work places the traditional notion of 'discovery' in a different context, for no longer can discoveries be viewed as the assertion of pure facts, a contribution to the linearity of scientific development. Science is not a conglomeration of facts, laws, and theories, says Kuhn, therefore, it is a difficult and unnecessary task for the historian of science to identify the individual(s) responsible for a particular discovery. Instead, the historian's concern should be to place discoveries within the framework of a particular time - to display the distinctiveness of science in the various periods of its development. It is in this sense only that Kuhn's conception of discovery differs from that of Merton - in all other aspects their arguments are quite similar.

Demarcation

Kuhn and Merton possess a very similar understanding of the demarcation criteria used in science. Demarcation simply means making a distinction between what should or should not be considered scientific knowledge. This follows a basic requirement of science: there must exist a means of according scientific or non-scientific status to knowledge. Were this not the case, then it would be difficult not only to distinguish science from other forms of knowing, but also to realize the advancement of science.

Both the sociologist Merton and historian Kuhn seek to establish a basis for demarcation criteria within the sociological nature of science.[17]

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17. This point is implicit in Merton's analysis of the social behaviour of scientists. For Kuhn, see his 1970c, especially section one.

Each stresses the autonomy of science from other social institutions - scientists are the only ones capable of validating knowledge as scientific, and therefore, scientific groups are portrayed as the only relevant audience for that purpose. The process of validation stem from commitment to a normative framework. From Merton's perspective, scientists are provided with procedural norms for the allocation of scientific or non-scientific status to statements of purported scientific value by following the 'ethos of science'. As science advances in incremental fashion, scientists must also express a commitment to current forms of knowledge. Scientists do not strive to displace all current knowledge, but to use it as a building block upon which to further scientific development. Hence the premium which is placed upon 'original' knowledge. The offer of the reward of recognition helps to ensure that scientists will stay 'in bounds', that is, employ what are considered acceptable methods and techniques in the creation of new knowledge. Socialization of young scientists is very important, for it is necessary that they acquire proper understanding of what is considered acceptable by current scientific standards. Mechanisms to accord or deny scientific status are institutionalized, coming in the form of the journal and referee system. Once published, the scientist's work is legitimized, and displayed as potential for further development by becoming part of the total fund of knowledge.

Similarly, Kuhn emphasizes the importance of a commitment to scientific norms and existing knowledge. He suggests that it is normal science where demarcation criteria may be found, not in revolutionary science. Normal science is guided by a paradigm which informs the nature of a scientific community's work. The paradigm is comprised of shared values, models, and beliefs - to which the community members are committed. Through the use of

common exemplary problem-solutions, scientists share a commitment to existing knowledge, one which is acquired during socialization.

During periods of normal science, the articulation of the paradigm is carried out. The paradigm in effect generates puzzles which scientists attempt to solve. Once solution is achieved, validation as scientific automatically follows. Kuhn makes this point by comparing the concepts of testing and puzzle-solving. Basic commitments (to certain methods, values, and so on) undergo testing during revolutionary science. However, the points that are tested and the manner in which such is carried out is determined by normal science. During normal science, the testing of basic commitments generally does not occur (unless occasioned by an anomaly). Scientists are mainly concerned with puzzle-solving, something which does not take place during revolutionary science. Agreement must exist over the solution to a puzzle - this is supplied by commitment to the paradigm, which breaks down during revolutionary science. Therefore, for science to progress, it must have the ability to generate puzzles. The Kuhnian scheme thus negates the necessity of continually testing procedures to determine scientific status. A science must achieve a state of normal science in order to be legitimated. Periods of revolutionary science may produce change, but procedures for continued operation are absent, a problem which is rectified by the emergence of normal science.

The Role of Tradition in Science

Merton and Kuhn both emphasize the importance of tradition in the progress of science. Merton's postulation of the cumulative nature of science means that discoveries have their basis in existing knowledge. What knowledge exists at present is therefore vital to what new knowledge will be developed. From the very beginning of their careers, scientists

undergo a socialization process which reinforces the current tradition. They are encouraged to maintain an attitude of skepticism when presented with new findings - and only to evaluate such purported additions to scientific knowledge in terms of 'pre-established criteria'.

Similarly, the role of tradition in scientific research is one of the chief points that Kuhn is trying to get across - to indicate that the cognitive aspects of science have a social dimension. That is, traditions of thought dictate to a certain extent the social conduct of scientific activity. This places a limitation on scientific progress, helping to ensure that high standards are maintained in determining the validity of new findings. In what basically amounts to a contradiction, scientists, committed to seeking new knowledge, will strongly resist revolutionary innovations. When a revolution does succeed, the paradigm which it produces becomes a tradition in itself. Effectively, Kuhn's revolutions amount to the replacement of one set of dogmatic beliefs and assumptions by another. Therefore, the revolutionaries of a given time period become the conservatives of the next, similar to other social groups. Tradition, it would seem, predominates in scientific research - stemming in part from the social mechanisms which govern that activity.

Critiques of Kuhn generally place too much emphasis upon the notion of scientific revolutions. It is understandable why this is so, for revolutions certainly have more impact as far as scientific development is concerned than do periods of normal science. As this is the case, revolutions will attract more attention. However, revolutionary periods constitute only a minute proportion of scientific activity. Most of that activity is concerned with the conduct of normal science. These periods of science are innately conservative, where scientists exhibit more dogmatic than revolu-

tionary tendencies. Scientists are mainly committed to solving puzzles within the criteria of a given framework, not to undermining the basis of that framework.

Summary

There are striking similarities between the Kuhnian and Mertonian analyses of science. Both examine the sociological nature of the production of scientific knowledge, utilizing the scientific community as the unit of analysis. Scientific activity is structured by a normative framework, which regulates the conduct of scientists. The development of science comes about because mechanisms of change are institutionalized - in Merton's case, by a reward system that allocates recognition for contributions of original knowledge; and in Kuhn's case, by the interplay of normal and revolutionary periods of science. Both Kuhn and Merton therefore locate the source of change in the social character of science.

Where these similarities exist, the two analyses may be used to complement one another. For example, each focuses upon the concept of scientific community at the level of specialty - Merton as well stresses the the global level - the combination of which provides an understanding of the social structure of both levels, and the relation between the two. Similarly, their respective conceptions of a normative framework readily complement each other. Merton provides a detailed analysis of the social behaviour of scientists, and acknowledges that they utilize common methods of inquiry. Kuhn focuses more on the latter through his concept of paradigm, which emphasizes the significance of shared methods, for example, exemplary problem-solutions. By positing the connection between the consensus obtained by a paradigm and the community structure of a scientific group, Kuhn provides a legitimate means to indicate the institutionalization

of science, something which is decidedly missing from his earlier work. Furthermore, their commitment to a normative framework illustrates that norms and values must always exist - a factor which ensures the continuity of science. Though for Kuhn a set of norms and values may be overthrown during a scientific revolution, they will be effectively replaced by another set. This can be seen when the unanimity of one scientific community is destroyed, producing two separate groups. When this occurs, new normative patterns are constructed on the basis of common agreements on the part of scientists in each group.

The major difference between the two arguments concerns the processes whereby scientific change leads to development. Kuhn's notion of the overthrow of existing traditions suggests that while it is important to emphasize the coherence of knowledge, it is not necessary to equate scientific development with accumulation. Here the progress of science is directly dependent upon the ability of the existing tradition to produce puzzles and solve them. Once this ability becomes suspect, and no longer proves tenable, science will not progress unless an overthrow of the tradition takes place. Merton, in contrast, suggests that new knowledge merely replaces old knowledge, and in this manner permits science to progress. Anything approximating the treatment of knowledge in terms of revolutions has no place in his argument. While Kuhn and Merton differ on the issue of how knowledge develops, the important point is that they both consider it important to analyze change in science in terms of the social factors involved.

Two factors emerge from the comparison which are of importance to the specification of a program for the sociology of science. One, the similarities and differences in the two positions indicate the develop-

ment of a pattern in this field of study. Two, they can also be equated with various conceptions of science, a point which we shall discuss in some detail in the next chapter.

As far as the first factor is concerned, Merton began his work by concentrating upon the social organization of science, the sociological importance of which he has emphasized throughout his career. This became the starting point for analysis in the area - the pattern was set, so to speak. Kuhn also unhesitatingly adopts the usefulness of this approach, in spite of the fact that his image of the nature of science is diametrically opposed to Merton's. Kuhn maintains the pattern initiated by Merton, he does not reject all that the latter has to offer in establishing his own position on the discontinuous nature of scientific development, as many Kuhnians would have us believe. Certainly, Kuhn does reject the notion of the cumulative nature of science posited by Merton, but only partially. For while Merton would not accept the contention that entire bodies of knowledge in a scientific community may be overthrown, Kuhn will accept the accumulation of knowledge during periods of normal science. All science is for Merton "normal" in this sense, proceeding in undisrupted fashion through the extension of knowledge. The nature of scientific knowledge is thus unproblematic for Merton, permitting him to focus upon the organization and behaviour of scientists. Kuhn can also focus on this aspect through his concept of normal science, but he does render the nature of knowledge to be problematic - here he extends the pattern begun by Merton. Anomalies may surface which cause a particular body of knowledge to be of limited value - it therefore disrupts the research activity in a scientific community, throwing it into turmoil until a resolution is reached. If accomplished by revolution, a redefining of research activity

will take place. New bodies of knowledge will be utilized which may be substantially different in content and style from their predecessors. Kuhn thus not only considers it important to analyze the social organization of science, he also emphasizes the utility of relating scientific knowledge to a social context. Given the combination of the Mertonian and Kuhnian analyses the pattern as it now stands consists of (1) science as a social activity and (2) science as bodies of knowledge and methodological rules. The focus of this combination is upon the internal dimensions of science.

The second factor to emerge from the comparison is the various conceptions of science in the pattern that has been set for the sociology of science by Merton and Kuhn. Both see science as social activity, this is the starting point of their respective analyses. Kuhn's work adds the conceptions of science as knowledge and rules - which can not only be treated as distinct in themselves, but as related to science as social activity. It is the connection rather than the distinction that is of interest here. For it is the connection between these three conceptions that spells out the pattern of development in the sociology of science.

That pattern is faced with an obstacle to further development, namely the fact that the three conceptions of science derived from the works of Merton and Kuhn share the limitation of failing to account for the external factors which influence scientific activity. The latter is a crucial issue to the sociological analysis of science - placing the institution of science in the context of the wider society would help to explain, for example, why Merton's description of an 'ethos' is at best idealistic. The task would then be to describe what factors influence the normative structure of science - factors which are surely rooted in more than

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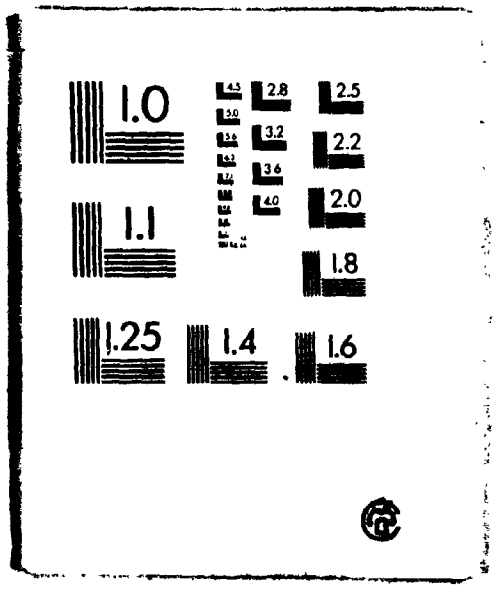

strictly scientific interests - and become evident if one considers how other institutions use science to their benefit.

This obstacle to the sociological analysis of science can be easily removed, and, once this is done, creates a whole new set of problems for study. Determinants of research problems are now given more meaning when economic, political, career mobility, technological advancement, and a host of other factors enter into the picture. These in turn have an effect on the internal structure of science, that is, scientist's work. Certainly one cannot accept any suggestion that determinants of research problems are strictly based upon the whims and interests of scientists. Yet this type of conclusion is much more likely to be reached if account is not taken of external factors. Therefore, it will be the task of the following chapter to describe the various conceptions of science that have emerged from the works of Merton and Kuhn, and to deal with the question of the external factors in specifying a program for the sociology of science.

Chapter Four

Towards a Sociology of Science

The thesis here proferred is a specification of certain conceptions of science, and how a synthesis of these would enhance sociological analysis in this area of study. Existing sociological work generally concentrates upon one conception of science at the exclusion of others. Examples of this are the most influential works in this area, those of Robert Merton and Thomas Kuhn. Merton's approach examines science as a social system. Focus is given to the manner in which scientist's organize their activity. The central idea is that science is characterized by a normative structure which guides scientists in their efforts to extend empirical knowledge about the natural world. Kuhn's approach, though originally intended as a study in the history of science, has had considerable impact upon the sociology of science. His chief concern is to describe the characteristics of scientific knowledge that produce change and development in science. Kuhn relates bodies of knowledge and methodological techniques to social gatherings - the operation of this relationship results in the production of scientific knowledge.

Both approaches by themselves do not represent an adequate sociology of science. Merton does not pay enough attention to the importance of the characteristics of scientific knowledge in the development of science. The nature of one piece of knowledge is assumed to be identical with the next, and is therefore rendered a non-problematic status. Kuhn does not provide a description of exactly what it is that scientists share - while his emphasis upon consensus as a factor in the development of knowledge is heading in the right direction, he does not detail the tenets of that

consensus. Both approaches exhibit a major shortcoming by not incorporating analysis of the external influences upon the structure of science. A multitudinous body of literature in the past two decades provides strong support for the claim that most scientific research is influenced by factors external to science.[18]

The program for the sociology of science which is here proposed would follow the line of analysis begun by Merton and continued by Kuhn, and would extend this to include consideration of the external influences upon science. It would therefore emcompass the following conceptions of science: 1) science as social activity; 2) science as knowledge; 3) science as techniques or rules; and, 4) the external influences upon science. Until the advent of Kuhn's contribution, sociologists of science concentrated almost exclusively on the first point, because it is considered to be an inherent aspect of sociological analysis. Kuhn has offered a basis for consideration of points two and three. By combining the efforts of Merton and Kuhn on the first three points, as well as outlining the significance of the fourth point, the basis for a theoretically and empirically useful approach to the sociology of science may be achieved.

Science as Social Activity

The basic assumption of work in the sociology of science to date is that science is a social activity. Most work in this area is based upon a functionalist theoretical approach, and seeks to explain the behaviour and organization of scientists in terms of social groupings characterized by a normative structure. This approach is exemplified in the works of Merton, Barber, Hagstrom, Storer, the Coles, et al. Merton is the intellec-

18. The majority of this literature is not explicitly sociological, however.

tual father of this school, so to speak. The debate over the validity of functionalism aside, it is important that the emphasis remain upon studying science as a social activity, for this is a point which is intrinsic to sociological analysis. However, attention must not be focused exclusively upon this aspect, for that would negate consideration of other important facets in the study of science. Traditionally, scientific knowledge is viewed as a product that is placed in the market of exchanging original knowledge in return for recognition. The quality of the product is measured by its exchange value, that is, greater increments of recognition are accorded for high-quality work than for low-quality work. This view analyzes the means of production detached from what is produced. Social factors are therefore isolated from cognitive factors. Further, this traditional approach to the sociology of science is in the main concerned with the internal dimensions of the structure of scientific activity, and does not analyze the effect which extra-scientific factors have upon the development of science. By including analysis of both the internal and external dimensions, the sociology of science would provide a more complete understanding of science as social activity.

Science is based upon a complex social organization, comprised of a network of social groups known as scientific communities. The problem for the sociology of science is to locate the intricate relationships within these social groups - a task which involves two aspects. One is to consider the internal dimensions of science; for example, the norms and values that govern scientific work, the division of labour created for the most efficient performance of research activity, the communication of ideas and findings between scientific groups, the commitment of scientists to various exemplars, and so on. The operation of these internal dimensions

serves to structure scientific activity and to maintain the social system of science. A second aspect concerns the external dimensions of science, that is, its social setting. Science is an institution existing in cohabitation with other major institutions, for example, the political and economic sectors of society. These institutions do not operate autonomously, but rather are interrelated. Therefore, it is important to examine the effect of external influences upon the internal structure of science, to see, for example, how economic and political considerations can in part determine the types of problems that scientists will work on. The sociology of science should therefore involve an examination of both the internal and external dimensions of scientific activity. Focus upon one aspect at the exclusion of the other will not provide a complete analysis, for both play an important role in the structure of the social organization of science.

Merton and Kuhn both make the basic assumption that science is a social activity, and this is the starting point of their respective analyses. Both focus primarily upon the internal structure of science, providing an analysis of the normative dimensions of this structure, and how this relates to social groupings of scientists. Merton, however, stops at this point. His image of science is constructed on the basis of examining its social organization, as we discussed in chapters one and three. Kuhn, on the other hand, begins at this point. He proceeds to demonstrate the connection between the community structure of science and bodies of knowledge based upon empirical observations of the history of science.

The emphasis upon the internal structure of science means that the Mertonian and Kuhnian analyses are addressed mainly to the conduct of pure research, as opposed to applied research. Very briefly, the distinction between the two is that the former involves a response to internally

generated problems, whereas the latter is a response to problems posed by extra-scientific interests. As Shepard (1956) suggests, the distinction is not between types of science, but between different social contexts of research activity. Mulkay (1977:95) provides a summary of the characteristics of these different contexts. Within the social context of pure research, problems are pursued mainly on the basis of their significance - findings are intended solely for the extension of scientific knowledge. The audience for evaluation purposes is other scientists working in the same area - who exert control over research activity by reference to scientific standards (Cole and Cole,1967). The social context of applied research is somewhat different. Here research is expected to have practical consequences - findings are to be used for purposes other than solely the extension of scientific knowledge. The audience receiving the results is comprised mainly of non-researchers, who exercise formal control by means of bureaucratic authority over salary and promotion (Marcson,1960).

Therefore, application of Merton's 'ethos' or Kuhn's shared values can only be made in the context of pure research. But an effective sociology of science would have to take account of the context of applied research as well. Many of the criticisms surrounding the two can be attributed to a failure to distinguish in which context of science the respective arguments are intended. For example, Merton's ethos comes under considerable attack for its idealistic portrayal of the scientific normative structure. Norms like universalism and communism are found to be unable to gain widespread empirical verification - a factor which becomes evident when extra-scientific influences are taken into account. The end result is that his conception of a normative structure must be limited to the

context of pure research. The task then for the sociology of science would be to formulate a normative model that could be applied in both the pure and applied research contexts - a task which can be accomplished by retaining the basic assumption that science is a social activity. Similarly, Kuhn's notion of the transition from one paradigm to another could be better understood if external factors are taken into account. His explanation is primarily based upon internal considerations, and would require modification to be placed in the applied research context.

Further, problems involved in defining the concept of scientific community would be clarified somewhat through consideration of external interests rather than the internal requirements of science. Merton and Kuhn both postulate the concept of community in conjunction with the notion of science as an autonomous activity. Communities of scientists are portrayed as being bounded by common intellectual interests, a factor which could be substantiated were all science characterized by pure research. Most scientific activity is oriented towards applied research, however, and it is to factors related to this context that we must look for the boundaries of a scientific community - for example, the economic and political influences on the research activity of groups of scientists. These external influences have greatly changed the structure of the social system of science as conceived by the traditional sociology of science. So must our conceptions of that structure change as well, to include study of the relationship between both the internal and external dimensions of science.

Science is a social phenomenon, and therefore, a conception of it as social activity is central to sociological analysis. Merton laid the groundwork for this approach, and Kuhn provided an extension of the

boundaries of study. Basically, the latter's contribution amounted to improving the level of conceptualization of the normative structure of science, and this in part was accomplished by relating science as a social activity to bodies of knowledge and methodological procedures. Whereas social processes were problematic to the traditional sociology of science, Kuhn also renders cognitive processes problematic. He introduces the conception of science as knowledge and rules, in the process adding new factors to the sociological analysis of science.

Science as Knowledge

The etymological implication of the term 'science' suggests an association with a body of knowledge. Perhaps it would be more correct to speak of science as comprised of bodies of knowledge - each associated with a sub-discipline or specialty, and in many cases, interrelated. That is, the body of knowledge used in one specialty may also be useful in another.

The predominant characteristic of bodies of knowledge is that they are organized and coherent, and obtained by the systematic application of methodological procedures. Here we are speaking solely of the natural and physical sciences, but it must also be pointed out that other bodies of knowledge, for example, philosophy and theology, express a similar characteristic. The natural and physical sciences are based upon the method of experiment and observation, which serves as a source of distinction between them and other less rigorously obtained bodies of knowledge.

Scientific knowledge is therefore restricted to statements about the empirical world, to the description and explanation of various aspects of nature. These statements seek to describe the characteristics of a phenomenon, and establish in systematic fashion an explanation of these

characteristics. This systematization requires the development and use of methodological procedures, which introduce an order into what would otherwise be a random conglomeration of empirical observations, in the process attaching meaning to those observations. Empirical observations must be explicitly stated, implicit assertions are insufficient if description and explanation are required. Statements of empirical observations must be subjected to a process of validation in order to be accepted as scientific knowledge. Validation procedures are determined by the scientific community. The use to which the knowledge is put, however, may be determined by extra-scientific factors.

The history of science indicates a certain degree of continuity in the development of scientific knowledge. While a body of knowledge within a specialty may vary in content over time, it is unlikely that all statements held to be true at one point will be totally displaced. Rather, certain aspects of a body of knowledge will survive through refinement that results in increased utilitarian value for the research purposes of a group of scientists.

Knowledge is the content of science, it is what scientists produce. The creation of new knowledge is not entirely dependent upon formalized procedures, but extends into various social factors. Consideration of the role of knowledge in the study of science is important to sociological analysis in order to understand the relation between the intellectual and social dimensions of science. This permits a dichotomization of the social organization of scientists and what they produce - the two aspects come to be seen as intrinsically related. The criteria of this relationship may change from one situation to another - the structural aspects of the social organization of science may change as much as knowledge content within

specialties. It is therefore fundamental that the sociology of science seek to establish the nature of these changing features.

Merton and the traditional school in the sociology of science have focused their analysis almost exclusively on the organization and behaviour of scientists, detached from what those scientists produce. For Merton, the social processes involved in the development of knowledge basically do not change from one situation to another. Knowledge is presumably defined by the method that produces it (Whitley,1972:70). Thus, sociological analysis is deemed best suited to study those processes that are not so logical or predictable, namely the organizational and behavioural aspects of science. The relation between the scientist and the product is thus taken to be an extrinsic one.

Merton's followers are even less concerned with defining knowledge. They concentrate on measuring products from the social structure of science to which some value can be attached. For example, the number of times a particular scientist is cited by other scientists is taken to be some indicator of the scientific value of that scientist's work (Merton,1973d). A further indicator is the amount and quality of scientific publications (Cole and Cole, 1973). As Whitley (1972:70) notes: "Published papers are assumed to represent items of knowledge since they have been published and presumably referees have certified the author used the scientific method." Though these types of analysis may give some idea of the output of scientific activity, they do not provide an understanding of how the content of knowledge is rooted in a social context. Other than strictly scientific factors are often associated with the value of a particular item of knowledge; for example, its utility as an economic resource.

Kuhn, on the other hand, presents a very different role for

sociological analysis. He has made a major contribution by illustrating the role which existing bodies of knowledge play in the development of science, and outlining the social mechanisms of scientific change. Kuhn's approach is explicitly concerned with the social processes through which particular traditions of knowledge are established in a scientific community - these traditions define the scientific method. He attempts to come to an understanding of this aspect of study through the notion of paradigm, thereby drawing an intrinsic connection between scientific knowledge and the social organization of science. This indicates that knowledge, that is, empirically-verified statements about some aspect of nature, has a social dimension. A factor such as this aids analysis of why certain statements of purported scientific value come to be accepted and others do not, of what instigates change in a body of knowledge utilized by a particular specialty, and so on. Kuhn's later work further clarifies the intrinsic relation between knowledge and the social organization of science. An example is the concept of paradigm in the sense of exemplar, or concrete puzzle-solutions. Exemplars represent a body of knowledge - scientists must learn the meaning of certain exemplars used in a given specialty in order to construct problems and achieve solutions. A consensus must exist within the scientific community over the use of particular exemplars, consisting of collective agreement over its value or utility, and the promise it holds for further development in the field. When the utility of an exemplar is questioned because of the emergence of an anomalous situation, a state of crisis may result, wherein the validity of the exemplar as a body of knowledge is challenged. For in the crisis situation it becomes debatable what should count as valid knowledge. Through his discussion of the structure of scientific groups and the para-

mount values which scientists share, Kuhn attempts to demonstrate how the resolution of a crisis situation is really the operation of the relation between knowledge and the social organization of science.

A synthesis of certain facets of the Mertonian and Kuhnian approaches gives us some understanding of how knowledge is validated in science, and how the content of a particular item of knowledge is based in a social context. To demonstrate this, we must ask the question: how do scientists convince other scientists of the validity of their findings? We can summarize the relevant criteria as follows:

(1) The other scientists must be convinced that no scientific rules have been broken, that the procedures employed conform to the existing tradition. Where they are convinced otherwise, as in the case of Velikovsky, they will reject the findings without submitting them to systematic forms of evaluation.

(2) The new finding must be of some value to science. It must solve some existing problem and generate new ones. In effect, the finding must be simultaneously <u>traditional</u> (that is, conform to existing patterns or current views) and <u>innovatory</u> (that is, offer new direction). The finding must fit into the current social and historical context.

(3) The other scientists must be assured of the credibility of the scientist offering the finding. This is purely a subjective determination on their part, affected by factors such as one's status in the hierarchy, personality, nationality, and so on - all of which play an important role in determining the scientific value of knowledge. Basically, these types of factors are determined just as systematically as methodological procedure in science - the scientist acquires the ability to make quick judgments about the credibility of other scientists.

(4) The finding must be concerned with an area of research that is of interest to those to whom it is offered. Barring the interdisciplinary utility of a given finding this factor almost assuredly limits the reception audience to those members of the scientific community in which one operates. This is increasingly true as knowledge becomes more and more specialized, and particular findings are generated towards a very limited audience who are qualified to judge its validity.

(5) The new finding must be original.

If it meets these criteria, the new piece of knowledge is likely to be accepted more quickly than it would otherwise. These criteria relate primarily to the internal dimensions of science, but still apply when external factors are involved if other scientists are the main audience for evaluation purposes. Most scientists would argue that only they are qualified to judge the work of their peers, and this is probably true in terms of an ideal conception of science as the unrestricted quest for new and improved forms of knowledge. However, scientists are retaining increasingly less control over science, as other institutional interests and the public-at-large are assuming greater control over the content of research activity. Therefore, scientists no longer are the exclusive audience for evaluation purposes.

Kuhn thus continues the work begun by Merton. By stressing the subjective factors involved in the development of new knowledge, Kuhn introduces a dimension of analysis that eluded the Mertonian approach, yet which provides answers to many of the questions created by the limitations of the latter's work. For example, Merton's conception of scientists following an objective procedure in the conduct of their research, seemingly removed from the encumberances which would be produced should

subjective factors assert influence over their work. The stress on the objective was idealistic, at best, and did not explain what actually went on in science. That explanation was enhanced when Kuhn removed the barrier that had been unwittingly constructed by the traditional sociology of science. Subjective factors play a major role in the development of knowledge, often determining how and what findings will be accorded legitimate scientific status. The fact that the scientist who produced the item of purported knowledge did so by the strict application of approved systematic procedures may be the least significant factor involved in the whole evaluation process. If the subjective aspect is important at the internal level of the scientific social structure, it becomes even more noticeable when the external dimension is considered, which shall be examined in detail later in the chapter.

The claim that the development of knowledge has a social dimension is not a new one, but it has nevertheless been ignored by most of the work in the sociology of science to date. Kuhn's work opens the door for new exploration in this area, which should in the process serve to instigate a more detailed understanding of the behaviour and organization of scientists.

Closely allied to a conception of science as knowledge is one of science as rules. The growth of science would be all the more difficult were there not consensus on procedure. Factors must exist which provide a semblance of structural coherence for research activity, and it is to a consideration of such that we now turn.

Science as Rules

Rules operate as a major source of normative control in science (Mulkay,1972). In this respect, rules perform a major role in determining

the social structure of science, and serve to provide regulations and guidelines for scientific research activity. We cannot define rules as explicit prescriptions (they are not written down in textbooks or journals, for example) because they are derived from the scientific process. Scientists acquire an understanding of methodological rules in much the same fashion as they do exemplars, that is, during the period of socialization into their area of specialization.

Mulkay (1972) suggests that methodological rules may be referred to as "cognitive or technical norms". This is in contrast to what may be construed as purely social norms, that is, those which regulate the behaviour of scientists but are not in any way directly attached to the substantive aspects of science, or, in other words, the content of knowledge. From a sociological perspective, the concept of norm is an empirical one. In order to identify the existence of a norm, it must be possible to demonstrate that an offence against it will bring about negative sanctions. Incidents of these may be located by examining scientific controversies, where scientists debate the validity of methodological criteria. The Velikovsky affair was one such incident, where scientists applied negative sanctions because Velikovsky violated accepted methodological rules. The response to this on the part of the scientific community was essentially one of the assertion of normative control. Velikovsky's ideas and techniques did not fit into the tradition of the day, so his work was given an outright rejection, to the point where scientists attempted to suppress the communication of his findings. These occasions of controversy result in the rules of science becoming more easily identifiable - scientists, in effect, strengthen their commitment to certain ideas and methods.

Merton's work is more concerned with social norms than those of

the cognitive or technical variety. His description of an 'ethos' is an attempt to provide an understanding of how scientists commitment to a normative framework results in the development of scientific knowledge. But as Kuhn shows, scientists must not only be committed to social norms, but also cognitive or technical norms. In all fairness to Merton, he does provide some description of the latter. Two examples we discussed in the first chapter are the requirement of developing accurate and reliable empirical evidence, and being logically consistent in the conduct of research to enhance the validity of predictions. Both of these can be considered very general methodological rules.

Kuhn is more comprehensive on the matter of cognitive norms in science, suggesting that problem-solving in normal science is affected by a network of commitments - conceptual, theoretical, instrumental, and methodological - to rules that regulate scientific activity. As we discussed in the second chapter, rules help to set problems and limit acceptable solutions, provide criteria for the use of instruments and techniques, instruct scientists on what research problems they should attempt, and define requirements on the use of research tools. Rules are for Kuhn the standarization of procedure, and serve as a major factor in the articulation process of normal science, which is oriented towards the determination of solutions to prescribed problems. During periods of normal science, the rules that govern a particular scientific community are generally agreed upon by all members of the group. Only during a crisis situation will the enforcement of rules be difficult, as their utility becomes questioned during the re-organization process a community undergoes in the transition from one paradigm to another. During these times, rules that transcend particular communities and apply to

a larger segment (for example, an entire discipline) help to provide a semblance of order in the crisis resolution.

Merton shows that rules are related to science as social activity, and Kuhn shows that they are part of the conception of science as knowledge. Rules play an integral role in the structure of scientific activity, providing criteria for what scientists should and should not do, and set up a line of demarcation to determine what scientific assertions will be declared as valid. Those who resist the normative controls of one group may attach themselves to another, or begin a new group, with its own set of rules.

As far as our specification of a program for the sociology of science is concerned, the conception of science as rules raises the need for certain types of investigation. Firstly, empirical studies of what cognitive or technical norms exist in science. This problem is compounded by the implicit nature of rules; were they explicitly stated, the task of identification would be much easier. Similarly the role of particular rules in the structure of scientific activity would be simpler to describe. As mentioned previously, perhaps the best place to begin this process of identifying rules would be through examination of scientific controversies. Secondly, empirical investigations should be placed within some theoretical framework - to provide a basis for describing the role of rules in the production and legitimation of knowledge. Thirdly, following the approach of Merton, there is a need for describing the motivation for commitment to particular cognitive and technical norms. Why are scientists committed to certain methodological rules and not others? A question such as this can be answered by considering both the subjective factors that operate in science and goals of particular groupings of

scientists. That is, the type of problems that a scientific community is attempting to solve will in large part determine the rules that are followed. Rules derive from science, and not the other way around.

There is the further question of what role social expectations play in the determination of rules for science. The discussion to this point has centered upon the internal dimensions of science, where most decisions concerning the content and operation of rules takes place. Certainly, the lay public is not qualified to impose directives on the technical aspects of scientific work. However, they are entitled to retain expectations as to the nature and results of that work. In this sense, rules may be imposed which limit certain types of research activity. The following section considers the impact of external factors upon the internal structure of science, helping to demonstrate that science is not quite as autonomous as Merton and Kuhn portray it.

The Relationship Between Science and Society

There is a fundamental inadequacy of the Mertonian and Kuhnian analyses to account for the extra-scientific influences upon the development of science. Both portray science as insulated from other institutions, in the process stressing the internal generation of research problems. Determinants of research problems may also be external to the scientific community, a factor which can be illustrated by considering the relationship between the institution of science and the wider society.

During the first few centuries of modern science[19], scientists were almost solely concerned with scientific development for the sake of

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19. The period of "modern science" is generally considered to start from the time of Galileo, for he first combined experiment with observation, stressing the relation between theory and experiment.

the extension of knowledge and were not concerned with its possible applications or consequences. This period may be referred to as one of amateur science, because those engaged in research generally did so at their own expense - often relying upon other means with which to earn a living. It was the epitome of pure research - internally generated problems were the locus of activity. The present century has experienced a complete reversal of this trend. The professionalization and bureaucratization of science have created a situation whereby extra-scientific factors determine the majority of research activity. These external factors - for example, what influences scientists to choose the types of problems they attempt to solve, or what influences other institutions to support certain types of research - are paramount in any sociological analysis of scientific activity. Explanation of these factors is essential to understand the changes that have taken place in science through the centuries, and the relationship between science and society. Society shapes science by the demands it places for the solution of problems which are central to its various institutional interests. Social control assumes two basic forms. Firstly, externally-controlled sources of funding for scientific activity - these are mainly situated within industry and government. The types of science that receive primary consideration will be those which promote the greatest benefit in terms of the goals of these particular institutions. Secondly, control over scientific research activity can emanate from the general public. This is so particularly in areas of research that produce adverse ramifications for public welfare. The combination of these two types of control places great limitations on the amount of pure research that is conducted - most research is now applied.

Scientists at present work mainly in the following institutional settings: academic, industry, and government (Hirsch,1968;Lakoff, 1977). Very few scientists are self-employed (Hirsch,1968:52). As the social organization of science became highly bureaucratic in nature, a whole series of relationships arose with other large-scale bureaucracies. Science is now a major contributor to economic growth and stability. The individual scientist does not retain much control over his work; rather, research strategy is connected to the interests of the institution for which the work is performed. The result of all this is a considerable loss of autonomy for science in comparison to past times - the collaboration with other institutional interests has placed the ideal of science for science's sake in serious jeopardy.

Most pure research takes place within the academic setting (Hirsch,1968:55). Scientists are not completely autonomous in university and college circles however, for their research activity is often dependent upon the allocation of grants (the source of which largely stems from government and industry). The award of a grant may be contingent upon the performance of a specific type of research more closely related to the interests of the funding source than those of the scientist. This situation undermines the autonomy of the scientist as a determinant of problem-choice because the objective of the selection process is institutional rather than personal. A further loss of autonomy is experienced when academic-based scientists become involved in extra-curricular activities with industry and government. The scientist in this case must hold down two jobs - one in his capacity as professor and the other in his capacity as researcher for the outside interest. Conflict can arise with the role of scientist as professor -

outside research concerns can become more important than teaching and conducting research within the confines of the academic setting.

The academic institutional setting possesses a monopoly on the training and certification of scientists. That certification is generally regarded as the awarding of the Phd degree, which can only be obtained by proceeding through the appropriate university program. Universities determine the curriculum of the training of scientists, and are duly authorized to carry it out. The curriculum will be devised in response to what are considered the most important research areas of the day, and these for the most part will fit into the category of applied science. The academic setting may possess the legitimate means of training and certifying scientists, but it is really a combination of institutional influences that determines socialization procedures.

A large number of scientists are employed within the industrial setting (Hirsch,1963:52). This situation initially developed around the turn of the century when industrial concerns realized the potential of science as a major resource for economic growth. At that time, industry began to devote laboratories to the fusion of pure and applied research. The General Electric Company provides an example (Fischer, 1971:52). Around the year 1900, it established a laboratory to conduct pure research - research which had no immediately foreseeable application. Scientists employed by the firm were at liberty to devote a portion of their time to research of their own choosing. One of the scientists, Irving Langmuir, chose to study the effects of introducing different types of gases into a high-vacuum tungsten filament lamp. The result was the incandescent light bulb, a development of consid-

erable commercial significance. Langmuir's work received recognition from the scientific community in the form of the Nobel Prize. Since that time, countless other industrial research laboratories have been created for the purposes of conducting applied research.[20]

Kornhauser (1963:25) points out that the marriage of science and industry results in the conflict of certain values and goals held by each. Industrial organizations are in the main concerned with the application of research, and have little interest in conducting research for its own sake. Whereas science favours original contributions to knowledge and strives for high-quality research, industry favours economic profits and low-cost research. The introduction of these economic factors often serves to compromise the quality of scientific work - creating conflicting responsibilities for the scientist (to science and his employer). Leaders of industrial organizations have only minimal interest in the values and goals of science, and this attitude stimulates the environment of conflict within which most scientists must work (Kornhauser,1963;Marcson,1960).

In the industrial setting, the recruitment of scientists is largely controlled by the administration of the organization rather than the scientific community (Kornhauser,1963:45). This represents a major threat to science - a basic tenet of science is that only scientists are capable of judging the qualifications of their colleagues. Similarly, only scientists are qualified to judge scientific work, a

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20. The growth of these research and development laboratories in the United States is indicative of the general trend. By 1927, there were one thousand research laboratories operating within industrial organizations, and by 1960, there were over five thousand. For further data, see OECD, 1970.

factor which has led many scientists to assume managerial responsibilities. This change in role means the scientist/manager must be more oriented towards the goals of the industrial organization rather than with those of science. Following the importance of tradition in science, scientists will tend to resist changes in their field - it often becomes the duty of the research manager to impose changes against the will of those working in the laboratory (Birr,1957). Further, industrial scientists often realize that the greatest potential for rewards in terms of income and status will mean assuming managerial responsibilities at some point in their careers - a factor where external interests override the internal goals of science (Duncan,1972).

Government is another major source of employment for scientists (Hirsch,1968:52). Scientific laboratories in government are similar to those in industry. However, governments also establish policy which links research activity to national goals. This creates national boundaries for science, which vitiates against the Mertonian and Kuhnian conception of a global scientific community. As Salomon (1977) notes, the requirements of the war effort in World War II firmly established the link between science and the state. Institutions were created to develop science policy in conjunction with research activity. The Manhattan Project and the development of atomic weaponry mark the beginning of government utilization of scientific resources. In the ensuing years scientists have come to play an increasingly important role in the government setting, both in terms of their scientific contributions and in their capacity as national policy advisors. The latter poses a problem similar to that of the

scientist/manager in industry, for there is a definite conflict between their scientific 'ideals' and the nature of their political role. Judgments of the technical requirements of their role as scientists can become influenced by the political aspect. For example, while science may ideally demand an objective and impersonal approach to research activity, in the role of policy advisor, scientists will be influenced by the political ramifications of their decisions.

Scientists working in government are faced with one problem that is less likely to affect those in the industrial setting - namely, their visibility to groups both inside and outside government (Hirsch, 1968:71-72). Thus, government research activities may be subject to greater attack from public interests - however, by the same token the scientist or research team may draw upon the resources of government to fend off such attacks. In these circumstances, they run the risk of surrounding scientific activity with political controversy, which can have an adverse effect upon the development of science.

Besides the control over science exercised by these various institutional settings, there is a more fundamental source of control stemming from the public at large. This type of social control over research activity is expressed as a general concern that science must not go unchecked, that scientists must exhibit a certain amount of social responsibility in considering the potential dangers of their scientific creations. A contemporary example of this public expression of control is directed at a group of Harvard University scientists, who ran into opposition over their efforts to establish a laboratory for the purpose of conducting special genetic research. These scientists wanted to investigate what is known as recombinant DNA experi-

mentation, a procedure which involves combining genes from one substance with genes of another - which might lead to discovering why some genes cease to function as life-sustaining agents, as in diseases such as cancer. Questions arose as to what unforseen ramifications might develop if this type of research was successfully carried out. For example, the splitting of the atom led to two possibilities: the atomic bomb, and radiation treatment for cancer. Similarly, DNA research might provide a break-through in the treatment of cancer, but could also result in the creation of totally new and uncontrollable forms of life.

This was a major concern for those opposed to the establishment of a special laboratory at Harvard for the purposes of conducting this type of genetic research - the fact that some form of life could be accidentally created which is unknown to man - a form of life that man may not have the resources to combat. These geneticists ran into further repercussions with the public when it became known they were planning to introduce a recombined gene into a bacterium called E coli. The natural home of E coli is the intestinal system of humans and animals - therefore the possibility that everyone would become infected if the geneticist's creation ever got out of the laboratory was enough to elicit strong public protest.

Not only did the Cambridge, Massachusetts City Council ensure that the geneticists not get their special laboratory, but the United States Federal Government enacted legislation that placed severe restrictions on what types of genetic research could be conducted in the future. What began as an unorganized public protest developed into a form of legal control over scientific activity.

Not only has the alliance of science with various other institutional interests resulted in the formation of new types of controls over research

activity, but also the increasing complexity of science and its pervasiveness in all facets of life have heightened public concern over its direction. The importance of science is readily acknowledged, but the public's attitude toward it has changed from one of laissez-faire (science will solve all the world's problems, so scientists should be encouraged to do as they like) to one that stresses the necessity of placing restrictions over what types of research activity should be conducted. Public expectations of science have changed - science is still regarded as the one enterprise that provides the best hope of achieving solutions to problems such as health and welfare - but it must be able to accomplish goals of this nature without at the same time contributing to the downfall of mankind. The connection between the theory of relativity and atomic weaponry has awakened the wider society to that fact.

Since the Second World War this change in the public expectation of science has become particularly manifest. Part of the reason for this is changing social needs - where economic growth was once paramount, mere survival has become important. No longer is the idealistic unrestricted growth of science accepted. Science must now be allied with useful social objectives and not just economic growth, a factor which poses ramifications for its internal structure. Now when programs for research are being designed, social objectives must be kept in mind. Failure to do so will incur the wrath of the wider society, as the geneticists at Harvard discovered.

What social responsibility do scientists have to warn of the possible dangers of their work? Traditionally, scientists have not deemed it necessary or useful to take their "intellectual discussions" to the public, but this is a reflection of their basic ideal that the development of knowledge should proceed without restriction. The very concept of social responsibil-

ity is a restriction, and increasingly demands are being placed on science to exercise concern for the public expectation. Scientists are still left with an enormous task, for ultimately they must make judgments on the utility of conducting certain types of research - a process which involves factors such as the feasibility of the work, the possible outcome, the benefit in terms of whose interest the work is being conducted, the benefit in terms of science, and so on. Along with these factors is the increased social responsibility which scientists must express for their work, something which is very often compromised by the institutional interests they represent - to say nothing of the implications which social duty has upon a conception of an objective scientific method. The interference of the wider society with the ideals of science does much to destroy the notion of those types of conceptions.

The effect of the external influences upon science has been to change the structure of its social system - science is no longer primarily governed by an internal logic. Internally formulated problems take a secondary role to those externally formulated. Taking these factors into consideration, science can now be characterized as follows:

1) Applied research is the predominant feature of scientific activity. Internally-generated problems play a less significant role in terms of scientific development.

2) As the major determinants of problem selection are external, science as an institution is less autonomous than in past historical periods.

3) Science is presently recognized as a major source of economic growth and solution to social problems (for example, health), not simply as an activity to extend knowledge.

4) This emphasis upon the instrumental value of science means that research will be of a highly planned and organized nature.

5) The group structure of science is characterized by team research, operating in the university, industry, and government institutional contexts.

6) Leadership of these groups is mainly institutional as opposed to scientific.

7) Scientific communities are now comprised of research administrators as well as practicing scientists.

8) Recruitment of scientists for industry and government is often done by non-scientists, which violates the basic ideal that only scientists are capable of evaluating scientific qualifications.

9) The scientist has a conflicting role, namely that between institutional interests and science.

10) The values of science have been modified to conform with those of other institutional interests. External factors are not explicable in terms of those conceptions which focus on the internal dimensions of science.

11) The scientific socialization process is generally conducted in the university setting - but in response to other institutional requirements.

12) Social control over science is of two types: institutional interests and public expression.

13) There are increasing public demands for scientists to exercise social responsibility in their work.

14) Research programs must reflect social objectives.

These factors indicate that the development of scientific knowledge is greatly affected by the external dimensions of science. The traditional sociology of science has paid little attention to these external factors, and has treated them more as obstacles than contributors to the development of science. If indeed the energies of science are directed more towards institutional interests than the extension of knowledge, then the sociology of science should attempt to formulate the determinants of research activity through examination of the combined internal and external dimensions. Constructed in such a manner, the sociology of science would be comprised of elements of the Mertonian and Kuhnian analyses which explain the internal structure of science, and those external aspects which, to some extent, determine that internal structure.

Concluding Remarks

The move towards a sociology of science therefore requires a synthesis of approaches - science as an internally and externally generated activity, science as knowledge, and science as rules - to provide a basis for analysis of the relationships between members of scientific communities, of the relationships between different groups, and of the relationship of science to other institutional settings. Sociological analysis cannot simply focus on one approach at the exclusion of others, because each covers one aspect of the interrelated nature of science. By combining the approaches and their various conceptions of science, a more complete understanding of science and its various facets may be attained.

Briefly, we can summarize these conceptions of science as follows:

1) Science as social activity - the basic premise of sociological analysis, concentrating upon the social mechanisms that comprise scientific activity and result in the production of knowledge. Emphasis is placed upon the internal structure of science, but could be extended to include external factors.

2) Science as knowledge - considers what is produced in science to be just as important as how it is produced. Attempts to determine how bodies of knowledge are related to the social mechanisms of scientific activity.

3) Science as rules - the scientific process is seen as creating certain procedural rules (otherwise referred to as methodological norms). Seeks to understand the relation between these, the substantive aspects related to the conception of science as knowledge, and the social organization of science.

4) Relationship between science and society - scientific activity

is conducted in various institutional settings, each of which wields considerable influence over science. It is important to determine the nature and effect of this type of influence. Addition of the external dimension should enhance sociological analysis of the internal structure of science.

The combination of these conceptions constitutes the outline of a research program for the sociology of science. Each represents a particular chapter in the history of this area of study. Merton showed the sociological importance of studying science - certainly his work stood as the major exemplar in the field for many years, as witnessed by the numerous followers who developed and refined his approach. The emphasis that Merton placed upon the analysis of the social processes which led to the development of scientific knowledge became the basis for all subsequent work in the area. To this day, the basic conception of science as social activity has not changed, although the level of conceptualization has definitely improved from the time of Merton's initial work.

In large part these improvements can be attributed to the work of Kuhn, which must be considered to have attained a similar rank to that of Merton. Kuhn's work is not explicitly sociological, and any attempts to construct is as such are mainly inferred - a task which has been amply done by his many followers. He adds the chapters on knowledge and rules to the historical development of the sociology of science. This permits the analysis of the relation between bodies of knowledge, methodological rules and social/historical conditions. In the process, the analysis of the social organization of science is enhanced. It is in connection to the latter that Kuhn's work can be seen as extending that of Merton, as well as making his own contribution by adding new conceptions of science

which illustrate the subjective character of scientific activity.

The major inadequacy of the Mertonian and Kuhnian contributions is a failure to account for the external factors that influence the operation of science. Their concern is chiefly with the internal structure of science, and how the social organization of scientists is constructed. Science by this view is postulated as a relatively autonomous, politically neutral enterprise - scientific communities being for the most part self-governing. When science is viewed within the context of the wider society and the interaction between it and other institutions is taken into account, then it becomes evident that other than strictly internal factors determine the structure of its social organization. For example, the basic unit of this social organization is the scientific community. Ideally, every community is comprised solely of and governed by scientists. However, the external demands of other institutions dictate that this conception remains an ideal, for often research administrators who do not possess scientific qualifications will have a major role in determining what research activity is conducted within the community. These administrators must translate the demands of the institutional interest they represent into some form of research strategy, which is then carried out by the scientists.

It is evident that the introduction of factors such as external influences would change certain aspects of the Mertonian and Kuhnian images of science. For example, the norms which comprise Merton's 'ethos of science' would retain limited value in describing what really goes on in science. Conditions of secrecy and competition serve to strain the norm of communism; actual scientific activity as recounted in James Watson's The Double Helix[21] suggests that scientists do attempt to derive

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21. One of the review comments on the back cover of The Double Helix

personal gain from their work. Merton's conception of a normative structure would have to be reformulated to account for external factors. This reformulation would require separating the ideals of science with the reality that research activity is related to other institutional interests. Science can no longer be viewed as autonomous, but must be analyzed in terms of its social context. A similar example can be applied to Kuhn's notion of scientific revolution. His analysis would have to be reformulated to indicate what external factors may induce a crisis situation, and how these external factors can prove decisive in a paradigm debate. This can be illustrated by examining the question of leadership in science. Institutional leadership defines priorities in terms of the requirements of a given enterprise, and takes precedence over what we may refer to as paradigmatic leadership. The distinction is that between the external and internal influences on the structure of scientific activity, and poses ramifications for what criteria are important in the change from one paradigm to another. Both internal and external factors may play a role instigating the transition and therefore, should be examined together, and not separately.

The fundamentals of the Mertonian and Kuhnian approaches would remain, for both are highly useful in their respective ways of looking at some aspect of science. Comparison of the two indicates that many common premises exist, even though each possesses a different conception of the

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states: "This is a book by a scientist about scientists - and the inside story of one of the key discoveries of the century, the structure of DNA, the heredity molecule. Even more fascinating, perhaps, it is the superbly human tale of how a very unusual 23 year-old American saw his chance for scientific immortality and promptly set out to seize it."

analysis of science. Merton is chiefly concerned with science as social activity, and Kuhn, particularly in his later work, also recognizes the importance of this approach. Kuhn's basic argument emphasizes the necessity of identifying the characteristics of scientific knowledge and methodological techniques that result in change and development in science. Combining his conception of science as knowledge and rules with the traditional approach of analyzing the behaviour and organization of scientists is the preliminary step towards a sociology of science. Addition of the important category of external influences is the next step, placing science within its institutional context. This permits study of all the facets that influence scientific development, which is not possible if one merely adopts the Mertonian or Kuhnian approaches as they stand. Combining these four conceptions of science is in a sense an aid to analysis, making it possible to develop a more complete understanding of all facets of science, and the interrelationships which define its structure.

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