TRADITION IN SCIENCE

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To

My Motherland, China

My Mother, Shulan Li

My Father, Juyi Wang

My Wife, Huizhen Ren

ABSTRACT

Tradition is usually thought to be anti-rational. Many people think it should be ruled out from scientific inquiry because they believe that science develops according to fixed rules and that this is what distinguishes science from other enterprises. On the contrary, Kuhn believes that we have too long ignored the manner in which experiences play a role in science, and he tries to rectify this error by introducing the notion of a paradigm. He applies the developmental pattern recognized in enterprises such as art and philosophy to science as well. More importantly Kuhn's *The Structure of Scientific Revolutions* got people to pay more attention to tradition in science. This thesis considers how what Kuhn says about paradigms is closely linked with the idea of science as involving tradition.

Kuhn takes paradigms to be a source of coherence in scientific traditions and the carrier of them, i.e., in normal science traditions are embodied in paradigms and hence maintained by them. I argue that when Kuhn emphasizes the priority of paradigms, he is actually emphasizing the role of tradition in normal science. In my view, to argue for the priority of paradigms is to defend the view that tradition is fundamental in science. I suggest that Kuhn's discussion about the priority of paradigms not only shows that he emphasizes the role of tradition, it also clarifies how traditions are formed and changed, specifically traditions are

embodied in paradigms. In particular it reveals how traditions are encapsulated in paradigms that include both theoretical and practical knowledge.

Kuhn's work is often taken as the basis for resisting the rational view of science. His "incommensurability thesis" and "community authority thesis" are viewed by people, such as Feyerabend, as implying science is arational or irrational. They suggest that in science there is only replacement of paradigms, not the continuation of tradition.

To better understand Kuhn's view about tradition, I argue that we need to understand his views about practice, rule, and theory. Comparing Kuhn's and Wittgenstein's views, I argue that Kuhn holds that practice and rules stand at the same level. Thus when Kuhn emphasizes the priority of paradigms, what he wants to say is that paradigms are more binding and more complete than any set of rules for research that could be unequivocally abstracted from them. I suggest that what Kuhn is attempting to do is to emphasize that people assimilate existing theories and rules through their applications rather than by learning abstract, separated theories and rules. This point implies that when scientists acquire a new theory, they learn it first in a mixture of theories, methods, and standards, namely in a paradigm; i.e., when he argues for the importance of practice, he does not want to rule out theoretical elements in scientific inquiry.

Although many of Kuhn's ideas about the genesis of paradigms are important, they have been accorded less attention than they deserve, since he couples them with his "incommensurability thesis" and "community authority thesis", both of which are questionable. I believe that it is helpful to bring out his ideas about the genesis of paradigms for our discussion of tradition. For Kuhn paradigm changes are caused by the discovery of new facts and the invention of new theories together and this takes time. I argue that this leads directly to the idea that changes of tradition have both a practical and a theoretical side and these occur in the context of the old tradition. This in turn shows that there must be a continuity of tradition. Since before a change of tradition, only those anomalies that have some connection with the existing tradition will be seriously investigated, the change that occurs will depend on them. Thus, when the old tradition gives way to the new one, many concepts and ideas of the old tradition will inevitably be retained. Moreover, during the change of tradition, the old tradition plays a role in the formation of the new one, a point that Kuhn overlooks. In addition, a change of tradition in the development of science usually appears as a change of a small part of science while the main part of it continues as before. I suggest that Kuhn misses this because he overemphasizes the incommensurability of paradigms. Worse still he ends up embracing a relativistic view about paradigm choice because he defends the "community authority thesis".

To understand how the old tradition plays a role in the formation of the new one, we need to understand tradition-following activity and rule-following activity better. To my way of thinking, these have common aspects and both can lead to new insights. Scientific practice is not something in which scientists engage without thinking.

Based on this, I examine how tradition plays a role in scientific discovery and justification. I first clarify the understanding of the distinction between the context of discovery and the context of justification. I believe that the process of discovery is a subject for philosophical analysis. What Kuhn should have said, I believe, is that just as in normal science scientists use solutions to typical problems as paradigms and solve new problems by imitating the way in which typical problems were solved, so they use paradigms of individual sciences as examples of inquiry and deal with crises in scientific revolutions, i.e., they form new paradigms by imitating previous paradigms. To illustrate this I show J. C. Maxwell did this when he established his paradigm in electromagnetic theory. I take this to be a good example of how scientists establish new paradigms.

I suggest that we should rethink the view that takes tradition and reason to be conflicting. In opposition to Feyerabend's suggestion that we think of relativism as reasonable, I argue that the process of the establishment of a new tradition is a rational process. In this connection I analyze the case of the development of quantum mechanics. Also, I attempt to show that changes of criteria are the result of reasoning, and that this does not necessarily lead to relativism.

In addition to these arguments, I argue against Kuhn's "community authority thesis". I point out that for Kuhn community authority does not come from or represent tradition, rather, it seems to be independent of traditional authority, even to be definitive of tradition. I suggest that when a community decides on a new paradigm the scientists are guided by the existing tradition and hence that community authority depends on tradition. In support of this view I consider how scientists in a community align themselves with the existing tradition and I explore in detail Kuhn's view about how people acquire and store knowledge. Finally, I argue that scientists accept the views they do because of objective factors, not subjective ones.

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1

Introduction

Scientific inquiry is always conducted in the context of a body of existing knowledge. Scientists never begin from scratch. Both experimental practice and theoretical conceptions are embodied in scientific traditions. Furthermore, when people learn the theories that form the background of their investigations, they inevitably begin by applying them in the way the previous generation applies them. This suggests that scientists are always immersed in traditions, and that it is important for philosophers of science to consider carefully the nature and role of tradition.

Tradition in science has been discussed in the philosophical literature, but only poorly. This is partly because tradition is thought to be opposed to reason, and partly because philosophers like Thomas Kuhn and Paul Feyerabend have provided accounts of scientific tradition which make science seem relativistic or irrational. In addition, for rationalists, tradition has no place in science.

There is thus, I think, a need to re-examine the issue of tradition in science along with related issues such as those of the nature of practice and of authority. In particular, we need to consider the role of tradition during periods of scientific change and the ways in which traditions are handed down. I shall argue that this in turn requires us to explore the nature of rule-following in the context of scientific discovery and theory evaluation. Finally, we need to review the claim that an emphasis on tradition leads to relativism and irrationalism.

There is much literature in this area, but most of it deals only indirectly with the issues to be discussed in this thesis. For my purposes the works of P. Feyerabend, T. Kuhn, M. Polanyi and L. Wittgenstein are particularly important. More specifically, my study builds upon Feyerabend's views concerning the incommensurability of theories and relativism, on Kuhn's conception of how scientists learn by examples, on Polanyi's views about tacit knowledge, and on Wittgenstein's discussion of rule-following. In my opinion, we can obtain a better understanding of tradition in science by bringing these thinkers' ideas together, since each provides us with important information about how knowledge is communicated, about the way in which existing knowledge plays a role in scientific inquiry, about the sense in which scientists follow rules, and about how scientists learn theories. I believe that the views of these authors are partly right, partly wrong, and I shall attempt to provide more adequate answers to the questions they address.

In chapter 2, I outline the views about origins of science and tradition in science. I argue that tradition in science has a much longer history than science itself. It is a part of a longer common cultural tradition and affects the form and style of the knowledge acquired by scientists that follow it. However, different philosophers hold different views on the role that tradition plays, especially regarding the role in scientific revolution. In chapter 3, I deal with what Kuhn says about tradition in science. I shall be especially concerned with how Kuhn describes normal science and scientific revolutions using both the notion of paradigm and that of tradition. I attempt to show that when Kuhn emphasizes the importance of paradigms, he takes tradition to be embodied in paradigms and intends to emphasize the importance of tradition in science.

It is true that since Kuhn's *The Structure of Scientific*

Revolutions[1] was published, people have paid more attention to scientific practice. But it is also true that he was interpreted as identifying tradition with practice and as giving practice pride of place. In chapter 4, I will discuss how we should understand Kuhn's view regarding the priority of paradigms. I first argue that both Wittgenstein and Kuhn hold that both theoretical and practical knowledge play a role in rule-following. In opposition to the common view that Kuhn's conception of tradition is a kind of practice, I argue that it is better understood in terms of the persistence of existing theories and the manner in which the previous generation conducted its inquiries. Otherwise, we miss his crucial point that in learning and using theories, scientists assimilate theories and their applications together.

In chapter 5, I consider whether there is continuity of tradition through scientific revolutions by examining how paradigms come into being. I show that Kuhn's ideas about the source of paradigms are important for discussion of tradition, but since he over emphasizes "incommensurability of paradigms", these ideas have not been accorded the attention they deserve. I shall attempt to isolate the important ideas in Kuhn's discussion of revolution and show that these ideas require there to be a continuity of tradition. Here I also discuss Kuhn's incommensurability of paradigms and Feyerabend's thesis

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[1] T. Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, Chicago, 1962.

regarding the incommensurability of theories.

To understand the role that tradition plays in the development of science, we need to have a clearer understanding of rule-following. This is because traditions--although not reducible to sets of rules--include many requirements regarding what scientists should and should not do. I believe that in submitting themselves to a tradition scientists become obliged to follow principles that are much looser than a set of rules. Accordingly in chapter 6, I attempt to show that even a set of rules cannot entirely determine rule-following activity, and that scientists can obtain different results while following the same rules. Since rule-following is not a mechanical activity, it can also lead to novelty. If I am right, tradition in science can, contrary to what is standardly believed, play a role in scientific revolutions and lead to novelty.

To discuss the role that tradition plays in scientific revolutions, we need to discuss the role of tradition in scientific discovery and justification. I address this topic in chapter 7. I first discuss the distinction between the context of discovery and the context of justification. I contend that Kuhn actually rules out the investigation of the process of discovery. Through an examination of Kuhn's view of education, I argue that Kuhn overlooks scientists' acquired ability to see the similarity between paradigms. I contend that scientists are able to proceed on the basis of models of existing knowledge in revolutionary science and in the way they proceed on the basis of paradigms in normal science. In this sense, tradition plays a role during the shift from one kind of normal science to another, no less than in normal science itself.

In chapter 8, I discuss various issues concerning tradition and reasoning, and I try to show that tradition and rationality are not contradictory. I show that people are so deeply influenced by a tradition of rationality that no one can judge anything without it. First I argue that Feyerabend uses the same general procedure as the rationalist, and shifts his standpoint, when he argues against rationalism. Then I try to show that there is something beyond individual theories which provides a base for reasoning in scientific revolutions, by showing what scientists did in the development of quantum mechanics.

In chapter 9, I question Kuhn's community authority thesis. I argue that tradition plays a more important role than Kuhn supposes. I first clarify Kuhn's thesis by distinguishing between community authority and the authority of tradition. Then I argue that tradition deeply influences scientists, and because of this they do not have much freedom to choose but must follow specific ideas to reach satisfactory conclusions. I point out that subsequent to *The Structure of Scientific Revolution*, Kuhn reasserts the community authority thesis even though he emphasizes subjective factors. I maintain, on the contrary that objective factors are crucial in the development of science. Finally, in chapter 10 I summarize the main conclusions of the thesis.

2

Views About Tradition in Science

In recent years and especially as a result of Kuhn's work, more and more philosophers have come to believe that it is wrong to oppose science and tradition. In this chapter I outline how some philosophers think of tradition in science, and I attempt to clarify the concept of tradition in science and the role it has played in the development of science.

What is meant by tradition in science? In general, tradition refers to something, such as social precepts, ideas, values, rules of conduct, etc., that are historically shaped and handed down from generation to generation.[2] It has two main characteristics: it is formed beforehand by previous generation and it has continuity. In particular, tradition in science may be understood as the continuity of knowledge and methods. In fact, as I will show later, styles of knowledge are often identified with traditions. This fact alone indicates that tradition is very important for science.

1. The Origins of Science

First of all, how far can we go back in history when we track scientific traditions? Before Copernicus, science was not regarded as a special sort of knowledge and we cannot say that there was a distinct scientific tradition. At very beginning, however, science developed from the knowledge people acquired earlier, i.e., it was a result of the development of knowledge and carried with it many earlier achievements. In this sense there was continuity, a tradition in the development of science. It is for this reason that some people trace science back to the fourteenth century and, consequently, that the scientific developments of the seventeenth century were much less of

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[2] Cf. *Dictionary of Philosophy*, ed. by Murad Saifulin and the late Richard R. Dixon, Progress Publishers, Moscow, 2nd rev. ed, 1984. p. 427.

revolutionary achievements.[3]

Some believe that the origin of science can be traced even further back to the beginning of civilization. For example, S.F. Mason in his book on the origin of science says:

> The roots of science, however, ran deep, stretching back to the period before the appearance of civilization. No matter how far back in history we go there were always some techniques, facts, and conceptions, known to craftsmen or scholars, which were scientific in character, though before modern times such knowledge in general was subordinate to the requirements of either the philosophical or the craft tradition.[4]

Some people put this idea another way. They believe that science has its roots in common tradition, that it is a part of this tradition, and should be understood as emerging from it. For example, J.D. Bernal says:

> Mankind has had at all times a "Great Tradition", comprising the basis for what at different times has been deemed to be true belief and right action, ever since it can be recognized as emerging from the dim past of prehistory, is essentially one, though we can discern partly independent branches in the Mediterranean countries, in India, and in China. The growth and change of this great tradition cannot be understood without science, but equally, science cannot be understood unless it is seen as a natural part of the common tradition.[5]

This suggests that tradition in science can be found in the earliest common tradition. For Bernal tradition in science is

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[3] Cf. H. Kearney, *Origins Of The Scientific Revolution,* Hugh F. Cox & Wyman Ltd, London,1966, p. xiii.

[4] S.F. Mason, *Main Currents Of Scientific Thought, A History of The Sciences*, Henry Schuman, New York, 1953, p. 1.

[5] J.D. Bernal, *Science In History*, The M.I.T. Press, Cambridge, Massachusetts. 1965, p. 57.

a part of "an unbroken chain of tradition".[6]

However, Bernal also thinks that the tradition of science can be distinguished from others. He points out that there is one aspect that distinguishes scientific and technical advance from all other aspects of social achievement, namely, that science is cumulative. He maintains:

> It is this cumulative nature that marks off science from the other great human institutions, such as those of religion, law, philosophy, and art.[7]

He defends this by listing the differences between the tradition of the sciences and those of religion or arts. He believes that "the tradition of science ... is one which can be directly checked by reference to verifiable and repeatable observations in the material word".[8]

It is necessary to note that by the "cumulative tradition of science", Bernal does not mean the cumulative development of science, i.e., he does not hold the questionable view that the development of science adds new knowledge without changing existing knowledge. On the contrary, he points out that the scientist, as an individual, is always deliberately striving to change accepted theory and that the edifice of science, as a whole, is permanently "under repair". Thus in speaking of the cumulative tradition of science Bernal is attempting to say that the continuity of tradition is more important and distinct in

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[6] *Ibid.*, p. 61.

[7] *Ibid.*, p. 43.

[8] *Ibid.*, pp. 43-44.

science than in other aspects of human activity.

This suggests that we should think of tradition in science as having a much longer history than modern i.e. 17th century science has, and we should understand tradition in science as part of a longer common cultural tradition. This will help us when we deal with Thomas Kuhn's community authority thesis; for tradition in science influenced scientists from the very beginning.

2. Tradition in Science

Let us examine what people have in mind when they talk about tradition in science. For some people scientific tradition are characterized by a specific view of nature, specifically with the way in which people attempt to know nature. They believe that a tradition determines the nature of the knowledge that people acquire. For example, Hugh Kearney in his book *Science And Change 1500--1700* says that during this period, there were at least three approaches to nature which may be broadly termed "scientific": organic, magical and mechanistic.[9] He distinguishes these by reference to their language and terminology. According to him, each view of nature has its dominant analogies:

> Within the organic tradition the scientist explained the natural world in terms of analogies drawn from what we now call biology. The language which he used originated in observation of growth and decay, with

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[9] H. Kearney, *Science And Change 1500--1700*, McGraw-Hill Book Company, New York, 1971.

> the analogy of the acorn growing into the oak always ready to hand.
>
> A second tradition, the magical, provided a scientific framework in which the world of nature was seen as a work of art. (I use the word "magical" in preference to "aesthetic" because it suggests the overtones of mystery which I think were involved.) The appropriate analogies and the language of the scientist derived from a view of nature in which beauty, contrivance, surprise and mystery were seen as its dominant characteristics.
>
> The third tradition rested upon a view of nature in which the dominant analogy was the machine. What struck scientists who worked within this framework was the regularity, permanence and predictability of the universe.[10]

It is obvious that for Kearney tradition refers both to kinds of knowledge and to model of nature through which people acquire and express their beliefs about nature.

Again some people talk of tradition in terms of method and style of thought. For instance, Stephen F. Mason mentions that science has two original traditions: the technical tradition and the spiritual tradition. He points out that in the history of the development of science, these two traditions were separate for a long time but that both figured in the history of science. He says:

> Science had its historical roots in two primary sources. Firstly, *the technical tradition*, in which practical experiences and skills were handed on and developed from one generation to another: and secondly, *the spiritual tradition*, in which human aspirations and ideas were passed on and augmented. Such traditions existed before civilization appeared. ... In the bronze age civilizations, the two traditions appear to have been largely separate, perpetuated on the one hand by craftsmen, and on the other by corporations of priestly scribes, though the latter had some important utilitarian techniques of

[10] *Ibid.*, pp. 23-24.

their own.

> In the subsequent civilizations, the two traditions remained separate for the most part, ... There were occasional rapprochements, notably those in ancient Greece, but, in general, it was not until the late middle ages and early modern times that *elements from the two traditions began to converge, and then combine, producing a new tradition, that of science.*[11]

On this views tradition plays a significant role in the development of science. Not only does it provide the background for knowledge and method, it also determines the form in which knowledge is stored. In particular, technical and spiritual traditions appeared in different ways in different areas, and thus affected achievements in those areas differently.

An example is the technical tradition in ancient China. During the Spring and Autumn Period and Qin (770-207 B.C.) the ancient Chinese produced many technical achievements, including a primitive magnetic compass, paper-making, and some knowledge of mechanics and optics.[12] All these achievements have an obvious technological aspect. They seem to be instances of disconnected knowledge rather than elements of a system of the sort characteristic of modern science. Even in mathematics, which is usually believed to be a field of theoretical knowledge, the ancient Chinese put forward many methods to solve different questions, but they did not develop a theoretical

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[11] Mason, *op. cit.*, p. 1. Mason holds that both technical and spiritual traditions existed before.

[12] Cf. *Ancient China's Technology And Science*, Compiled by the Institute of the History of Natural Sciences, Chinese Academy of Sciences, Foreign Languages Press, Beijing, 1983, p. 152, 176, 124.

system. For example, the oldest mathematics book, *Zhow Bi Suan Jing* (not later than 100 B.C.) describes the right triangle (in ancient Chinese called "gougu") as used in primitive astronomical calculation for measuring height and distance by proportion. In another important book *Jiu Zhang Suan Shu (Nine Chapters on the Mathematical Art)* the author gives several rules for answering different kinds of questions, and he provides many examples. It was an advance in mathematical knowledge at the time, but it was obviously experimental in character.

In ancient Greece in sharp contrast to that in ancient China, Greek philosophers did establish a system of theoretical knowledge. From Thales' theory to the work of Anaximander, to Pythagoras' principle of the universe, to Aristotle's physics we can see an attempt to formulate a theory of the world. The influence of the spiritual tradition made their works more theoretical and today when we track some ideas in modern science, we often go back to these Greek philosophers. These and similar examples indicate that a tradition may deeply affect the corresponding form of knowledge.

After the emergence of modern science, science produced results which influenced the tradition and had an effect upon the view of nature and the method of cognition. Thus people started to talk about tradition in science in a more particular way and they started to connect the scientific tradition with the success of scientific works or theories.

Scientists often claim that today's science is the result of a scientific tradition that started with Copernicus. For example, Werner Heisenberg says:

> As we celebrate the five hundredth birthday of Copernicus, we believe that our present science is connected with his work; that the direction which he had chosen for his research in astronomy still determines to some extent the scientific work of our time. We are convinced that our present problems, our methods, our scientific concepts are, at least partly, the results of a scientific tradition which accompanies or leads the way of science through the centuries.[13]

In fact, successful scientific works not only established new knowledge, they also influenced people to follow a new way of studying nature. For example, after Newton's theory was established, for over two hundred years many scientists tried to follow the ideas of atomism and mechanism implicit in Newton's theory. In the development of electricity, the study of the nature of gas, optics, chemistry and even modern atomic theory, the influence of Newton's theory is obvious. Albert Einstein characterized this as the view that every physical event is a movement of particles in space determined by unchanging laws. Using it, people attempted to explain all things in the world, until Maxwell's theory was established.[14]

3. Recent Philosophical Views About Tradition

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[13] W. Heisenberg, *Encounterers with Einstein*, Princeton University Press, Princeton, New Jersey, 1989, p. 1.

[14] Cf. A. Einstein, "Maxwell's Influence on the Development of the Conception of Physical Reality", in *James Clerk Maxwell: A Commemoration Volume*, Cambridge University Press, 1931, pp. 66-73.

Contemporary philosophers recognize that traditions play a role in science as well. They understand tradition in science in different ways. For example, Larry Laudan takes research traditions to include "more global theories". In particular, he speaks of himself as "outlining a new account of the *more global theories (which I shall be calling research traditions)*".[15] Moreover, when he explains his view, he takes "research traditions" to be traditions in the general sense as well as traditions within a specific domain of science. For example, he gives common traits of research traditions as having "a number of specific theories", exhibiting "certain *metaphysical* and *methodological* commitments", going through "a number of different, detailed (and often mutually contradictory) formulations and generally having a long history extending through a significant period of time".[16]

For other philosophers, "tradition" refers to a style of cognitive activity. For example, Thomas Kuhn distinguishes two traditions in science. One is *a mathematical tradition*. He says that all five classical physical sciences (astronomy, statics, optics, mathematics and harmonics) "from some significant points of view, ... might better be described as a single field, mathematics", because "their considerable ancient development required little refined observation and even less

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[15] L. Laudan, *Progress And Problem*, University of California Press, Los Angeles, 1977, p. 72.

[16] Cf. *ibid.*, pp. 78-79.

experiment".[17] Kuhn believes that this shows that classical sciences were the products of a mathematical tradition. In addition, Kuhn says that there is another tradition, *an experimental tradition*. He believes that this tradition has a long history and can even be found in the works of Aristotle. Kuhn says:

> Aristotle's methodological writings contain many passages which are just as insistent upon the need for close observation as the writings of Francis Bacon. Randall and Crombie have isolated and studied an important medieval methodological tradition which from the thirteenth century into the early seventeenth, elaborated rules for drawing sound conclusions from observation and experiment.[18]

It is obvious that Kuhn is talking of tradition here in much the same way as Stephen F. Mason does. Kuhn's view also shows that the two original traditions of science, pointed out by Mason, are sometimes individually seen as a single cognitive style in science.

Feyerabend similarly talks of different traditions, distinguishing between theoretical traditions and empirical traditions. He says:

> I shall call the knowledge desired by the early philosophers theoretical knowledge and traditions embodying theoretical knowledge *theoretical traditions*. The traditions to be superseded I shall call *empirical or historical traditions*. The members of theoretical traditions identify knowledge with universality, regard theories as the true bearers of information and try to reason in a standardised or "logical" way. They want to bring knowledge under the

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[17] Cf. T. Kuhn, *The Essential Tension*, The University of Chicago Press, Chicago and London, 1977, p. 37.

[18] *Ibid.*, pp. 41-42.

> rule of universal laws. Theories, according to them, identify what is permanent in the flux of history and thereby make it unhistorical. They introduce genuine, i.e., non-historical knowledge. The members of historical traditions emphasis what is particular (this includes particular regularities such as Kepler's laws). They rely on lists, stories, and asides, reason by example, analogy, and free association and use "logical" rules when it suits their purpose. They also emphasize the plurality and, via the plurality, the history-dependence of logical standard.[19]

These quotations from Kuhn and Feyerabend show that they think of a tradition as, in part, a cognitive style.

From all of the above, we see that although different scholars look at tradition in science in different ways, they seem to agree that there are traditions which are formed before science and that they deeply influence the emergence and development of science. However, on the role that tradition within science plays, different philosophers hold different views, for example, about the continuity of traditions and about whether tradition plays a role in scientific change. Both Kuhn and Feyerabend hold a view that I think is questionable. For our discussion about tradition, the continuity of knowledge, the method embodied in tradition, and the role of tradition in scientific revolutions need to be emphasised. To do so, I will first discuss Kuhn's view of tradition in detail in the next chapter; I shall question his and Feyerabend's views later on, especially in chapters 7 and 8.

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[19] P. Feyerabend, *Farewell to Reason*, Verso, New York, 1987, p. 118.

3

Kuhn on Tradition

In this chapter, I consider Kuhn's views about tradition and his conception of a paradigm, a notion with which his notion of tradition is closely associated. Indeed Kuhn invariably has the notion of tradition in mind when he talks about the role of paradigms. We can see this both in his discussion of normal science and in his discussion of scientific revolutions. In these discussions, "paradigm" and "tradition" are virtually interchangeable.

1. Tradition and Paradigm at Normal Science

Focusing on normal science, we see that Kuhn uses "paradigm" and "tradition" in the same way to illustrate his view. He defines normal science by appealing to the notion of a paradigm, characterizing normal research as paradigm-based research and as "a highly cumulative enterprise".[20]

In fact, Kuhn says that in paradigm-based research scientists directly imitate examples of successful practice. For him normal science maintains itself through paradigms handed down by examples, which is also an important characteristic of traditions. Moreover, it should not be forgotten that the idea of an enterprise developing cumulatively is closely associated with the idea of an enterprise that is governed by a tradition. After the notion of paradigm was criticized by many philosophers, Kuhn says in the **"postscript"** of *The Structure of Scientific Revolutions* that in his book

> the term "paradigm" is used in two different senses. On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community. On the other, 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.[21]

It is clear that in both senses the term "paradigm" stands for the elements, such as beliefs, values, techniques, and examples,

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[20] T. Kuhn, *The Structure of Scientific Revolutions*, University of Chicago Press, Chicago, second edition, 1970, p. 52.

[21] *Ibid.*, p. 175.

that are usually linked with tradition.[22]

Besides, Kuhn also directly uses the word "tradition" when discussing normal science. He says normal research is "tradition bound work"[23] in which "the scientist requires a thoroughgoing commitment to the tradition. ... In part this commitment is demanded by the nature of the problems the scientist normally undertakes".[24] Here, we can see Kuhn using both "tradition bound research" and "guided by paradigm" to define normal science; for him research guided by a paradigm is just "tradition bound research".

Let us now consider what Kuhn means by "tradition bound research" or "paradigm guided research". For Kuhn traditions, as well as paradigms, limit the problems that scientists undertake. When Kuhn explains this point, he uses the term "tradition". He says that

> the research scientist is not an innovator but a solver of puzzles, and the puzzles upon which he concentrates are just those which he believes can be

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[22] In "Second Thoughts on Paradigms" (in *The Essential Tension*) Kuhn also says that the term "paradigm" occurs in two main senses, namely as a disciplinary matrix and as an exemplar. He describes the former as "embracing all the shared commitments of a scientific group (p. 294)" and as "include[ing] most or all of the objects of group commitment described in the book as paradigms, parts of paradigms, or paradigmatic (p. 297)"; he describes the latter as "concrete problem solutions, accepted by the group as, in a quite usual sense, paradigmatic. (p. 298)". In my view what Kuhn says about paradigms in his subsequent writings supports my contention that Kuhn takes them to embody traditions, his later view being that they include whatever members of particular scientific groups share.

[23] Kuhn, *The Essential Tension*, p. 234.

[24] *Ibid.*, p. 232.

> both stated and solved within the existing scientific tradition.[25]

This shows that Kuhn takes an existing scientific tradition to play an important role in determining which problems should be chosen and how they should be solved. But he also talks of choosing problems in terms of paradigms. For instance, he says that

> [One] of the things a scientific community acquires with a paradigm is a criterion for choosing problems that, while the paradigm is taken for granted, can be assumed to have solutions. To a great extent these are the only problems that the community will admit as scientific or encourage its members to undertake.[26]

This shows, again, that Kuhn describes the role of traditions and the role of paradigms in normal science in the same way with regard to the issue of which problems scientists work on. In Kuhn's mind the success of research scientists in normal science strengthens the authority of the tradition.

That Kuhn identifies tradition and paradigm is also reflected in his view that the acquisition of a paradigm is a sign of maturity in the development of any given scientific field, since it enables scientists to investigate some part of nature at a level that would otherwise be unimaginable. Similarly, he holds that tradition is very important for scientists, and that this is a good thing. He says:

> In the sciences ... it is often better to do one's best with the tools at hand than to pause for

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[25] *Ibid.*, p. 234.

[26] Kuhn, *The Structure of Scientific Revolutions*, p. 37.

> contemplation of divergent approaches.[27]

Here it is obvious that the "tools at hand" are handed down from the last generation, i.e., they are part of a tradition. In particular, Kuhn emphasizes that the best way to start a scientific inquiry is to follow a tradition. Moreover, it is important to remember that for Kuhn the existing "tools" are not abstract rules, but "accepted concrete examples of scientific achievement, actual problem solutions", or "exemplars". For him "to follow a tradition", "to do best with the tools at hand", is to imitate the paradigm.

2. Traditions and Paradigms in Scientific Revolutions

In cases of the scientific revolutions we also find Kuhn using both "paradigm" and "tradition" in the same way. For example, he defines a scientific revolution as a change of paradigms. He says:

> Scientific revolutions are here taken to be those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one.[28]

Furthermore he also asks:

> What (in a scientific revolution) causes the group to abandon one tradition of normal research in favour of another?[29]

More specifically, for Kuhn in a scientific revolution a change

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[27] Kuhn, *The Essential Tension*, p. 224.

[28] Kuhn, *The Structure of Scientific Revolutions*, p. 92.

[29] *Ibid.*, p. 144.

of tradition occurs.

This point of view reappears in his comparison between science and art. Thus he says that while controversy in art ends with the acceptance of a new tradition, it does not mean the end of the old tradition. In science, by contrast, acceptance of a new tradition means the end of the old one. As he puts it,

> The success of one artistic tradition does not render another wrong or mistaken, art can support, far more readily than science, a number of simultaneous incompatible traditions or schools.[30]

This shows that, for Kuhn a change of tradition, appearing as a change of paradigm, is involved in scientific revolutions.

Moreover, when Kuhn discusses how scientific revolutions happen, he uses both paradigm change and change of tradition to describe it, his view being that people rooted in paradigms and traditions are likely to break out of them and initiate new ones. For instance, when he uses "paradigm" to talk about scientific revolutions, he says that "research under a paradigm must be a particularly effective way of inducing paradigm change".[31] Likewise, he uses "tradition" to illustrate this issue, his view being that "only investigations firmly rooted in the contemporary scientific tradition are likely to break that tradition and give rise to a new one".[32] Thus, for Kuhn paradigm

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[30] Kuhn, *The Essential Tension*, p. 348.

[31] Kuhn, *The Structure of Scientific Revolutions*, p. 52.

[32] Kuhn, *The Essential Tension*, p. 227.

change is change of tradition, it occurs because people firmly following the old ones adopt new ones.

So far I have compared Kuhn's use of "paradigm" and "tradition" in his discussion of science (and art). Although "paradigm" is Kuhn's preferred term, he uses both terms in his discussion the same way. I shall show that this is not carelessness but rather intentional and natural. However before doing so, I shall elaborate this point by discussing Kuhn's comments about the connection between the two concepts. This shows why he treats them as interchangeable.

3. Connection Between Traditions and Paradigms

Turning to Kuhn's point of view about the connection between traditions and paradigms, we need to note firstly that although Kuhn uses the two concept as interchangeable, in his mind tradition is more general than paradigms, while the latter appears within the individual science. Thus he holds that paradigms are the source of traditions. For example, he notes that he introduces "shared paradigms rather than shared rules, assumptions, and points of view as the source of coherence for normal research traditions".[33] For Kuhn, then, a particular scientific tradition starts as a paradigm and when a scientific achievement is accepted by scientists, they will imitate this achievement to solve problems. In a word, the way in which this achievement is appropriated becomes a new tradition.

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[33] Kuhn, *The Structure of Scientific Revolutions*, p. 42.

Secondly, we need to note that for Kuhn paradigms encapsulate scientific achievements. They are widely emulated, and thus give rise to research traditions. He says:

> Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, i.e., *for the genesis and continuation of a particular research tradition.*[34]

In fact, Margaret Masterman, in her article "The Nature of A Paradigm",[35] points out that one of Kuhn's uses of paradigm involves regarding it "as a whole tradition".[36]

What I have said so far can be summarized as the following. For Kuhn traditions are embodied in paradigms and hence maintained by them. Moreover, traditions guide research in normal science through paradigms, and change in scientific revolutions.

4. The Role of Tradition

I now turn to discuss Kuhn's view about how traditions play a role in science. First, by examining a comment Kuhn makes about his own work, I suggest that he aimed to emphasize tradition when he wrote *The Structure of Scientific Revolutions* and that he took this to be one of his more important

[34] *Ibid.*, p. 11, emphasis added.

[35] Margaret Masterman, "The Nature of a Paradigm", in *Criticism and the Growth of Knowledge*, Edited by Imre Lakatos and Alan Musgrave, Cambridge University Press, Cambridge, 1970, PP. 59-89.

[36] *Ibid.*, p. 62.

contributions. For example, when he talks about science and art, he says:

> I have been concerned to emphasize the similarity of the revolutionary lines of the two disciplines. In both the historian can discover periods during which practice conforms to a ***tradition*** based upon one or another stable constellation of values, techniques, and models. In both he is also able to isolate periods of relatively rapid change in which one ***tradition*** and one set of values and models gives way to another. That much, however, can probably be said about the development of any human enterprise. With respect to gross developmental pattern *my originality, if any, was only the insistence that what has long been recognized about the development of, say, the arts or philosophy applies to science as well.*[37]

This indicates that Kuhn sees his contribution as partly, if not mainly, one of acknowledging the role of tradition in scientific development, and that he holds that tradition plays a role in science similar to the role it plays in other human enterprises.

Moreover, I would argue that Kuhn did what he intended to do, namely to show that tradition plays a major role in science. When we look at what Kuhn says in *The Structure of Scientific Revolutions* and *The Essential Tension*, we find him frequently emphasizing the role of tradition in science. But since, as I pointed out above, Kuhn takes the notion of a tradition to be very closely linked with that of a paradigm, he actually emphasizes the role of tradition in science more often by emphasizing the role of paradigms. The reason that Kuhn does this is that he takes traditions to play a role in science as paradigms. In what follows, I begin with his arguments

[37] Kuhn, *The Essential Tension*, p. 348, emphasis added.

concerning the role of traditions in science, then I discuss his emphasis on paradigms. I believe that these not only reveal more about the relationship between the concepts of paradigm and tradition, they also illuminate what he wants to say about the forming and changing of traditions.

First, I would like to draw attention to the fact that Kuhn's general claims about his own position show that he emphasizes the importance of adhering to traditions in science. For example, when he compares his view with the view of Karl Popper, he says: "We both insist that adherence to a tradition has an essential role in scientific development".[38]

Second, Kuhn actually emphasizes that traditions play a role in normal science. For instance, he holds that no part of science has progressed very far or very rapidly before an unequivocal tradition is formed[39] and he insists that "the scientist requires a thoroughgoing commitment to the tradition".[40] For him this is because the tradition is demanded by the nature of the problems the scientist normally undertakes; without faith in a tradition the scientist's work would be wasteful of time and talent; and without a firm commitment to the contemporary scientific tradition the recognition and evaluation of anomaly would be impossible.[41]

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[38] *Ibid.*, p. 267.

[39] Cf. *ibid.*, p. 237.

[40] *Ibid.*, p. 235.

[41] See *ibid.*, pp. 235-236.

Third, Kuhn emphasizes that traditions play a role in scientific revolutions, his view being that "the productive scientist must be a traditionalist".[42] In particular, he holds that only investigations firmly rooted in the contemporary scientific tradition are likely to break a tradition and give rise to a new one.[43] In his view, therefore, tradition plays a role in scientific revolutions only in so far as it concentrates attention on the trouble or causes of crisis that will lead to scientific revolutions. Once it is clear what the trouble is or what the causes of crises are, the old tradition cannot but give way to the new one. (I question this view about the role of tradition in a period of scientific revolution in chapter 7.)

I have argued that Kuhn explicitly emphasizes the role of tradition when discussing scientific revolutions. Next I turn to Kuhn's discussion of this issue in terms of paradigms. Kuhn uses "paradigm" to emphasize the role of tradition and to explain the way in which traditions actually function. In fact, the above-mentioned points regarding the role of tradition in normal science and scientific revolutions are concentrated in

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[42] *Ibid.*, p. 237.

[43] Thus he says that "at least for the scientific community as a whole, work within a well-defined and deeply ingrained *tradition* seems more productive of tradition-shattering novelties than work in which no similarly convergent standards are involved. How can this be so? I think it is because no other sort of work is nearly so well suited to isolate for continuing and concentrated attention those loci of trouble or causes of crisis upon whose recognition the most fundamental advances in basic science depend." *Ibid.*, p. 234, emphasis added.

the two themes in his discussion, namely those of the "priority of paradigms" and the "incommensurability of the pre- and postrevolutionary normal-scientific traditions".[44] I believe that the first theme brings out well the way in which a tradition plays a role in normal science, while the second one involves a mistaken view about the incommensurability of successive theories. The issues are different and if we are to understand Kuhn's view about tradition, we must examine both in detail. I will discuss incommensurability in chapter 5 and focus here on Kuhn's view regarding the priority of paradigms.

5. Tradition and Priority of Paradigms

I now want to suggest that Kuhn's remarks about the priority of paradigms can be best understood as stressing the role of tradition. As I pointed out above, Kuhn allies paradigms with traditions and takes scientists to adhere to a tradition by imitating paradigms. In addition, Kuhn claims that

> We have too long ignored the manner in which knowledge of nature can be tacitly embodied in whole experiences without intervening abstraction of criteria or generalizations. Those experiences are presented to us during education and professional initiation by a generation that already knows what they are exemplars of. By assimilating a sufficient number of exemplars, we learn to recognize and work with the world our teachers already know.[45]

In this quotation "whole experiences" refers to paradigms.

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[44] Kuhn, *The Structure of Scientific Revolutions*, p. 148.

[45] T. Kuhn, "Reflections on my Critics", in *Criticism and the Growth of Knowledge* Ed. by Imre Lakatos and Alan Musgrave, Cambridge University Press, Cambridge, 1970, p. 275.

This, in Kuhn's view, is passed on as traditions, which makes the new generation work with the same "world" that their teachers work with. Keeping these in mind, I propose to investigate Kuhn's discussion about the priority of paradigms. This investigation will help to clarify his main point that tradition plays an important role in science and throw light on his views about how traditions function.

"The priority of paradigms" is the title of chapter V of *The Structure of Scientific Revolutions.* In this very important chapter, Kuhn argues that paradigms have a status prior to that of shared rules. When Kuhn does this, what he refers to as "rules" are very widened: from those that are at a level lower or more concrete than that of laws and theories to the higher level, quasi-metaphysical commitments.[46] He argues that

> paradigms may be prior to, more binding, and more complete than any set of rules for research that could be unequivocally abstracted from them.[47]

He gives four reasons for this, each of which can be understood as stemming from his view regarding the importance of tradition in science.

Let me start with Kuhn's main reason. This reason--which is actually his second reason--concerns the nature of education. Kuhn takes scientific development as a chain of traditions and

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[46] He also says the same thing somewhere else: "shared examples have essential cognitive functions prior to a specification of criteria with respect to which they are exemplary." *The Essential Tension,* p. 313.

[47] Kuhn, *The Structure of Scientific Revolutions,* p. 46.

emphasizes that all scientists require considerable education before they can do scientific research. He says that "scientists work from models acquired through education and through subsequent exposure to the literature".[48] This suggests that he takes all scientific inquiries to happen historically in the chain of the development of science, and that he regards education as giving rise to the continuity of tradition. Even those scientists who break the tradition have to learn the tradition that happens to be in place when they learn their trade and they too must work from the models that they acquire. Moreover, Kuhn holds that the nature of education is responsible for the priority of paradigms. In education, scientists

> never learn concepts, laws, and theories in the abstract and by themselves. Instead, these intellectual tools are from the start encountered in a historically and pedagogically prior unit that displays them with and through their applications. A new theory is always announced together with applications to some concrete range of natural phenomena; without them it would not be even a candidate for acceptance.[49]

In other words, scientists grasp the existing intellectual tools by observing applications of these tools to problem-solutions and making their own applications of them. They accept the way in which the last generation conducted scientific inquiry "often without quite knowing or needing to know what characteristics have given these models the status of community paradigms".[50]

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[48] *Ibid.*, p. 46.

[49] *Ibid.*, p. 46.

[50] *Ibid.*, p. 46.

Thus since it is obvious that in education, neophyte scientists align themselves with the community that already uses the paradigms, we can see that Kuhn holds that in accepting existing knowledge such scientists actually accept a tradition.

To put the point more generally, what Kuhn is saying is that the imparting of knowledge is not just a matter of passing on abstract theories, but rather one of inculcating how this knowledge may be used. While it may be helpful to demonstrate this knowledge by deducing it from other abstract theories, such knowledge cannot be learned apart from its application. Here, in saying that paradigms have priority, Kuhn is saying that in education theories are passed on with the help of a paradigm, i.e., scientists acquire a knowledge of theories by examples and practice. And this, of course, is also the way in which tradition is passed on, namely by education. Moreover, notice that Kuhn holds that people assimilate existing theories and rules in their applications, in a mixture of theories, methods, and standards rather than by learning abstract, separated theories, or rules. This point implies that when scientists acquire a new theory, they learn it first in a mixture, namely in a paradigm.[51] However, since this relates to another of Kuhn's reasons for thinking paradigms have priority, I will reserve discussion of it until later.

Let me now deal with Kuhn's third reason, namely that when

[51] Also compare *ibid.*, p. 109: "In learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture."

scientists are engaged in their research, paradigms guide research by direct modelling as well as through abstracted rules. I believe it indicates a further way in which tradition plays a role in science. As Kuhn puts it, "normal science can be determined in part by the direct inspection of paradigms";[52] for "while the paradigm is taken for granted," the "scientific community acquires ... a criterion for choosing problems that ... can be assumed to have solutions".[53] In Kuhn's mind, then, normal science is established once the scientific community accepts an example; in normal science, scientists do not have to know the relevant rules, since they will only choose the problems which are similar to the example and solve them by imitating the example. Thus Kuhn insists that only the change of paradigm can provide an alternative solution, for it provides another way to choose the problems and another way to solve them.[54]

Kuhn also puts this idea in the term of tradition:

> If we can accept a considerably broadened use of the term "rule"[55]--one that will occasionally equate it with "*established viewpoint*" or with "*preconception*" --then the problems accessible within a given research *tradition* display something much like this set of

[52] *Ibid.*, p. 44.

[53] *Ibid.*, p. 37.

[54] Cf. *ibid.*, p. 40 Kuhn says this in the discussion about the application of Newton's laws. There, the "rules of the game" should be read as paradigms.

[55] Again, here "rule" should be read as "paradigm".

puzzle characteristics.[56]

The terms "established viewpoint" and "preconception" refer once again to how traditions are handed down. Kuhn here equates paradigms with previously established research traditions. Because a paradigm is actually a solution of a problem which is selected by the research tradition, it determines the particular kind of puzzles that are pursued and the particular sorts of solutions which are allowed. In this way it determines normal science. Further, when scientists accept a paradigm, they accept a tradition, for they adhere to the tradition by choosing the same kind of puzzles and they attempt to solve them in the way the paradigm problems have been solved.

We can now also better understand Kuhn's first reason for saying that paradigms have priority, which is that in scientific inquiry it is much easier for scientists to acquire a paradigm than to acquire a set of rules. I suggest that this actually relates to the crucial question of how tradition plays a role during a change of a tradition. Kuhn rightly points out that this point is a corollary of the second one (the one I have just discussed). Since scientists learn and use the existing intellectual tools as a paradigm, when they deal with new problems, it is much easier for them to recognize those examples that can become paradigms than to find the new theories contained in them.

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[56] Kuhn, *The structure of Scientific Revolution*, pp. 38-39, emphasis added.

Establishing new paradigms may include development of new theories, but at the beginning scientists may not have clearly recognized new theories, notwithstanding the fact that they recognize that the new paradigm is successful in problem-solving. Scientists first reach a commitment for a new paradigm; then they sort out new theories. Thus Kuhn says that paradigms are the vehicle for theories.[57] For Kuhn a new paradigm is approached not by a theoretical search, but by finding out the resemblances among problems and among ways in which scientists solve them, which is to say that they do so by forming a tradition.

Kuhn's fourth reason for granting paradigms a status prior to that of shared rules is that "substituting paradigms for rules should make the diversity of scientific fields and specialties easier to understand".[58] By this Kuhn means that a theory can be employed by different paradigms for different scientific fields. But since a theory is comprised of paradigms, however paradigms appear as exemplars of problem-solutions, they must include something different in different fields. When such problem-solutions serve as paradigms, they become the basis of different traditions and can be recognized more easily than theories by different groups within the scientific community.

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[57] See *ibid.*, p. 109, where Kuhn refers to "the paradigm's role as a vehicle for scientific theory".

[58] *Ibid.*, p. 49.

As an example, Kuhn says, quantum theory meant different things for scientists in different fields. In other words, quantum theory can become components of different traditions. While some fields may overlap, traditions usually will not be coextensive, and "a revolution produced in one of these *traditions* will not necessarily extend to the others as well".[59] In arguing this point Kuhn actually talks about the relationship between theories and paradigms and by implication the relationship between theories and traditions. According to him, one theory can be embodied in different traditions and those traditions can be more easily identified than theories for each individual science.

The discussion I have presented of Kuhn's four reasons for according priority to paradigms thus leads us to the conclusion that when he emphasizes the priority of paradigms, he is actually emphasizing the role of tradition in normal science.

Let me summarize how I understand Kuhn's point of view. I read Kuhn's argument about the priority of paradigms as an argument about the priority of traditions. There are three main points that I would like to stress. (1) Both the formation and handing down of traditions are multifaceted; they involve theories, methods and standards along with their applications; they involve "whole experiences". This means that tradition includes both theoretical and practical knowledge. (2) Education gives rise to the continuity of tradition. And (3) a

[59] *Ibid.*, p. 50, emphasis added.

single theory can apply across different traditions.

I have argued that Kuhn takes traditions to be embodied in paradigms. I have also shown that Kuhn emphasizes that traditions play an essential role in science. Finally, I have drawn attention to Kuhn's discussion of the priority of paradigms. I believe that his discussion not only shows that he emphasizes the role of tradition, it also clarifies how traditions are perpetuated. In fact, I believe Kuhn's discussion includes many ideas that are useful for our discussion, and will attempt to develop them in the chapters that follow.

4

Practice, Theory and Tradition

In the last chapter, we saw that Kuhn emphasizes that scientists adhere to a tradition when engaged in normal science. Before looking into the role that traditions play in scientific revolutions, I want to consider the philosophical foundation of traditions. This involves discussing the relationship between knowing and doing (and correlatively the relationship between theory and practice). There are several reasons to do this.

First, most discussions dealing with tradition concern the relationship between practice and rules. For example, when Michael Polanyi talks about tradition, he talks about learning by example, and in connection with this he examines the relationship between practice and rules. He says: "By watching the master and emulating his efforts in the presence of his example, the apprentice unconsciously picks up the *rules* of the art".[60]

Secondly, it is necessary to discuss the relationship between practice and tradition because some philosophers, including Peter Barker, understand scientific practice (and other practices) as something prior to scientific theorizing. In my opinion, however, scientific practice involves many theoretical elements.

Barker notes a strong similarity between Kuhn's view that scientist adhere to a tradition in normal science by imitating its paradigms and Wittgenstein's claim that people learn rules through examples and practice, that rule-following is a kind of practice.

In this chapter, I discuss the relation between practices and rules by examining Kuhn's view of practice, paradigm and theory. I also discuss Wittgenstein's views about this relationship. I shall suggest that in the handing down of traditions, both practice and theoretical knowledge play an important role. First, however, I would like to clarify the

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[60] M. Polany, *Personal Knowledge*, p. 53. Also note that when Wittgenstein talks about rule-following, he speaks of "pre-knowledge" (which refers to custom).

notions of theory and practice.

Theories are general systems of scientific knowledge. Philosophers often take theories to be axiomatized sets of propositions that permit strict logical and mathematical manipulation. However, this conception of theory seems to be unnecessarily strict since it would exclude most of the sets of propositions which have historically been *thought* of as theories and have been used as such by scientists. For the purposes of the present study, I propose instead to adopt a conception of theory that captures the *systematic* aspect of what are generally taken to be theories while allowing that theoretical reasoning need not be formalizable. Let us say that a theory is a set of propositions which, together with specifications of initial conditions, jointly imply a range of characteristics of events falling within a given domain, where both the domain and the parameters of measurement for events within the domain are themselves specified by propositions in the theory. (This is intended as a partial *explication*, rather than a proper definition, of the idea of a theory.) Then we may say that *theoretical reasoning* is any reasoning in which an appeal to a theory or theoretical proposition is made on *the basis* of propositions that are part of the theory in question.

The concept of "practice", as it has been used by philosophers, is even more nebulous than the concept of "theory". I shall understand by "practice" those aspects of science that require skills and judgements going beyond or separate from the understanding of particular theories. These include skill at experimental manipulation and design, engineering lore, grasp of laboratory and research community politics, and so on. Practical scientific capacities, it will be assumed, are developed primarily on the basis of practical *experience* rather than on the basis of formal pedagogy.

Rules are usually expressed as instructions about how something should be done. In scientific research, there are different sorts of rules. They give directions to activities in that they express what is permissible or what is metaphysically or methodologically right.

1. Barker's Argument

I intend to show that Barker thinks that for Wittgenstein and Kuhn rules and practice stand at different levels, and that practice stands behind rules. He supports his view by reconstructing Wittgenstein's section 198 in *The Philosophical Investigations* as an infinite regress argument.[61] In his way to

[61] Cf. L. Wittgenstein, *The Philosophical Investigations*, Basil Blackwell, Oxford, 1958, Section 198:

> "But how can a rule show me what I have to do at *this* point? Whatever I do is, on some interpretation, in accord with the rule."--That is not what we ought to say, but rather; any interpretation still hangs in the air along with what it interprets, and cannot give it any support. Interpretation by themselves do not determine meaning.
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> "Then can whatever I do be brought into accord with the rule?"--Let me ask this: What has the expression of a rule--say a sign-post--got to do with my actions? What sort of connexion is there here?--Well, perhaps this one: I have been trained to react to this sign in a particular way, and now I do so

reconstruct Wittgenstein's argument Barker seems to take interpretation as another rule to judge the activity of rule-following. He claims that no system of rules could exhaustively govern a language. If a language is a system governed by rules, he argues, for every case of using the language, there must be a rule which sanctions it. But how do we know that we have invoked the correct rule, and that the rule was cited correctly? Presumably, we must use another rule to judge whether we are using the rule correctly or incorrectly, and so on, i.e., for every usage of one rule we need another rule to justify and explain it. This leads to an infinite regress. Barker holds that the only way to avoid this regress is to say that there must be something behind rules which is not a rule; i.e. that explains and legitimates our applications without appeal to further rule. Baker views 'practice' as fulfilling this role. He concludes: "practices stand behind rules rather than at the same level, practices fix the application of rules."[62] By this, Barker is saying that practices are prior to rules and determine the application of rules. He is suggesting that "a key feature

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> react to it.
>
> But that is only to give a causal connexion; to tell how it has come about that we now go by the sign-post; not what this going-by-the-sign really consists in. On the contrary; I have further indicated that a person goes by a sign-post only in so far as there exists a regular use of sign-posts, a custom.

[62] P. Barker,"Wittgenstein And The Authority Of Science" in W. Leinfellner and F.N. Wuketits (eds.), *The Tasks Of Contemporary Philosophy,* Holder-Pichler-Tempsky, Vienna, 1986, p. 165.

of the customs or practices ... is that they are the things we do without thinking". For him we simply proceed as we have been educated to proceed.[63]

Barker rightly points out that Wittgenstein's "examples" and Kuhn's "exemplars" play a similar function in the education of scientist. Furthermore he is right to suggest that what Kuhn says about science is just a particular case of what Wittgenstein says about rule-following in general. However, Barker holds that for Wittgenstein practice stands behind the rules scientists follow, and since he draws an analogy between Kuhn and Wittgenstein, it is obvious that he understands Kuhn to hold that practice is prior to rules as well. Accordingly, he maintains, Kuhn's scientific "exemplars" may be seen as a special case of Wittgenstein's nonscientific "examples",[64] his view being that "both [exemplars and examples] function as a *sui generis* mode of initiating new practitioners or explaining things to mature practitioners."[65] I believe that this interpretation bears directly on our discussion of tradition, because to understand tradition we need to understand the relation between practice and theories and the relation between practice and rules. In my opinion, however, both Kuhn and

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[63] *Ibid.*

[64] Wittgenstein holds that teaching a person how to follow a rule is achieved by giving examples to him and having him use it. Thus for Wittgenstein, "example" is an activity showing the process of rule-following and its result.

[65] Barker, *op. cit.*, p. 166.

Wittgenstein hold that practice and rules stand at the same level. By this I mean that scientific practice is not prior to rules and theories as Barker suggests. Instead, scientific practice always involves rules and theories; there is no scientific practice without rules and theories. To demonstrate this point, I would like to introduce the notion of theory into my discussion since rules and theories are closely connected and because in Kuhn's discussion when he talks about rules, he invariably has theories in mind. Otherwise put, I want to show that both Kuhn and Wittgenstein hold that scientific practice and theory go hand in hand, that neither of them can occur without other.

2. An Error in Barker's Understanding of Wittgenstein

I now want to argue that Barker has misunderstood Wittgenstein's point. According to Wittgenstein, practices and rules stand at the same level.[66] For Wittgenstein rule-following is not an action done by following an interpretation. His point is that it does not make sense to take rule-following as interpretation-following. Barker, by contrast, in the rule-following argument views an interpretation as something standing behind rules, and it is this that allows him to reconstruct

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[66] Cf. Andrew Lugg "Wittgenstein, Science And The Authority Of The Community", in *Philosophy Of Law, Politics, And Society*. Holder-pichler-Tempsky, Wien, 1988, pp. 376-378. Lugg argues that Wittgenstein holds that rules and practices stand at the same level and that we should picture scientists as following rules when engaged in practice and as engaging in practice when following rules.

Wittgenstein's section 198 in *The Philosophical Investigations* as an infinite regress argument. However, Wittgenstein holds that we should not view an interpretation as something standing behind what it interprets. Rather we should think of it as another rule standing at the same level as the rule it interprets, i.e., as another expression or formulation of the rule it interprets.[67] So for wittgenstein the question of "interpretation-following" is the same as the question of "rule-following".[68]

This is why Wittgenstein says that "any interpretation still hangs in the air along with what it interprets, and cannot give it any support".[69] He suggests that people do something by a rule only in so far as there exists a regular use of the rule, i.e., a custom.[70]

Now Barker takes this latter point to mean that for Wittgenstein practices stand behind rules. However, on my view, Wittgenstein is saying that rules and the way in which we follow

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[67] Cf. Wittgenstein, *Philosophical Investigations*, Section 201. "... we ought to restrict the term 'interpretation' to the substitution of one expression of the rule for another."

[68] Cf. Edward H. Minar, *Philosophical Investigations Section 185-202 Wittgenstein's Treatment of Following a Rule*, Garland Publishing, New York and London, 1990, p. 238. Minar puts the point as follows: "Wittgenstein, I take it, is trying to show that our grasp of the notion of accord with an interpretation relies on no less than our grasp of what it is to follow rules."

[69] Wittgenstein, *Philosophical Investigations*, Section 198.

[70] Cf. *Ibid.*, section 198: "a person goes by a sign-post only in so far as there exists a regular use of sign-posts, a custom".

them are established at the same time and neither of them has priority. When scientists learn how to follow rules, they gain the knowledge of the rules and acquire the techniques needed to follow them together. In other words, for Wittgenstein in so far as there is a rule, there is a use of the rule. The way to react to a rule is determined along with the rule itself. In fact, Wittgenstein rules out the existence of the rule standing behind the rule that we are following.

Elsewhere, Wittgenstein puts this point more succinctly. He says: "It is not possible to penetrate behind the rules, because there is no behind."[71] I take this to mean that a rule and regular use of the rule go together. We cannot say that practices are prior to and therefore explanatory of rules, because we can only say that we know a rule--say a signpost--if we know how to follow it (signpost). In other words, it does not make sense to say that practice stands behind the rules that are being followed.

Someone may argue that it might make sense to say that one may know how to do something without knowing its connection with a rule and hence that it makes sense to say practice is prior to rules. However, when we consider the relation between action and rules, we have to consider whether the action is appropriate to the rule that we are to follow, and we now face a no less serious difficulty than when we think that there is a rule

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[71] L. Wittgenstein, *Philosophical Grammar*, Basil Blackwell, Oxford, p. 244.

first, then consider how it determines our action. Certainly before a sign-post is posted, people may happen to go along the direction which the sign-post will later indicate. But this is not the practice that we are talking about here. Also if we take practice to be prior to rule, then after a rule is formed, how can we know the practice is appropriate to the rule, e.g. after the sign-post is in place, how can we say an action is appropriate to this sign-post? Answering this is as difficult as answering the question of how a rule determines an action in accordance with it. Therefore, contrary to Barker, I believe we should understand Wittgenstein as saying that rules and practice cannot be separated from each other, not that practice stands behind rules.

Wittgenstein's point is also illustrated in his discussion about how one learns a rule. He believes that to understand a rule means to master the technique required to follow it. Wittgenstein says: "To understand a sentence means to understand a language. To understand a language means to be master of a technique."[72] By this, I take him to be saying what learning a rule involves. For Wittgenstein, to "understand a language" means to "understand a rule"; to "understand a sentence" means to "understand the use of a rule on many occasions"; and "to be master of a technique" means "to be master of the technique

[72] Wittgenstein, *Philosophical Investigations*, Section 199.

required to follow the rule".[73]

If I am right, for Wittgenstein, to understand a rule means to master the technique required to follow it. From this, it follows that we cannot say that someone has understood a rule until he is able to follow it, because, as Wittgenstein says, "obeying a rule is a practice."[74] In such a practice, one's understanding of a rule and one's mastering of a technique are manifest together. When people learn a rule, nobody first understands a rule, then follows it; nor can one first master a technique then understand the rule.[75] In fact, one masters a technique of following a given rule by the same process required to understand its meaning. This is why Wittgenstein says that there is a way of grasping a rule which is not an interpretation, but which is exhibited in what we call "obeying the rule and going against it in actual cases."[76]

Finally I should mention in passing that Wittgenstein's point that rules are exhibited in practice is important for our discussion of the relation between practice and tradition. In

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[73] Cf. *Ibid.*, In section 199 Wittgenstein talks about language right after the sentence, "To obey a rule... is a custom". In my way of reading him, rather than using language as a example, he concludes that learning a rule means to be master of a technique for following it.

[74] *Ibid.*, Section 202.

[75] Gerd Brand puts Wittgenstein's point as follows: "We do not first learn a rule which we then apply; it is given to us in the actual applying." *The Essential Wittgenstein*, translated and with an introduction by Robert E. Inis, Basic Books, Inc., Publisher, 1979, p. 127.

[76] Wittgenstein, *Philosophical Investigations*, Section 201.

fact, the nature of going by a tradition is the same as that of going by a rule. Sometimes, people even talk about following a tradition by talking about following rules. I discuss this issue in detail in chapter 6.

3. Different Ways of Understanding Paradigms

Now I will examine Barker's understanding of Kuhn. People understand Kuhn's notion of paradigm in different ways and Barker is not alone in thinking that Kuhn holds that practices stand behind rules. For many readers, this is what Kuhn means when he speaks of the priority of paradigms; for them Kuhn believes that paradigms somehow constitute practices.

This interpretation of Kuhn is opposed to that of Sheldon S. Wolin and others, who take a paradigm to be a general theory. As Wolin puts it, "the particular theory which dominates a scientific community is designated a paradigm".[77] This difference suggests that we should look more carefully at what Kuhn means by "paradigm". As I have shown in chapter 3, Kuhn's notion of a paradigm is closely linked with the notion of tradition. Hence we must discuss the connection of both paradigms and traditions to practice and theory. In what follows, I attempt to show that for Kuhn, theoretical knowledge also plays a very important role in paradigms. Also, I attempt to clarify how theories are encapsulated in traditions. By

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[77] See S. Wolin, "Paradigms And Political Theories", in *Politics And Experience*, edited by Preston King and B. C. Parekh, Cambridge, Cambridge University Press, 1968, p. 132.

doing this, I aim to show that Kuhn, like Wittgenstein, holds that practice and rules stand at the same level. I shall conclude that this is the main parallel between Kuhn and Wittgenstein.

Kuhn attempts to understand the details in the process of scientific inquiry in its initial stages and focuses on shared paradigms, rather than on shared rules, when discussing scientific inquiry. In order to understand Kuhn's view, I will discuss both the relationship between paradigms and the practice of science and the relationship between paradigms and rules.

4. Paradigm and Practice

I believe that Kuhn is best read as holding that practice and paradigms stand at the same level and that whenever scientists engage in scientific practice they have a paradigm in mind; they are guided by the paradigm. The main reason for thinking this is that Kuhn holds that paradigms appear at the beginning of the practice of scientific inquiry. For Kuhn paradigms play a role even in the earliest periods of scientific inquiry. Kuhn's view can easily be found in the sixth chapter of *The Structure of Scientific Revolutions*. In this chapter Kuhn discusses anomalies and the emergence of scientific discoveries which prompt new scientific inquiry. He holds that discovery is a process which involves a change in paradigm. First he points out that the view which takes discoveries or novelties of fact to precede inventions or novelties of theory is wrong. For him, the "distinction between discovery and invention or between fact and theory will, however, immediately prove to be exceedingly artificial ... they are not isolated events but extended episodes with a regularly recurrent structure".[78] Then, he argues that the structure, or process by which a new fact becomes a scientific fact is the process of adjustment of paradigm.[79] So, for Kuhn a new paradigm appears at the same time as discovery of a real scientific fact, i.e., paradigms appear with the beginning of scientific practice.

Furthermore we should pay attention to what Kuhn says in the article "Second Thoughts on Paradigms". In this article, Kuhn acknowledges that he was wrong to think that there is a pre-paradigm period in which there is no paradigm yet. He says that "the most damaging of these consequences grows out of my use of the term 'paradigm' when distinguishing an earlier from a later period in the development of an individual science".[80] He now holds that "whatever paradigms may be, they are possessed by any scientific community, including the schools of the so-called pre-paradigm period".[81] So Kuhn believes that whenever scientists engage in scientific practice they are guided by a paradigm.

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[78] Kuhn, *The Structure of Scientific Revolutions*, p. 52.

[79] *Ibid.*, pp. 52-53

[80] Kuhn, *The Essential Tension*, p. 295

[81] *Ibid.*.

5. Paradigm and Theoretical Knowledge

First, I want to show that Kuhn takes the notion of a paradigm to be connected both with the notion of practice and that of a theoretical achievement. Otherwise put, Kuhn holds that scientific practices involve theories. For him the theoretical content and theoretical achievement of a successful practice are embodied in a paradigm.

Kuhn clearly claims that a paradigm includes both theoretical and practical knowledge. For example, in *The Structure of Scientific Revolutions*, he says:

> By choosing it (paradigm), I mean to suggest that some accepted examples of actual scientific practice--*examples* which **include** *law, theory, application, and instrumentation together*--provide models from which spring particular coherent traditions of scientific research.[82]

In addition, considering Kuhn's discussion of the application of law and theory and instrumentation, we can say that theories are important ingredients of paradigms. Take application first: Kuhn believes that the application of theory is very closely related to theory. When he discusses the priority of paradigms, he not only emphasizes that law and theory are from the start encountered in a historically and pedagogically prior unit that displays them with and through their applications, he also insists that "a new theory is always

[82] Kuhn, *The Structure of Scientific Revolutions*, p. 10, emphasis added. See also p. 42, where Kuhn says: "the existence of this strong network of commitments--conceptual, theoretical, instrumental, and methodological--is a principal source of the metaphor that relates normal science to puzzle-solving."

announced together with application to some concrete range of natural phenomena".[83] Kuhn holds that theories would not even be candidates for acceptance without applications.

Also when Kuhn says that applications of theories are included in paradigms, he is certainly talking about the applications of theories included in paradigms. The practical knowledge involved in the applications of a theory goes with the knowledge of it.

Finally, when he discusses the process by which paradigms come into being, Kuhn again emphasizes that theories play an important role. He holds that when a theory matches the experiment, *"it (i.e., the theory) becomes a paradigm"*.[84] For instance, when he talks about the revolution in chemistry, he suggests that the "[oxygen *theory* of combustion] was the keystone for a reformulation of chemistry so vast that it is usually called the chemical revolution".[85]

Kuhn's discussion of instrumentation also shows that theories incorporated in paradigms are very important for instrumentation. Kuhn suggests that instrumentation depends upon theories. For him, to recognize the reading of an instrument within the framework of a paradigm is to recognize the theories that this instrument depends upon. For instance when he talks about measuring optical wavelengths Kuhn says:

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[83] See *ibid.*, p. 46.

[84] See *ibid.*, p. 61.

[85] *Ibid.*, p. 56.

> The man who builds an instrument to determine optical wave lengths ... must show, by analyzing his apparatus in terms of the *established body of optical theory*, that the numbers his instrument produces are the ones that enter theory as wave lengths. If some residual vagueness in the theory or some unanalyzed component of his apparatus prevents his completing that demonstration, his colleagues may well conclude that he has measured nothing at all.[86]

So, the design and use of the instrument, also essentially embodies a theory. Once one uses an instrument or accepts the reading of an instrument, one actually accepts the theory embodied in the instrument. Therefore, instrumentation cannot be separated from theory; the instrumentation appropriate to a paradigm depends on the theories that are incorporated in the paradigm.

To summarize, I have pointed out that for Kuhn, law, theory, application, and instrumentation are important elements of paradigms. Also, I have shown that for Kuhn, application and instrumentation cannot be independent of theories.[87]

In fact, Kuhn directly says that it is theories that make paradigms constitutive of research activity. He says: "Through

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[86] *Ibid.*, p. 39.

[87] In Kuhn's later works, he revises his notion of paradigm. In the "Postscript-1969", when he talks about what scientists of one community share, Kuhn says: "Scientists themselves would say they share a theory or set of theories, and I shall be glad if the term can ultimately be recaptured for this use. As currently used in philosophy of science, however, 'theory' connotes a structure far more limited in nature and scope than the one required here." *Ibid.*, p. 182. Kuhn intents to say that "theory" is not an appropriate notion for discussing of what scientists of one community share, but he does not deny that the shared examples include theoretical knowledge. In my later discussion I will return to this point.

the theories they embody, paradigms prove to be constitutive of the research activity."[88] From this, we can say that when Kuhn argues that "examples of successful practice ... were ... paradigms",[89] he is talking about both practical and theoretical knowledge embedded in paradigms.

This point can also be demonstrated in another way. I want to point out that for Kuhn, paradigms serve two kinds of functions, the investigation of which, I believe, is helpful for understanding his conception of tradition. Kuhn says:

> By shifting emphasis from the ***cognitive*** to the ***normative*** functions of paradigms, the preceding examples enlarge our understanding of the ways in which paradigms give form to the scientific life.[90]

Here Kuhn points out that paradigms have both a "cognitive" function and a "normative" function.

In saying that paradigms have a "cognitive" function, Kuhn is tacitly acknowledging that paradigms and theories go hand in hand. For Kuhn when paradigms serve a cognitive function, they function as a source of new theories. Kuhn says that at that time, paradigms play the "*role of a vehicle for scientific theory*",[91] i.e., he holds that theories are determined with paradigms. As Kuhn says,

> In [its cognitive] role [a paradigm] functions by

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[88] *Ibid.*, p. 109.

[89] Kuhn, *The Essential Tension*, p. 318.

[90] Kuhn, *The Structure of Scientific Revolutions*, p. 109, emphasis added.

[91] *Ibid.*, p. 109, emphasis added.

> telling the scientist about the entities that nature does and does not contain and about the ways in which those entities behave.[92]

In my opinion, for Kuhn the relationship between paradigms and theories is simply that theories are embodied in paradigms. However, scientists first see the paradigm.

Kuhn gives us an example to show the relationship between a theory and a paradigm. He points out that in the eighteenth century, scientists failed to derive the observed motion of the moon from Newton's laws of motion and gravitation. He says:

> [Some scientists] suggested replacing the inverse square law with a law that deviated from it at small distances. To do that, however, would have been to change the paradigm, to define a new puzzle, and not to solve the old one. In the event, scientists preserved the rules until, in 1750, one of them discovered how they could successfully be applied. Only a change in the rules of the game could have provided an alternative [theory].[93]

It is clear that for Kuhn, theories are strictly determined with paradigms and that they change as paradigms evolve. Thus since paradigms are the source of tradition, we can say that theories are encapsulated in traditions.

Kuhn also discusses the relation between a theory and a paradigm in another context. When he discusses discoveries that seem to be theory-induced and which have emerged with new theories and new paradigms, he says:

> Not all theories are paradigm theories ... scientists usually develop many speculative and unarticulated

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[92] *Ibid.*.

[93] *Ibid.*, pp. 39-40. Here the "rules of the game" refers to the paradigm.

> theories that can themselves point the way to discovery. Often, however, that discovery is not quite the one anticipated by the speculative and tentative hypothesis. Only as experiment and tentative theory are together articulated to a match does the discovery emerge and the theory become a paradigm.[94]

Here, Kuhn explicitly speaks of theories as paradigm theories, and maintains that some discoveries are theoretical developments. Moreover, he explicitly states that when a tentative theory matches an experiment, it becomes a paradigm. He gives an example, the discovery of the Leyden jar, to support his point.[95] This suggests that for Kuhn, some paradigms are recognized as establishing new theories.

I have shown that when Kuhn talks about the "cognitive" functions of paradigms he is thinking of the link between theories and paradigms. For him, they go together. The cognitive functions of paradigms can help scientists to form new theories, in some case, they come into being together.

6. Paradigms and Rules

Let us now turn our attention to the "normative" function of paradigms. When Kuhn talks about the function of a paradigm, he implies that the contents of what are commonly called rules are incorporated in paradigms when they are formed. He suggests that when paradigms serve a normative function, they give

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[94] *Ibid.*, p. 61.

[95] See *ibid.*, pp. 61-62.

scientists some of the directions for map-making.[96] According to Kuhn, a paradigm serves the "normative" function of showing how the map can be made more detailed and clear. In particular, Kuhn says that "paradigms guide research by direct modelling as well as through abstracted rules."[97]

At this point, we must ask how paradigms guide research (or, how paradigms serve a normative function). I believe that a clear understanding of Kuhn's point can help us understand the relation between rules and paradigms. When Kuhn explains how paradigms serve a normative function, i.e., how paradigms give scientists "some of the directions essential for map-making", he is saying that it is in learning a paradigm that the scientist acquires theories, methods, and standards.[98] In addition, when Kuhn discusses the relation between paradigms and rules, he says:

> Turn now to another, more difficult, and more revealing aspect of the parallelism between puzzles and the problems of normal science. If it is to classify as a puzzle, a problem must be characterized by more than an assured solution. There must also be *rules* that limit both the nature of acceptable solutions and the steps by which they are to be obtained.[99]

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[96] *Ibid.*, p. 109. Kuhn says: paradigms "are also, however, constitutive of science in other respects, and that is now the point. In particular, our most recent examples show that paradigms provide scientists not only with a map but also with some of the directions essential for map-making."

[97] *Ibid.*, p. 42.

[98] See *ibid.*, p. 109

[99] See *ibid.*, p. 38.

In brief, the main rules that Kuhn thinks are needed, are rules to select the problem, and to limit the nature of solutions and the steps towards them.

The following example, provided by Kuhn, shows that when a paradigm serves a normative function it functions in the same way that rules function to select a problem, to limit the nature of solutions and to indicate where a solution should lie. Kuhn believes that during the eighteenth and nineteenth centuries, Newton's Laws[100] "performed those functions", namely helping to "set puzzles and to limit acceptable solutions".[101] Kuhn also points out that the content of rules changes with the paradigm.

> Therefore, when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions.[102]

There is thus no doubt that criteria determining the legitimacy

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[100] In the context of Kuhn's discussion, "Newton's Laws" should be read as "Newton's tradition" or "Newton's paradigm".

[101] *Kuhn, The Structure of Scientific Revolutions*, p. 38. See also p. 40: "The study of normal-scientific traditions discloses many additional rules, and these provide much information about the commitments that scientists derive from their paradigms. What can we say are the main categories into which these rules fall? The most obvious and probably the most binding is exemplified by the sorts of generalizations we have just noted. These are explicit statements of scientific law and about scientific concepts and theories. While they continue to be honoured, such statements help to set puzzles and to limit acceptable solutions. Newton's Laws, for example, performed those functions during the eighteenth and nineteenth centuries. As long as they did so, quantity-of-matter was a fundamental ontological category for physical scientists, and the forces that act between bits of matter were a dominant topic for research."

[102] *Ibid.*, p. 109.

both of problems and of proposed solutions are an essential element of the rules scientists follow. Kuhn takes the content of rules that scientists follow in the course of a normal scientific investigation to be in a paradigm already. He is right to hold that rules have not, at that time, been abstracted from them and to suggest that "rules ... derive from paradigms".[103] In this sense the content of rules is determined at the same time as the paradigms with which they are associated.

Let me summarize what I have done. First, I demonstrated that for Kuhn, practice and paradigms stand at the same level; then I showed that he takes paradigms and rules to stand at the same level. Therefore, we may conclude that for Kuhn, practice and rules stand at the same level.

7. The Priority of Paradigms

If Kuhn takes practice and rules to stand at the same level, how should we understand his view that paradigms have priority? In order to answer this question, we must obtain a clearer understanding of Kuhn's view about rules in scientific practice.

First, Kuhn does not deny that there are shared rules. He says: "there obviously are rules to which all the practitioners

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[103] *Ibid.*, p. 42. As I read this it doesn't mean that paradigms stand behind rules. It shows that the content of rules is already in the paradigms. I will come back this point later.

of a scientific specialty adhere at a given time."[104] And he concedes that

> nothing in the preceding discussion proves that there is no set of correspondence rules adequate to explain the behaviour of the community under study. A negative of that sort scarcely can be proven.[105]

He also says that he does not himself "doubt that the examination of a given scientific community would disclose a number of such rules shared by its members. Probably a few others could legitimately be induced from close observation of their behaviour."[106] Thus, it is obvious that Kuhn acknowledges that there are shared rules. In fact, he often mentions rules in scientific practice. For example, in the fourth chapter of *The Structure of Scientific Revolutions*, he mentions, in "a considerably broadened use of the term 'rule'", a few kinds of rules such as (1) the rules provided by "theories and laws";[107] (2) the rules provided by "endurance of instrumental commitments";[108] (3) "higher level quasi-metaphysical commitments"; and (4) at a still higher level "another set of

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[104] *Ibid.*, p. 42.

[105] Kuhn, *The Essential Tension*, p. 305.

[106] *Ibid.*, p. 302.

[107] Kuhn mentions that during the eighteenth and nineteenth centuries Newton's Laws provided rules for physical scientists. During that time, quantity-of-matter was a fundamental ontological category for them. Cf. T. Kuhn, *The Structure of Scientific Revolutions*, p. 40.

[108] See *ibid.*, p. 41. "in this century the curious history of chemical chromatography again illustrates the endurance of instrumental commitments that, as much as laws and theory, provide scientists with rules of the game."

commitments without which no man is a scientist".[109] Moreover, when Kuhn discusses crises and the emergence of scientific theories, he says: "failure of existing rules is the prelude to a search for new ones."[110]

Nor, secondly, does Kuhn deny that rules are necessary for scientific inquiry. He does not hold that practice is prior to rules or that tradition in science plays a secondary role in scientific inquiry. Rather, he takes practice, rules and theories as coming into being together and he takes theory, methods, and standards to be inextricably mixed in a paradigm. What Kuhn is trying to clarify here is how intellectual achievements play a role in the practice of science. He differs from many philosophers of science not so much in pointing this out but in the particular way he tries to account for it. For him, scientists' achievements function not as abstract rules but as paradigms, especially initially. His view is that since new paradigms are established in the process of practice and it determines a new understanding of past achievements and rules, previous philosophers of science are wrong to assume the existence of a set of rules governing scientific inquiry independently of how it is practised.

Considering what Kuhn says about paradigms and the way in which scientists learn a paradigm, we can understand him better. What he is arguing is that exemplars (understood as

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109 Cf. *ibid.*, pp. 40-42.

110 *Ibid.*, p. 68.

achievements) are fundamental for the understanding of scientific inquiry. When he discusses the function of paradigms, he says that "in learning a paradigm the scientist acquires theory, methods,and standards together, usually in an inextricable mixture".[111] Thus, for him, exemplars are fundamental for paradigms and learning a paradigm is principally a matter of learning an exemplar by direct modelling, one in which the scientist acquires a theory. Moreover, this process is, or at least includes, the process of learning rules that Wittgenstein suggests.

Even though Barker points out this similarity between Kuhn and Wittgenstein, he does not realize what it shows, for he does not correctly understand Wittgenstein's view about the relationship between practice and rules, taking him to hold that practice lies behind rules. As I showed earlier, Kuhn holds that practice and rules stand at the same level. Noting the similarity between Kuhn's and Wittgenstein's views, we can say that Kuhn must hold the same view about the relationship between practice and rules that Wittgenstein does, namely that "'obeying a rule' is a practice". Just as Wittgenstein supports his argument by showing how we learn a rule, specifically by learning rules through examples and practice, Kuhn takes scientists to learn paradigms through examples and practice. Kuhn clearly states that in, or even after, learning a paradigm, scientists do not need to know a theory in its abstract form but

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[111] *Ibid.*, p. 109.

in an inextricable mixture with method and standard.

For Kuhn, in scientific revolutions, a scientist's adherence to a tradition involves the "reconstruction of the field from new fundamentals, a reconstruction that changes some of the field's most elementary theoretical generalizations as well as many of its paradigm methods and applications".[112] Kuhn emphasizes that this reconstruction is achieved not in the form of rules but in the form of an inextricable mixture of theoretical knowledge and its application. Kuhn points out that the purpose of his discussion is not to dispute that there is a set of correspondence rules,[113] but to "lead us to take a bit more seriously some aspects of scientific training and behaviour that philosophers have often managed to look right though".[114] What Kuhn wants to stress is thus how scientists learn rules and the way in which existing knowledge plays a role in scientific development. More specifically, he wants to show that existing intellectual achievements play a role not in the form of a new rule which is usually seen prior to the practice of science, but rather in the form of a paradigm which comes into being with the practice, and in which a new theory or new rule is embryonic.

As I argued above, for Kuhn, practice does not stand behind

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[112] *Ibid.*, p. 85.

[113] Kuhn says that correspondence rules "have ordinarily been taken to be either operational definitions of scientific terms or else a set of necessary and sufficient conditions for the terms' applicability." *The Essential Tension*, p. 302.

[114] *Ibid.*, p. 305.

rules, since rules and theories are exhibited in practice and embodied in paradigms. Nor should they be thought of as prior to practice. Kuhn emphasizes that traditions do not provide scientists with rules for finding new paradigms. Moreover, as I demonstrated in the preceding chapter, Kuhn does not suggest that scientific inquiry cannot deviate from tradition. Rather, he emphasizes that traditions play an essential role in science. From all of this we can conclude that when Kuhn emphasizes the priority of paradigms, he does not mean that there are no rules, or that paradigms stand behind rules, but rather that in a scientific inquiry scientists acquire a new theory and new rules in a inextricable mixture first. In other words, Kuhn does not maintain the priority of paradigms in the sense of which, paradigms or rules, come into being first, but holds that this mixture of elements constitute an important form of knowledge.[115] As Kuhn puts it:

> When I speak of knowledge embedded in shared exemplars, I am not referring to a mode of knowing that is less systematic or less analyzable than knowledge embedded in rules, laws, or criteria of identification. Instead I have in mind a manner of knowing which is misconstrued if reconstructed in terms of rules that are first abstracted from exemplars and thereafter function in their stead.[116]

Thus we can see that Kuhn compares paradigms with rules and

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[115] In the fifth chapter of *the Structure of Scientific Revolutions*, Kuhn emphasizes that scientists learn and use even existing rules and theories in an inextricable mixture.

[116] T. Kuhn, Postscript to *The Structure of Scientific Revolutions*, University of Chicago Press, second edition, Chicago, 1970, p. 192.

assumptions because he holds that both paradigms and rules are linked with theories. When he emphasizes the priority of paradigms, what he wants to say is that paradigms are "more binding and more complete than any set of rules for research that could be unequivocally abstracted from them".[117] A paradigm can more easily be found, be learned, be used and be distinguished from other paradigms. Kuhn's view that paradigms have priority and his view that practice and rules stand at the same level are not contradictory.

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[117] Kuhn, *The Structure of Scientific Revolutions*, p. 46.

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Continuity or Incommensurability?

To get a clearer understanding about the genesis and continuation of traditions, I will first consider the question of how paradigms come into being. I will attempt to show that many of Kuhn's ideas are important. I believe that bringing out these ideas is helpful for our discussion of tradition's formation and change. These ideas still have not been accorded the attention they deserve, since Kuhn couples them with his "incommensurability" and "community authority". The former thesis claims that the pre- and postrevolutionary normal scientific traditions are incommensurable because the proponents of competing traditions will often disagree about the list of problems that candidates for the paradigm must resolve. Their standards or their definitions of science will not be the same and since they practice their trades in different worlds, they will fail to make complete contact with each other's viewpoints and misunderstand one another.[118] Based on this, Kuhn put forward the latter thesis, namely, that when a community chooses a paradigm among those "incommensurable" paradigms, there is no better criterion than the decision of the scientific group.[119] I believe that both theses are questionable. "Incommensurability" leads to the view that science involves irrational changes of paradigms or theories, and a change of tradition is thought to be a discontinuous process.[120] In this chapter I will discuss Kuhn's idea about the origins of paradigms and criticize incommensurability. I argue that Kuhn overemphasizes the incommensurability of paradigms. In addition, Kuhn's "incommensurability thesis" is extended by Feyerabend as "incommensurability" between different theories.

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[118] Cf. Kuhn, *The Structure of Scientific Revolutions*, pp. 148-150.

[119] Kuhn, *The Structure of Scientific Revolutions*, p. 170.

[120] According to Kuhn, the development of science has many revolutions in which "an older paradigm is replaced in whole or in part by an incompatible new one (*Ibid.*, p. 92)", and "after a revolution scientists work in a different world (*Ibid.*, p. 135)".

Feyerabend goes on to take the role of tradition in science to be relativistic. Therefore Feyerabend's view is relevant to our discussion of tradition. I will also question Feyerabend's thesis regarding the "incommensurability" of theories.

1. The Genesis of Paradigms

Let us look at the genesis of tradition. I begin by noting what Kuhn says about how paradigms come into being. Since Kuhn takes traditions to be closely related to paradigms, and regards paradigms in crisis as giving rise to new traditions,[121] a discussion of his view of how paradigms come into being will help clarify the question of the genesis and change of tradition. In what follows, I attempt to show that according to Kuhn the transition from a paradigm in crisis to a new one is a process, one that involves the discovery of facts and the development of new theories. This process, in which the paradigm theory is adjusted, takes time. Furthermore, the discovery of scientific facts and the invention of theories explaining these facts (thereby making them scientific facts) are completed at the same time. Based on this, I suggest that Kuhn's view should lead to the view that a change of tradition happens in the surroundings of the old one and is a continuous action. However, Kuhn did not extent his view in this direction.

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[121] See *ibid.*, p.84 "The transition from a paradigm in crisis to a new one from which a new *tradition* of normal science can emerge."

When discussing changes of tradition, the first question that arises is whether these changes are led by the discovery of new phenomena and theory invention. In particular we should like to know whether the genesis of a tradition needs both practical and theoretical inputs. The answer to this question is very important for our discussion of tradition, because it will have a direct bearing on the discussion of "tradition following" and "tradition choice". I shall argue that the genesis of tradition needs both practical and theoretical sides. In doing so, I will focus on Kuhn's discussion and Hanson's point of view about "theory-laden" observation, both of which suggests that theoretical and practical knowledge cannot separated.

Kuhn discusses research traditions in terms of paradigms. When he discusses what makes paradigms change, he points out that there are two kinds of causes or contributors to paradigm change, namely discoveries and the invention of new theories.[122] In addition, when he discusses scientific discovery he makes the point that factual and theoretical novelties are closely intertwined. I agree with this point and will try to establish that he holds that, in both cases, discovery of facts and invention of theories go together by examining Kuhn's two causes of paradigm change. I suggest that a change of traditions involves both discovery of scientific facts and theoretical innovation.

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[122] Cf. *ibid.*, p. 66.

Looking at the case where discoveries lead to the change of tradition, we can easily see that Kuhn believes that in science, discoveries of fact and inventions of theory go hand in hand. In fact, they cannot be separated; the change of tradition caused by factual discovery involves theory invention. Kuhn points out that scientific discovery includes "both observation and conceptualization, fact and assimilation to theory".[123] This point echoes Hanson's point about the interplay between observations and the notion system in which they are expressed. In his 1958 discussion of observation Hanson says:

> Physical science is not just a systematic exposure of the senses to the world; it is also a way of thinking about the world, a way of forming conceptions.[124]

Before Hanson got philosophers to pay more attention to the view that observation was "theory-laden" activity, some philosophers of science assumed that experiment and observation were a sort of neutral activity, not one involving theories. Like Hanson, Kuhn, who focuses on the relation between discovery of scientific facts and theory invention, tells us that they are intertwined with each other. Thus, what Hanson and Kuhn say should be understood as starting points drawn from their investigation of observation. For both, changes of tradition that are caused by the discovery of new facts need both

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[123] *Ibid.*, p. 55.

[124] Cf. 1958 discussion. He also says that "scientific observation is thus a 'theory-laden' activity." Norwood Russell Hanson, *Observation and Explanation*, Harper Torchbooks, New York, 1971, p.5.

discovery of fact and invention of theory.

Besides, the changes of tradition caused by theory invention are also connected with the discovery of scientific facts. This is apparent in Kuhn's discussion of the other source of paradigm changes, namely, theory invention. Kuhn contends, all inventions of theories occur in "a state of growing crisis". Kuhn gives many examples,[125] and concludes:

> In all these cases except that of Newton the awareness of anomaly had lasted so long and penetrated so deep that one can appropriately describe the fields affected by it as in a state of growing crisis. Because it demands large-scale paradigm destruction and major shifts in the problems and techniques of normal science, the emergence of new theories is generally preceded by a period of pronounced professional insecurity. As one might expect, that insecurity is generated by the persistent failure of the puzzles of normal science to come out as they should. Failure of existing rules is the prelude to a search for new ones.[126]

Kuhn's main point here is that the puzzles that lead to paradigm changes are discoveries of facts that scientists have known for a long time, and have been thinking about already (or would be finally) accommodated within the framework of normal science. Now they recognize that they need new theories to assimilate those facts. So by investigating theory change, Kuhn shows that theory invention requires re-considering of special scientific

[125] See Kuhn, *The Structure of Scientific Revolutions*, p.67: Copernicus' theory; Galileo's motion theory; Newton's new theory of light and colour; wave theory; thermodynamics and quantum mechanics.

[126] *Ibid.*, pp. 67-68.

facts.[127]

I have suggested that we should interpret Kuhn as holding that paradigm change, and thus change of tradition in normal science, whether caused by discovery of a new fact or by theory invention, involves both factual and theoretical sides. Thus, when we examine the continuity of tradition we should focus on both sides.

Secondly, when we discuss change of tradition, we need to recognise that change of tradition is an activity, a process that takes time. In fact, Kuhn's account of paradigm change leads directly to the conclusion that a change of tradition is a continuous process. Therefore, there must be a period in which both the newly formed tradition and the old one coexist, i.e., new traditions must be generated in the surrounding of old traditions. For example, Kuhn says:

> If both observation and conceptualization, fact and assimilation to theory, are inseparably linked in discovery, then discovery is a process and must take time.[128]

As I pointed out above, Kuhn holds that discovery is a source of change of tradition. Since discovery is a process and

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[127] From my point of view we can better understand why Kuhn puts Newton's case here. Kuhn holds that Newton's new theory "originated in the discovery that none of the existing pre-paradigm theories would account for the length of the spectrum." *Ibid.* Thus for him Newton's case can also be classified into the group, in the cases of which paradigm changes occurred by a discovery of fact. The only difference between this case and the cases of the group is that in Newton's case the fact seems to have been known only for a short time.

[128] Kuhn, *The Structure of Scientific Revolutions*, p. 55.

must take time, it is obvious that change of tradition takes time. But what happens in this process and how?

Change of tradition is a process in which the newly formed tradition is being adjusted in the surrounding of the old tradition. Kuhn rightly holds that in the process of discovery the paradigm theory is adjusted, and that the discovery of a scientific fact and the invention of the theory that makes it a scientific fact are completed at the same time. For instance, Kuhn says:

> We must now ask how changes of this sort can come about, considering first discoveries, or novelties of fact, and then inventions, or novelties of theory. That distinction between discovery and invention or between fact and theory will, however, immediately prove to be exceedingly artificial. Its artificiality is an important clue to several of this essay's main theses. ... [Discoveries] are not isolated events but extended episodes with a regularly recurrent structure. Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science. It then continues with a more or less extended exploration of the area of anomaly. And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected. Assimilating a new sort of fact demands a more than additive adjustment of theory, and until that adjustment is completed--until the scientist has learned to see nature in a different way--the new fact is not quite a scientific fact at all.[129]

Here, Kuhn suggests that the process of discovery is the process of theory adjustment. He begins by assuming that discoveries of fact and inventions of theory are achieved at the same time and concludes that this process ends when the phenomenon supporting the theory and the theory explaining the fact.

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[129] *Ibid.*, pp. 52-53.

However, before the new theory explains the fact, the existing paradigm describes it as an anomaly. Using existing theory scientists try to find out as much about its features as they can. Therefore, the process of theory adjustment starts and proceeds under an existing paradigm. As Kuhn puts it, "all the relevant conceptual categories are prepared in advance" of the new tradition.[130] This suggests that new traditions are generated in the surrounding of an existing one. Kuhn himself mentions that the effective way to change a paradigm is to do research under this paradigm. Kuhn says: "research under a paradigm must be a particularly effective way of inducing paradigm change"; "under one set of rules, their assimilation requires the elaboration of another set."[131] There can thus be no doubt that for Kuhn the existing tradition plays a role in the process of finding a new one.

I have argued that change of tradition needs both factual and theoretical sides. Also I have argued that Kuhn views the transition from a paradigm in crisis to a new one as a process in which a change of tradition occurs in the surroundings of the old tradition. If my interpretation of Kuhn's points is right, then I believe that this should lead to the continuity of tradition, rather than incommensurability.

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[130] T. Kuhn says: "Only when all the relevant conceptual categories are prepared in advance, in which case the phenomenon would not be of a new sort, can discovering that and discovering what occur effortlessly, together, and in an instant." *Ibid.*, p. 55.

[131] *Ibid.*, p. 52.

2. The Continuity of Tradition

Indeed the genesis and the continuity of tradition are interrelated since the genesis of tradition, understood as the forming of a new tradition, occurs in the surroundings of the old traditions. Thus the genesis of a tradition can be understood as involving the change of an existing tradition. The continuity of tradition, understood as either "the tenacity of some theories",[132] or, better, the tenacity of some ideas, methods and principles, is presented in it.

First, there is a continuation of tradition, since only those anomalies that have some connection or overlap with existing tradition can be easily found and seriously investigated, and the old tradition plays an important role in the development of the new one. In particular, when the old tradition gives way to the new one, many concepts and ideas of the old tradition will be carried on. Just as a tradition of architecture is embodied in a typical building, a tradition of science is embodied in the explanation of particular facts. However, as discussed above, the factual and theoretical aspects of scientific change cannot be separated. The observation and expression of a fact embody existing theories, and thus the existing tradition. For example, the reading of the instruments, the recording and analysis of data, the expression

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[132] Compare I. Lakatos, "Falsification And The Methodology Of Scientific Research Programmer", in *Criticism And The Growth Of Knowledge,* Edited by Imre Lakatos and Alan Musgrave, Cambridge University Press, 1970, p. 174.

of these facts, as well as their explanation depend not only on existing theory but also on (as Kuhn mentions) standard procedures of experimentation,[133] which reflect the existing theories.

Kuhn talks about the negative role of existing theory and the existing instruments. For him, the existing tradition precludes the discovery of the new theory. Yet he also argues that the discovery of anomalies are linked to existing traditions. Kuhn believes that existing traditions restrict the phenomenological field accessible to scientific investigation. For example, when he discusses the discovery of X-rays and the belated discovery of oxygen which led to the new tradition of chemistry, he says:

> Consciously or not, the decision to employ a particular piece of apparatus and to use it in a particular way carries an assumption that only certain sorts of circumstances will arise. There are instrumental as well as theoretical expectations, and they have often played a decisive role in scientific development.[134]

Moreover, he says that we ought not to conclude from the frequency with which such instrumental commitments prove misleading that science should abandon standard tests and standard instruments. That, he says, "would result in an inconceivable method of research. Paradigm procedures and applications are as necessary to science as paradigm laws and

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[133] See Kuhn, *The Structure of Scientific Revolutions*, p. 60.

[134] *Ibid.*, p. 59.

theories, and they have the same effects."[135] Thus for Kuhn existing tradition including theories and procedures and applications, is necessary for finding the anomalies which lead to new traditions. Because existing tradition results in anomalies, and because scientists are unable to abandon existing traditions. These anomalies must have some connection with existing tradition.

Second, there is a continuity of tradition, because during the change of tradition, the old tradition plays a role in the formation of the new one; it influences the choice of new theories, with the result that science grows as a interlinking or overlapping development of theories. During the period of a change of tradition old theories shape the form of the new tradition, because the new tradition is not completely formed yet; it has to be explained in terms of the old tradition, and its adjustment has to be evaluated by checking with observation, which is an old-theory-laden activity. This will not change until the new theory is established and the relevant facts and observations expressed anew. In fact, when Kuhn discusses discovery and theory invention he recognizes that the new theory and the new-theory-laden expressions of the facts are completed at the same time. Moreover, in the adjustment period there must be something by which scientists evaluate whether the adjustment is sufficient. This "something" must be old-theory-laden observation and old theory. I will discuss this point in detail

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[135] *Ibid.*, p. 60.

in chapter 7.

This idea can be discerned in what Kuhn says about the stability of value. His notion of value here actually includes the existing tradition, composing both existing theory and existing evaluative criteria. In fact, a value is formed in the surroundings of an existing tradition and must reflect the point of view of existing tradition. Kuhn believes that value change is slower than theory change, and so it can provide the justification for theory choice. He says:

> Clearly, if such value changes had occurred as rapidly or been as complete as the theory changes to which they related, then theory choice would be value choice, and neither could *provide justification* for the other. But historically, value change is ordinarily a belated and largely unconscious concomitant of theory choice, and the former's magnitude is regularly smaller than the latter's. For the functions I have here ascribed to values, such *relative stability* provides a sufficient basis. The existence of a feedback loop through which theory change affects the values which led to that change does not make the decision process circular in any damaging sense.[136]

For Kuhn, the relative stability of values provides justification for theory choice. I believe that this shows that it is tradition that plays a role in forming and choosing the new paradigm. There is no doubt that in this way the existing tradition passes something down. Thus, the role that an existing tradition plays in theory choice makes science develop as interlinked theories. I will discuss the role of tradition in theory choice in detail in chapter 9.

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[136] Kuhn, *The Essential Tension,* p. 336. emphasis added.

Moreover, the continuity of old traditions is a result of the fact that existing traditions serve as the basis for finding new ones. Scientists often find new theories in their efforts to revise existing ones. In fact, after finding an anomaly, the process of theory adjustment starts. No matter how different the new paradigm and the existing theories are, the adjustment invariably begins from existing theories. Scientists attempt to find what in the existing theories needs to be changed. For example, in the process of the discovery of oxygen, scientists began by adjusting the old theory. After all, while new paradigms are different from the existing ones, they normally borrow many terms and ideas from the existing ones. For instance, as Kuhn himself acknowledges, Maxwell's theory originated in Newton's theory; Newton's theory was the paradigm from which Maxwell's theory sprung.[137]

Third, in retrospect, a change of tradition usually appears as change in a small part of science, while the main part of it continues as before. For the most part of science the main tradition continues, as Kuhn has observed. He points out that a theory can "simultaneously determine several traditions of normal science that overlap without being coextensive. A revolution produced within one of these traditions will not necessarily extend to the others as well".[138]

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[137] See Kuhn, *The Structure of Scientific Revolutions*, p. 74.

[138] *Ibid.*, p. 50.

In fact after a change of tradition, science as a whole still looks much the same. The fact that it includes some principles that look to be contradictory notwithstanding. Not only does the change of one tradition typically not lead to the change of others, the changed tradition is itself integrated into the rest of science. This is particularly clear in cases such as the theory of special relativity and quantum mechanics, which are linked to Newtonian theory by correspondence principles. More to the point, traditions can be integrated although they come from different sources. For example, relativity theory comes from continuous medium mechanics, while quantum mechanics stems from acceptance of a quantum of action. However, in relativistic-quantum mechanics they are partly integrated.

3. On Incommensurability

In what follows, I will attempt to demonstrate that Kuhn's argument for incommensurability is questionable. In doing this I am not trying to deny that there are revolutionary changes in the development of science. What I am trying to do is to show that since new tradition forms in the surroundings of the old one, there are many connections between them, and thus that there is continuity between traditions.

Kuhn gives three main reasons for incommensurability thesis. First, he attempts to say that for within a single domain different traditions have different lists of problems, thus their standards or their definitions of science are not the same. Kuhn believes that this leads to different traditions that are incommensurable. He says: "the proponents of competing paradigms will often disagree about the list of problems that any candidate for a paradigm must resolve. Their standards or their definitions of science are not the same".[139] When he supports his point, he compares Newton's theory with Aristotle's and Descartes's theories and with general relativity; and he compares Lavoisier's chemical theory with phlogistic chemistry. Kuhn notes that Aristotle's and Descartes's theories, in contrast to Newton's theory, explain the cause of the attractive forces between particles of matter. Thus, since Newton's theory simply assumes the existence of such forces, once it is accepted, a question was therefore banished from science. That question, however, is one that general relativity may proudly claim to have solved. Kuhn believes that this supports the view that says Newton's theory and general relativity cannot be in contact with each other, i.e., the two theories cannot be measured by comparing how they have done on solving the same question.

But how clear is it that competing paradigms have radically different lists of problems? And how clear is it that their standards or their definitions of science are always different? If we look closely at Kuhn's examples, we can see that not only do the different traditions consider similar problems, they are

[139] *Ibid.*, p.148.

commensurate since they are concerned with similar problems. There is a connection between them, and they can be compared. For example, when Newton's theory had been accepted, the question concerning the cause of the attractive forces between particles of matter, in fact, was not banished from science. First, in Newton's mind natural forces are subjects of his work. He says: "I ... write not concerning manual but natural powers, and consider chiefly those things which relate to gravity, levity, elastic force, the resistance of fluids, and the like forces, whether attractive or impulsive".[140] This shows that Newton's "list of problems" includes the attractive forces between particles of matter. When Newton talks about the force between particles he says that the causes of this force is "*hitherto* unknown", and he hopes that "[his] principles ... will afford some light [to this force]".[141] Moreover, rather than feeling total achievement, Newton clearly knew where he was. He says: "I *have not been able to discover* the cause of those properties of gravity from phenomena."[142] This shows that Newton does not deny that there is a cause of gravity and it should be found out. Rather, he takes his principles of motion as a step in the development of "philosophy" and at this step he can be

[140] Sir Isaac Newton's *Mathematical Principles of Natural Philosophy and His System of the World,* Translated by Andrew Motte in 1729. the translations revised by Florian Cajori, University of California Press, Berkeley and Los Angeles, 1966. Volume 1, p. xvii.

[141] *Ibid.*, p. xviii.

[142] *Ibid.*, emphasis added.

satisfied with the discovery of the law of gravitation and leave this cause to be considered later. This is why he says:"to derive two or three general Principles of Motion from Phenomena, and afterwards to tell us how the Properties and Actions of all corporal Things follow from those manifest Principles would be a very great *step* in Philosophy, though the Causes of those Principles were *not yet* discovered. And, therefore, I scruple not to propose the Principles of Motion above-mentioned, they being of very general Extent, and leave their Causes to be found out."[143] The history of science shows that the step Newton made is very important. It is hard to think that general relativity can be built without the knowledge reached by this step. Also notice that even Newton himself did not think that a theory of motion ought to stop work on the question concerning the cause of the attractive forces between particles of matter. It is one thing to hold that at the first step we can simply note the existence of the attractive forces between particles, quite another to deny that there is a cause of this force and that it should be found out. Since Newton did not rule out the question that Kuhn mentioned, it is hard to say that the list of questions of Newton's theory and general relativity are so different that they cannot be evaluated by comparing their answers to a fixed set of questions.

Secondly, Kuhn attempts to say that since different

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[143] Isaac Newton, *Optics*, Dover Publications, INC. New York, 1979, pp. 401-402. emphasis added.

traditions use terms and concepts in different ways, the proponents of competing paradigms must fail to make complete contact with each other's viewpoints. Thus, the two competing schools cannot understand each other. Kuhn holds that although new paradigms

> incorporate much of the vocabulary and apparatus, both conceptual and manipulative, that the traditional paradigm had previously employed, ... they seldom employ these borrowed elements in quite the traditional way. Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with other. The inevitable result is what we must call, though the term is not quite right, a misunderstanding between the two competing schools.[144]

So for Kuhn, since the same terms of two competing paradigms have different meanings, the two competing schools cannot understand each other, thus the competing paradigms are incommensurable. I argue that this reason is not suitable, for it is not necessary that competing schools must misunderstand each other.

I suggest that the fact that a tradition employing the vocabulary and apparatus of the previous tradition shows the continuation of tradition, and that thus competing schools can understand each other. The point is why a new tradition employs the vocabulary and apparatus that the previous tradition employed. I believe it is not only because those borrowed vocabularies and apparatus existed beforehand, but also because the new tradition have many connections with the existing ones. It is true that some terms in different paradigms have different

[144] Kuhn, *The Structure of Scientific Revolutions*, p. 149.

definitions, but they are not so different that two paradigms cannot be connected. For example, special relacivity comes in the surrounding of Newton's theory and Maxwell's theory. It employs the concepts of both. It is interesting that before special relativity came into being, scientists had already had some results, such as the Lorentz transformations. In the special relativity the term of space and time still have the basic meaning as they have in the old theories, yet Einstein holds that we should not understand space as absolute nor understand time as absolute, that there is a relativity of time. Lorentz articulated a relativistic conception of time, but unlike Einstein, he did not take it to be real time, but rather called it mathematical time. It is obvious that special relativity is different from classical theories. But it is also obvious that their terms are connected. The different meaning of the terms in two traditions can be illustrated by considering their meaning in the way we are now explaining the difference of time view between the two, which involved using a basic meaning of time. These two competing schools did not have any problem understanding each other, despite the difference between their concepts of time. It is hard to believe that because they use those terms in different ways, people cannot understand both, nor compare them. So the different use of the same terms does not necessarily lead to incommensurability.

This also relates to Kuhn's third point. Based on his second point, Kuhn suggests that "the proponents of competing paradigms practice their trades in different worlds".[145] So scientists often cannot make a transposition from an old paradigm to a new one. I argue that since competing traditions describe the same world, yet in different ways, they are not as different as different "worlds". While the theories in the different traditions are different, they are related to each other; in other words, different traditions are not "worlds" which are so different that scientists cannot work in both. In fact, scientists often go wrong, when they actually hold that they are working in different worlds, and not following existing general principles. For example, when Niels Bohr held that in building the new quantum theory, we should not trust existing theories too much but should only make sure that there is no contradiction in the new theory. In the 1920's he twice denied that the law of conservation of energy holds for elementary processes. He believed that in such processes the law only holds in a statistical sense. But this is wrong. This shows that when he thought that he was working in another world, in which the law of conservation of energy does not stand, he erred. On the contrary, when he put foreword the "correspondence principle", he actually believed that the new theory in some cases should get observational result equivalent to the old one. This means that the new and the old theories are not absolutely separate from each other, rather they are correlative. In fact the history of the development of quantum

[145] Kuhn, *The Structure of Scientific Revolutions*, p. 150

mechanics shows that this principle plays an important role. This suggests that in some cases the two traditions can be compared in that they describe the same world and yield equivalent results. Niels Bohr's negative and positive experiences suggest that we should not view different paradigms as different worlds.

More than that, old tradition plays a role in learning the new one, the concepts of the old tradition actually are also a base of learning the new one. I believe that Kuhn overlooks this point. When Kuhn supports his point of view about two different worlds he cites what Max Planck, in his *Scientific Autobiography*, says: "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it."[146] I believe what Planck says is, at best, only partly true. By saying that, I do not simply mean that scientific truth does not triumph only when its opponents die; I mean that for many scientific traditions, a new generation has still to be trained in the old traditions. This has the effect of eliminating opposition as well as serving to reinterpret the old ideas within the new framework. In fact, up to now students in departments of physics still have to learn Newton's theory before they study special relativity. In learning classical theory they learn the meaning of terms like

[146] Max Planck, *Scientific Autobiography and Other Papers*, trans. F. Caynor, New York, 1949, pp. 93-94.

"mass", "velocity", "space", and "time". Not only is this experience not an obstruction to learning special relativity, it serves as a base for learning the new theory, the fact that these terms have different meaning in new theory notwithstanding. Besides, when the velocity of a body is very low, the results of the descriptions of both theories are equivalent. This again shows that there is a connection between the new tradition and the old one.

I hope I have shown that Kuhn's reasons for saying "incommensurability" of paradigms are unclear and weak.[147] Now I want to examine Feyerabend's point of view about the incommensurability of theories. His point of view about incommensurability of theories is closely related to our discussion about continuity of tradition. As I pointed out in chapter 2, that traditions usually built around theories. This is why Lakatos speaks of the continuity of existing tradition in terms of "the tenacity of some theories", and why Kuhn sometime uses the phrase "paradigm theory" instead of "paradigm" when discussing change of tradition. Moreover, Feyerabend explicitly compares his point of view with Kuhn's view regarding the incommensurability of paradigms. So if theories are, as Feyerabend says, incommensurable, we cannot even compare two

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[147] Related to the "incommensurability" of paradigms, some philosophers suggest that Kuhn also holds that even though scientists in a group share the same values, same standards, their understanding of those values and standards may not be the same. I think this relates to the theory choice and shall discuss this topic in chapter 9.

theories, and thus cannot find if there is any tenacity of theories of a tradition. Besides, Feyerabend puts his point of view in a clearer way than Kuhn does and makes use of his idea about "incommensurability" to support his relativism regarding progress. In what follows, I will attack his point of view about incommensurability of theories.

First, let us see what Feyerabend means by incommensurability of theories, and how he thinks about it. While Kuhn answers criticisms of his view of the incommensurability of paradigm by saying that it does not mean paradigms are incomparable, Feyerabend characterizes incommensurability of theory in term of incomparability of the facts explained by the two theories. Also, he insists that Kuhn's incommensurability of paradigm involves the same idea. Thus in *Farewell to Reason,* when discussing the progress of science, Feyerabend explicitly states that we cannot talk about progress because "it is no longer possible to compare the facts of one theory with those of the other".[148] As he puts it, "Professor Kuhn and I have used the term 'incommensurability' to characterize this situation."[149] Feyerabend believes that his idea of incommensurability of theories is not self-refuting. He agrees that "the idea (of incommensurability) may have unusual

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[148] Feyerabend, *Farewell to Reason*, p. 156.

[149] *Ibid.,*, p. 156. While Kuhn may not think so, I believe that Feyerabend is right to regard Kuhn's point as the same as his. Indeed, Kuhn's three points discussed above concern the impossibility of comparing of two paradigms.

consequences", but insists that "self-refutation is not one of them."[150]

But this is doubtful. I now want to suggest that Feyerabend falls into self-refutation since he overemphasizes the incommensurability of theories. I demonstrate this by bringing his two points of view together to show that he contradicts his own statement. One of the views is that "it is no longer possible to compare the facts of one theory with those of the other"; another view is that "given any set of facts, there always exists a variety of theories that agree with the set" and "even a refuted rival of a theory ... may overtake the successful theory: research may remove evidence from it and transfer it to the disreputable rival while at the same time using the evidence that makes the rival disreputable to discredit the successful point of view".[151] There are two points that Feyerabend emphasizes in the latter one. First, there is a variety of theories that agree with any one set of facts. Second, evidence may be removed from one successful theory to another theory. I shall argue that his latter view comports poorly with his former one, i.e., the view about incommensurability. This suggest that Feyerabend himself actually is vacillating on his idea of "incommensurability".

On one hand, if we cannot compare the facts of one theory with those of the other, how does Feyerabend know that there

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

[151] *Ibid.*, p. 156.

exists a variety of theories that agree with one set of facts? We cannot recognize that evidence is removed from one successful theory to another theory. For according to Feyerabend, "incommensurability" implies that theories decide what facts you can observe. In his view Feyerabend says that one theory "is incapable of expressing facts of (other theory). We must start all over again".[152] In other words, it is not possible to think that there are two theories that agree with one set of facts, and it is not possible to recognize that evidence is removed from one theory to another, as Feyerabend also contends.

On the other hand, if there exists a variety of theories that agree with one set of facts, then at least we can compare the facts of those theories. However, this runs counter to Feyerabend's other point. This contradiction suggests that his two views cannot be right at the same time. His idea of incommensurability does lead to self-refutation.

In what follows I show that Feyerabend actually compares a fact explained by one theory with a fact explained by a second one. When he says the fact of free fall of heavy bodies in both Aristotle's theory and Galileo's theory, he is talking of the very same fact, contrary to his view that "it is no longer possible to compare the facts of one theory with those of the other". Indeed, Feyerabend actually holds the following.

> The free fall of heavy bodies for a long time supported the idea that the earth was at rest. Experience teaches that motion needs a moving force

[152] *Ibid.*

> and comes to a standstill when the force stops acting. Now if I release a stone from the top of a tower, and if the earth moves, then it no longer follows the moving earth, it stays behind and should describe an inclined trajectory. The stone falls straight down-hence the earth is at rest. Galileo replaced the Aristotelian law of motion-each motion needs a moving force, otherwise there is no motion--by his own highly speculative law and was now obliged to transfer the evidence in favour of Aristotle to his own view. This he did not succeed in doing--the theory of friction and of air resistance, and the whole subject of aerodynamics, did not exist at the time. But he started a process in whose course the arguments in favour of an unmoved earth were gradually defused and transferred to the Copernican point of view.[153]

Here Feyerabend takes the fact of free fall of heavy bodies in both Aristotle's theory and Galileo's theory to involve the idea of falling straight down.

The explanations in two theories of why a stone falls straight down are different. But this is a theoretical, not a factual matter. As in his expression above, the free fall of heavy bodies does not itself support the idea that the earth was at rest. Rather, as Feyerabend says, the Aristotelian law and the fact of free fall of heavy bodies together support the idea that the earth is at rest. Also without Galileo's law of motion the fact of free fall of heavy bodies does not by itself support the Copernican point of view. Nor is it a theoretical matter that whether the earth is at rest, rather it is a matter of fact. Thus, we can say that the fact of free fall of heavy bodies plus the "fact" that the earth is at rest together support the Aristotelian theory: while the fact of free fall and

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[153] *Ibid.*, pp. 156-157.

the fact that the earth is moving together support the Galileo's theory of motion. Thus, the expressions of the facts explained by two theories are not different and can easily be compared, the one being true, the other false.

From the above we can see that Feyerabend's point of view about incommensurability of theories is self-contradictory, and contrary to what he says. The competing theories can be compared. Thus we can find out what in a tradition is changed and what is carried forward when one theory is replaced by another.

I have shown that Kuhn's ideas suggest that new traditions are formed in the surroundings of existing traditions. This implies that the new tradition must have a trace of the old one. Kuhn, however, does not develop these ideas. On the contrary, he overemphasizes the incommensurability of the pre- and postrevolutionary normal-scientific traditions. In the light of Kuhn's "incommensurability" of traditions, these other ideas I have mentioned have not been sufficiently appreciated.

I have been arguing that Kuhn reaches an untenable conclusion, namely that paradigm change is a "sudden and unstructured event", because he overemphasizes the incommensurability of scientific traditions. The trouble is that he thinks that a tradition of a normal science can only play a role in recognizing the anomalies that lead to crises, but not in the formation of new traditions. He says:

> Paradigms are not corrigible by normal science at all. Instead, as we have already seen, normal science

> ultimately leads only to the recognition of anomalies and to crises. And these are terminated, not by deliberation and interpretation, but by a relatively *sudden* and *unstructured* event like the gestalt switch.[154]

This point of view runs counter to his view that paradigm changes are "not isolated events but extended episodes with a regularly recurrent structure", and his contention that paradigm change "must take time" given that the discovery of new fact does not occur all at once.[155]

Furthermore, overemphasizing the incommensurability leads to the view that normal science can do nothing in the formation of the new tradition. To say now that the change of paradigm, or genesis of tradition, is a "sudden and unstructured event" is to rule out research on the sources of paradigm. Because an unstructured event cannot be analyzed, it is impossible for there to be any connection between old and new traditions. It is for this reason that he is forced to speak of a change of

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[154] Kuhn, *The Structure of Scientific Revolutions*, p. 122.

[155] See *ibid.*, p. 52, Kuhn says: "Examining selected discoveries in the rest of this section, we shall quickly find that they are not isolated events but extended episodes with a regularly recurrent structure. Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science. It then continues with a more or less extended exploration of the area of anomaly. And it closes only when the paradigm theory has been adjusted so that the anomalous has become the expected. Assimilating a new sort of fact demands a more than additive adjustment of theory, and until that adjustment is completed--until the scientist has learned to see nature in a different way--the new fact is not quite a scientific fact at all."

paradigm as coming to a person "in sleep" or by "intuition".[156] Obviously, if the new tradition and the old one are incommensurable and the new paradigm is formed suddenly, we cannot say the old tradition shapes the new one.

Finally, as I pointed out above, as a result of overemphasizing incommensurability, Kuhn ends up defending the community authority thesis, which claims that there is no better criterion than the decision of the scientific group. I shall discuss this thesis further in chapter 9.

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[156] See *ibid.*, p. 123.

6

Rule-Following

To clarify the role of tradition in science, I am going to discuss rule-following in more detail. First, I shall show why we need to discuss rule-following when considering tradition. Then I shall show that rule-following activities are not entirely limited by rules.

1. Rule-following and Traditional Activity

First, I want to argue that rule-following and traditional activity have some common aspects--they are both activities in which people follow existing criteria (or rules).

Some people defend this view without indicating it. For example, when Michael Polanyi talks about tradition, he holds that in learning a tradition, an apprentice not only learns the *rules* which the master knows, but also learns *hidden rules,* which are not known to the master. He says:

> By watching the master and emulating his efforts in the presence of his example, the apprentice unconsciously picks up the rules of the art, including those which are not explicitly known to the master himself. These hidden rules can be assimilated only by a person who surrenders himself to that extent uncritically to the imitation of another.[157]

From this, it follows that when people follow a tradition, they follow the rules acquired by watching and imitating the master's examples in the course of learning the tradition. Some of these rules are those that the master intended to pass on, while some of them are sorted out by the apprentice himself. For Polanyi, following a tradition, in the sense that there are certain rules for the practitioners, is rule-following activity.

Second, I want to point out that we should discuss rule-following with tradition, because when people learn a rule from a person who acts a certain way, they actually surrender themselves to a tradition. In other words, rules belong to traditions and rule-following is "tradition-following". We should not think of them as exclusive.

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[157] Polanyi, *op. cit.*, p.53

Third, we need to consider whether tradition can lead to new results because this directly relates to the question of whether tradition can play a role in scientific revolutions. Although people believe that rule-following is different from proceeding in a traditional way, they usually think that people who follow traditions follow the method used by the last generation, and that it thus cannot lead to new ideas. This too is what people tend to think about rule-following; usually they take rule-following activities to be entirely limited by rules and hence to be mechanical activities that cannot lead to radically new results. However, I shall argue that both following tradition and rule-following can lead to major new discoveries.

2. Rule-learning

Let me now turn my attention to rule-following itself. In chapter 4, when I discussed the relation between theory and practice, I maintained that rule-following is a practice. Now I want to suggest that in rule-following theoretical knowledge may play a significant role.

First, let us look at the beginning of rule-learning. When theories or rules are passed down, they play a role from the very beginning, since the context of practice in which a recruit learns a rule is chosen by the master in accordance with his understanding of that rule. Thus, learning a rule cannot be a straightforward mechanical exercise; rather it reflects a particular understanding of rules. In what follows I shall attempt to support this argument by analyzing Polanyi's view.

In *Personal Knowledge* Michael Polanyi develops the idea that scientists' knowledge is in an important sense personal knowledge and holds that this plays an important role in the tradition of science. He says:

> An art which cannot be specified in detail cannot be transmitted by prescription, since no prescription for it exists. It can be passed on only by example from master to apprentice.[158]

However, "to learn by example is to submit to authority"; it is to "submit to tradition".[159] For Polanyi passing on of knowledge cannot be done without showing its applications.

There can be no doubt that Polanyi points out an important fact. But we need to understand his idea properly as well as the similar ideas of Kuhn's and Wittgenstein's. We should take each of theses thinkers as emphasizing the importance of the role of practice in passing on traditions, but not as emphasizing its exclusive role. (I already argued against the latter point in chapter 4.) Furthermore, if we agree with their view that passing on knowledge cannot be done without showing its applications and how it works in practice, we must also recognize the role of theoretical factors. It is one thing to say that passing on knowledge involves showing its application and that acquiring an ability to follow a rule needs practice,

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[158] *Ibid.*, p. 53.

[159] *Ibid.*, p. 53.

quite another to say that giving rules in advance is always a hopeless task. Just the opposite, giving rules is as necessary for passing on knowledge as practice is. I would like to show this by briefly discussing education in general.

Education is passing on knowledge, in which rules play an important role. An ancient Chinese philosopher Meng Zi (371 BC.-289 BC.) says in the *Gao Zi Shang*: "A great master can give people rules but cannot have them be skilful". He holds that knowing a rule is one thing, skilfully following a rule is another, and that skill comes from repeated practice. This is similar to what Wittgenstein says. Meng Zi also says: "A great master must teach people by giving rules". This shows that for him the purpose of education and the way to approach it is to give rules. In fact, in many kind of arts education seems to involve giving rules first. For example, in Chinese traditional medicine there are many rules. Up to now, when people learn Chinese traditional medicine, the first thing they do is memorize these rules. At the beginning, neophyte doctors not only cannot use these rules, they do not understand them. Only after they can recite rules from memory, can they find out the use of those rules in observing their teacher's application, and then figure out what is the right way to use the rules. While it is true that they need practice to really grasp the rules, it is clear that knowing rules in advance is helpful for learning the art. In the education of modern science, students also are first given rules and theories, then practice. They are given examples, but the purpose of their teachers is to show the role of rules and theories in practice. It is true that a learner does not really understand a rule or theory until he can use it correctly, but it is also true that in the process of rule-learning a learner does not invent the rule or theory but learns how the master uses it. If a learner does not know which rule's application he is expected to see, he will hardly understand what the master is showing him. Thus knowing the rule in advance may well be helpful.

Now take Polanyi's own example of cycling. Learners at the beginning react to falling as they do in other activities. For example, when they fall during walking, they struggle to stand by moving in the direction opposite to the fall. And when learning bicycling they mistakenly turn the handlebars to the left when they start falling to the right. If there is nobody telling them the rule that "when you start falling to the right you should turn the handlebars to the right", they may find out this rule themselves by watching the master's performance, but this will usually take much longer. Here the guiding function of rules is clear.

In cycling there is a mathematical formula. Obviously, the typical cyclist does not know that adjusting the curvature of one's bicycle's path in proportion to the ratio of one's imbalance over the square of one's speed, or the various other factors to be taken into account in practice which are left out in the formulation of the rule. But knowing the rough rule new practitioners can concentrate on the main factors and try to have their reaction connect with their feeling of falling down. This shows that we should understand Polanyi's point that giving examples is the way for transmission of an art as emphasizing the role of practice, but not as precluding the role of theory. We should accept that practice is important as well as theory, rather than that we only need practice itself, i.e., we should recognize that in handing down tradition both practical and theoretical knowledge may play an important role.

In fact, in the transmission of an art, the practice of a new learner is not unguided. I mean that the condition of a new learner's practice usually is different from that of a person's who is not being taught the rule, the former being designed by a master in accordance with the teaching purpose. Only in such a context will new learners readily see the role of the rule, readily imitate the reaction that the master showed, and easily find out if he does it correctly. Thus, the role of the rule in the transmission of an art cannot be ignored.

Education in science provides an example. After a professor gives a theory or a rule and shows its application, he gives students assignments selected to reinforce the theory or rule. Students know that they are supposed to use what they have just been taught to do their homework. If they did not use it, they know that they did not do the right thing whether or not they obtained the right result. The practice in the transmission of an art is selected according to a theory or rule. Everybody knows what differences can emerge between two groups of students, when one group has a good teacher and the other does not. This partly reflects the fact that the teacher's understanding of rules influences their practice.

Second, the process of learning a rule is a process of interplay between understanding and using of rules. The practice of imitating examples can help people understand rules while they follow them. From the previous discussion, we can see that both Kuhn and Polanyi discuss passing on a tradition in the same way, namely by emphasising the role of practice. They rightly argue the point with regard to practice. But they overlook the fact that there are theoretical elements involved. In fact, learning to follow a rule, even a very simple one, needs feedback. For example, in art eduction, people are trained by following rules at the beginning. But even following a simple rule they need more than exercise. Consider the following example: the art of character penmanship. There are many rules to follow in Chinese character writing. One simple rule for writing one kind of character consists of two component parts (left and right roots) like the following:

讲 晓 课 动

The rule says: if the left part of the character is smaller than the right part, then write the left part higher than the middle position so that the two parts are even at the top of the character, as in the following:

呼　　峰　　吸

If the right part is smaller than the left part, then write the right part lower than the middle position so that the two parts are even at the bottom of the character, as in the following:

细　　知　　钿

This rule is used to train all people who study Chinese character penmanship. Although the rule is simple, to follow it is not easy. When one follows it, one must attempt to know how high (or how low) the part should be written. One has to understand what "even" means in this context. Moreover, the purpose of following this rule is to write a character beautifully. So the leaner needs feedback from exercises of following this rule to adjust his action to the rule.

In the process of learning how to follow a rule, an exercise must be accompanied by deliberation. By saying this I am not suggesting that rule-following is a matter of following an interpretation; rather I am trying to distinguish rule-following activity from the similar activity that people do without following rules, and trying to emphasize that rule-following needs deliberation as much as it needs practice. Take scientific education as an example. As soon as somebody gives an example of how to apply a rule in science, learners acquire some idea about the rule itself. When a learner does his homework what he does is no longer the same as what he may do before he learns the rule. To obtain the ability to react to a rule rightly, he needs to grasp what the rule means in practice, and he must attempt to find out what he has done right and to repeat those actions until it becomes customary. But the process of learning the rule involves deliberation, and one's ability to follow it and one's understanding of it grow up together and are mutually supporting.

3. How Rule-following May Lead to New Results

Now let me turn my attention to the question of how rule-following, as well as tradition-following, may lead to new results. I suggest that rules do not entirely limit the activities of following them.

First, I want to suggest that rules and rule-following activity do not uniquely corresponded to each other. On one hand, there is not only one unique set of rules that fits an activity, i.e., people may pick up different sets of rules for a single activity. As I have already noted, Polanyi points out that when people learn a tradition some of them may learn some rules that even the master, who shows them examples of the tradition, cannot articulate. Following Polanyi, I suggest that this means for one activity, or one tradition, people may grasp it by grasping different sets of rules. This implies that following different sets of rules may lead to the same activity, i.e., same rule-following activities can be interpreted as based on different sets of rules.

On the other hand, people who follow the same rules may act in different ways. First, for most activities we need more than one rule to describe it and when people follow those rules they may act in different ways. For example, Wittgenstein mentions a rule-following activity, playing chess. In playing a game of chess, the players must agree on a set of rules. They follow the same rules, however they rarely get the same results. Moreover, even for a single rule people may obtain different results. In learning penmanship, as in the above example, although people follow one simple rule to write the same character, their writing styles will usually differ from each other; this depends on their so-called personal style.

In science, rules often seem to determine an activity strictly. But even in elementary mathematics, we often see that for one question people can solve it in different ways, while following the same rule or rules. For instance, in geometry, when calculating the area of a triangle, people can use different bases and heights while applying the same formula. In particular in many activities involving a set of rules, people may react in different ways. When they solve the same question they may appeal to rules in different orders. Noting this, we may say that rule-following may involve creation on the part of the practitioner.

Second, rule-following is not a mechanical activity that cannot lead to innovation; rather it may lead to new results. This is due to the nature of rule-following. Let me look into the creativity of a practitioner in rule-following. Given Wittgenstein's view about learning rules and Kuhn's view about learning a paradigm, it is clear that the creativity of the practitioner may play a role in rule-following. Wittgenstein clearly held that rule-following is not done by following interpretations, and that people learn rules by giving examples, while Kuhn claims that people follow a paradigm by finding the similarity between the question that they deal with and the exemplar given by the paradigm. This suggests that when people grasp a rule they grasp those characteristics that impress them most in watching the master's application of the rules, and when they follow the rule they attempt to find signs of those characteristics and react similarly. In a word, how they follow rules depends, at least partly, on the way they imitate their teachers. But it is obvious that this does not strictly limit their activities. It leaves room for them to create different ways of following rules. In other words, different people may find different similarities and do different things while following the same rules. Thus, while for one rule, they all expect the same result, when they follow a set of rules, or face new complex situations, they may be guided by different similarities and end up with different results.

This nature of rule-following is alluded by some philosophers. Some, such as Kuhn, argue that shared criteria are not enough to determine theory choice. Kuhn takes these so called "imperfections" "to be in part responses to the essential nature of science".[160] In my view, this is not an "imperfection" but something integral to rule-following. Thus, while I agree with Kuhn's view that theory choice needs both objective and subjective factors, I disagree with his further thesis of community authority. I leave this to be discussed in chapters 8 and 9, where I shall show that the choice of which rule to follow is not an arbitrary matter.

The central point that is being laboured in this chapter is that tradition-following and rule-following have some common aspects and they may involve criticism and creation, and since learning and following a rule are partly based on imitation, it is possible that they may lead to new results. Thus during scientific revolutions, as I will show later, the old tradition can play a role in forming the new one.

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[160] Kuhn, *The Essential Tension*, p. 330.

7

Tradition in Discovery and Justification

The role that old traditions play in the formation of new ones is a subject that we are at long last prepared to approach directly. I have said that Kuhn speaks interchangeably of tradition-based research and research done under a paradigm, and I have pointed out that while Kuhn is right to hold that an old tradition can make us aware of an anomaly, he is wrong in thinking that an old tradition is useless in the formation of a new one. Also, I have attempted to sort out Kuhn's ideas about the formation of paradigms, and I have pointed out that these ideas are overlooked by many people because Kuhn emphasizes the incommensurability of paradigms. Finally, in the last chapter I have shown that the activity of following tradition and rule-following activity have some common aspects. Now, it is time to address the question of the role of old traditions in the formation of new ones. I believe that this is the core estion concerning the role of tradition in science.

As I mentioned in chapter 5, a new tradition comes into being in the surrounding of the old tradition as a consequence of scientific discovery and theory invention. Therefore, to investigate the role that an old tradition plays in forming a new tradition is to investigate its role in scientific discovery and theory invention. In this chapter, I shall first argue that scientific discovery and theory invention cannot be considered apart from scientific justification and theory evaluation; I shall contend that discovery and justification are not independent of existing theories and methods (and hence not independent of existing tradition). Then, in the course of a discussion of Kuhn's views on education and normal science, I shall argue that it is important to note that scientists possess the ability to discover similarities between new phenomena and old ones. This is crucial for understanding the way in which existing knowledge serves as a basis for acquiring new knowledge. After this, I contend that scientists are able to proceed on the basis of models of existing knowledge in revolutionary science in much the same way as they proceed on the basis of paradigms in normal science, and thus that scientists can go far beyond existing theories in scientific revolutions.

1. The Distinction Between the Context of Discovery and the Context of Justification

Before turning to my main argument, I would like to clarify the distinction between the context of discovery and the context of justification. I do this not only because I am going to discuss the role of tradition in discovery and justification, but also because I want to point out something Kuhn missed when he discussed discovery.

First of all, I should note that the distinction between the context of discovery and the context of justification is now understood differently from how it was understood when people started to use these terms. Tracing the distinction between the context of discovery and the context of justification, we discover that when Hans Reichenbach first mentioned it, he did not attempt to use the distinction between the two to exclude discovery from philosophical analysis, as Popper suggests. On the contrary, what Reichenbach attempted to do is to distinguish what he wanted to deal with from the standard way in which philosophers go about understanding discovery, i.e., he wanted to consider the rational reconstruction of the processes of discovery. He believed that what happens in the processes of discovery is different from H-D (hypothetico-deductive) justification. He emphasized that what he wanted to consider is how scientists obtain hypotheses from facts rather than the opposite, which is what the approach based on the H-D method focuses on. I call attention to this point, because it seems that we still need to deal with the point that Reichenbach was concerned with.

It is true that Reichenbach says that "epistemology does not regard the processes of thinking in their *actual* occurrence; this task is entirely left to psychology".[161] But by saying this, he does not mean that philosophy of science should not investigate the processes of thinking; rather he means that philosophy of science should investigate it differently. He says:

> What epistemology intends is to construct thinking processes in a way in which they ought to occur if they are to be ranged in a consistent system; or to construct justifiable sets of operations which can be interrelated between the starting point and the issue of thought-processes, replacing the real intermediate links. Epistemology thus considers a logical substitute rather than real processes.[162]

He holds that this rational reconstruction "is not arbitrary; it is bound to actual thinking by the postulate of correspondence. It is even, in a certain sense, a better way of thinking than

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[161] H. Reichenbach, *Experience And prediction*, The University of Chicago Press, Chicago & London, 1938, p. 5, emphasis added.

[162] *Ibid.*

actual thinking".[163] In particular, what concerns him are the "inductive relations" between "known facts" and "new theory", i.e., he wants to consider how scientists rationally argue from the one to the other.

It is also true that in his later work Reichenbach says that "the act of discovery escapes logical analysis".[164] However, as the context in which he says this makes clear, his point is that there are no logical rules in terms of which a "discovery machine" could be constructed that would take over the creative function of the genius. He does not deny that there may be logical relations between the fact and the theory that the fact leads to. Furthermore, Reichenbach holds that it is a misunderstanding to suppose that there exists no logical relations between facts and theories but only guesses, and he insists that it is possible to give a logical interpretation of the hypothetico-deductive method.[165] He says:

> The scientist who discovers a theory is usually guided to his discovery by guesses; he cannot name a method by means of which he found the theory and can only say that it appeared plausible to him, that he had the right hunch, or that he saw intuitively which assumption would fit the facts. Some philosophers

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[163] *Ibid.*, p. 6. Richenbach wants to say that rational reconstruction is not done by ignoring actual thinking, rather it is done by replacing correspondently real thinking processes with constructed operations.

[164] H. Reichenbach, *The Rise Of Scientific Philosophy*, University of California Press, Berkeley and Los Angeles,1968. p. 231.

[165] For Reichenbach, "a logical interpretation of the hypothetico-deductive method" refers to the logical relation leading from the facts to the hypothesis.

> have misunderstood this psychological description of discovery as proving that there exists no logical relation leading from the facts to the theory, and they contend that no logical interpretation of the hypothetico-deductive method is possible. Inductive inference is for them guess-work inaccessible to logical analysis.[166]

It is clear that, on one hand, Reichenbach believes that there are no logical rules by the use of which the "discovery machine" can do what a genius does. On the other hand, he holds that there are logical relations between facts and the hypotheses about these facts. What Reichenbach means here by "logical relation" and "logical interpretation" certainly should not be understood in terms of formal logic but rather in terms of rationality. His rational reconstruction does not attempt to provide an algorithmic path from fact to theory.

According to Reichenbach, we should not think that the hypothesis of H-D approach only come from guesswork. He believes that those people who take it as such are wrong. By this, he means that "(deductive) logic only involves the context of justification", in which a logician explains facts by deduction from theory. On the contrary, the context of discovery refers to the building up of a theory, which cannot be explained by deductive logic, because the constructed theory can only be accepted when people can show that it is based upon an inference from facts to theory. He holds that "this inference

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[166] Reichenbach, *Experience And prediction,* see p. 230 and p. 231.

is not deductive, but inductive",[167] and suggests that the context of discovery should be investigated through a discussion of the inductive method. He does not make a distinction between the context of discovery and the context of justification by distinguishing between subjects that are properly discussed in philosophy of science and subjects that are not, nor by distinguishing between rational and non-rational activities. (He holds that discovery and justification are both rational activities and the subjects of philosophy of science.) Instead, he distinguishes the context of discovery and the context of justification by showing that they should be analyzed in different ways. When he says that discovery cannot be analyzed, he is actually emphasizing that there is no deductive way in which people can make a discovery by following certain deductive steps. While he certainly does not rule out guesses in the process of discovery, he emphasizes that a guess is not enough for scientific discovery. He says:

> [The] scientist who discovered his theory through guessing presents it to others only after he sees that his guess is justified by the facts. It is this claim of justification in which the scientist performs an inductive inference, since he wishes to say not only that the facts are derivable from his theory, but also that the facts make his theory probable and recommend it for the prediction of further observational facts.[168]

In this sense, he holds that justification cannot be separated from the process of discovery. Many philosophers have

[167] *Ibid.*, p. 230.

[168] See *ibid.*, pp. 230-231.

misunderstood Reichenbach's distinction; they understand him as holding that discovery should be excluded from philosophical analysis.

One may say that whether we misinterpret Reichenbach or not, our distinction still makes sense. What matters, it may be argued, is not whether philosophers' ideas about the distinction between the context of discovery and context of justification are the same as Reichenbach's or whether they are right to draw a support from Reichenbach, but the ideas themselves. Nor, it may also be insisted, is it important that some of them address the question of whether the process of the discovery and the process of justification of this discovery are taking place in succession. Some hold that discovery differs from justification as the empirical differs from the logical, and still others hold that discovery belongs to history, psychology and sociology of science, while justification belongs to the philosophy of science.[169] The important thing is that their consideration seems to be the same, namely they want to show that discovery should be ruled out in philosophy of science.

2. Kuhn's and Hanson's Attacks

This distinction is of course attacked by some philosophers, such as Hanson and Kuhn. Although both Hanson and

[169] See Andrew Lugg, "The Process of Discovery", *Philosophy of Science*, 52 (1985), pp. 207-220 particularly p. 219. Also see Paul Hoyningen-Huene,"Context of Discovery and Context of Justification", *Studies in History and Philosophy of Science*, 18, (1987)), pp. 501-515.

Kuhn argue against the distinction between discovery and justification, they do it in different ways and draw different conclusions. Paul Hoyningen-Huene classifies them into different groups. According to him, Hanson's attack is built on the idea that there are alogical aspects to discoveries, while Kuhn's is built on the idea that justifications have sociological and psychological aspects. He summarizes Kuhn's point as follows: "It is maintained that in the actual decisions concerning theory choice there are factors that play a role which can only be described in sociological or psychological terms."[170] I believe that for our discussion we should look more carefully into Kuhn's attack and compare it with Hanson's.

Since my present purpose is to determine the role of tradition in scientific revolutions, in what follows I shall restrict my attention to Kuhn's and Hanson's attacks on the distinction as it applies to such science. As I pointed out earlier, Kuhn rightly suggests that the discovery of scientific facts and the development of theory occur at the same time and that when we discuss discovery and justification, we should also discuss theory invention and theory evaluation. Now I want to suggest that the reason that Kuhn holds that the context of discovery and the context of justification cannot be separated is that he holds that new facts and the new theories support one

[170] Paul Hoyningen-Huene,"Context of Discovery and Context of Justification", *ibid.*, p. 508.

another.[171] But if this is his view, it is problematic since it is hard to believe that the justification for discovery that people require has this kind of circular structure. Moreover, as discovery is a process, the justification must also be a process, and before the new theory is established the justification must have already taken place, with many steps having been taken on the basis of existing theory. Even if Kuhn thinks that his view is only about Reichenbach's context of justification, his view is open to question.

Moreover, Kuhn not only does not answer the question of what happens in the discovery, he also draws the wrong conclusion. By this I mean that he separates the whole process of discovery from the result of it, i.e., he does not analyze what happens in the process of discovery. What he says about paradigm choice is only what happens after discovery or after the invention of a new theory (i.e., after paradigm has been established). As I mentioned above, Kuhn believes that the process of discovery and theory invention is a process of paradigm change. He does not seem to realize that this process is also a process of justification, and theory evaluation is not justified or evaluated by itself but in the context of the

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[171] Kuhn, *The Structure of Scientific Revolutions*, p. 94. He says: "(Paradigm) choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue. When paradigms enter, as they must, into a debate about paradigm choice their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense."

existing tradition.

Kuhn also seems to hold that after a new paradigm comes into being, there is no such thing as justification--only persuasion in the conversion of paradigms. He says that "paradigm change cannot be justified by proof".[172] Thus he says that

> Our concern will not then be with the arguments that in fact convert one or another individual, but rather with the sort of community that always sooner or later re-forms as a single group.[173]

This shows that Kuhn actually excludes what happens in the process of discovery from his discussion. Because most discoveries of scientific facts or theory inventions do not occur all at once but during the process of discoveries, the important steps of one discovery are usually done by the individual scientist (sometime different individual scientists), and thus he (or they) must convert other individuals. Kuhn does not, however, consider the conversion of individuals, and rules out the process of discovery or theory invention from his discussion. We must conclude then that he does not see the role of existing knowledge in the formation of new traditions.

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[172] *Ibid.*, p. 152. Kuhn also says: "Just because it is asked about techniques of persuasion, or about argument and counterargument in a situation in which there can be no proof, our question is a new one, demanding a sort of study that has not previously been undertaken, We shall have to settle for a very partial and impressionistic survey. In addition, what has already been said combines with the result of that survey to suggest that, when asked about persuasion rather than proof, the question of the nature of scientific argument has no single or uniform answer."

[173] *Ibid.*, p. 153.

Kuhn's approach leads him to the conclusion that paradigms are not commensurable (which I have discussed in chapter 5) and that theory choice has no "better criterion than the decision of the scientific group".[174] (This is the "community authority thesis", which will be discussed in chapter 9.) For the moment, I shall confront myself to compare Kuhn's attitude with Hanson's. I believe that although both attack the distinction between discovery and justification, their aims are different and they draw different conclusions.

Unlike Kuhn, Hanson emphasizes that his interest lies, not in the testing of hypotheses, but in the development of new theories. He says:

> [My] concern is not with the testing of hypotheses, but with their discovery. Let us examine not how observation, facts and data are built up into general systems of physical explanation, but how these systems are built into our observations, and our appreciation of facts and data.[175]

Hanson concentrates his attention on the process of the generation of hypotheses. He believes that "physicists do not start from hypotheses; they start from data. By the time a law has been fixed into an H-D (hypothetico-deductive) system, the really original physical thinking is over."[176] What he wants to know is the nature of such original physical thinking, namely

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[174] *Ibid.*, p. 170.

[175] N. R. Hanson, *Patterns Of Discovery*, Cambridge University Press, Cambridge, 1961. p. 3.

[176] *Ibid.*, p. 70.

"how laws are come by in the first place".[177] It is clear that Hanson attempts to investigate the real process of discovery itself, not its results.

I suggest that Hanson is concerned with the role of existing knowledge in the formation of new traditions. Firstly, he wants to find out the way in which physicists start from data given that observation is a theory-laden activity. It is obvious that before a new theory comes into existence, the observation is an existing-theory-laden activity. Thus the data is expressed in the existing context. It is of course possible that the data can be explained by or built into the new theory later on, in some people's mind, the data embody as yet unknown theory. However, until the physicist finds a new hypothesis, these data are stated and analyzed by using existing theories, namely within the existing tradition. Thus, the process of discovery is a process that starts from the data based upon existing theory and ends up in certain cases in the establishment of a new tradition.

Secondly, not only are the data stated by the existing tradition, the ways in which scientists reason to new hypotheses are also based on the existing traditions. In Hanson's words, there is a continuity in science from earlier times to later. Hanson begins by distinguishing the two different kinds of reasons, reasons for accepting an hypothesis after it is formulated (which is Kuhn's point) and reasons for proposing or

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177 *Ibid.*, p. 70.

for trying an hypothesis in the first place.[178] Then he focuses on reasons for proposing an hypothesis. He claims that while developing new ideas may require genius, this cannot mean that the reflections leading to these ideas are unreasonable or a-reasonable, because "here resides the *continuity in physical explanation from the earliest to the present times*".[179] It is clear then that for Hanson the reasoning for proposing a new hypothesis is done in the context of the existing tradition and that there is a continuity in knowledge.

Now, let me summarize what I have done and draw a conclusion. After discussing Reichenbach's distinction, I compared Kuhn's and Hanson's attacks on this distinction. This led us to the point that we should not simply group viewpoints of philosophers by checking whether they draw the distinction; instead we should examine their points of view about the process of discovery itself. For example, as we saw above, Reichenbach holds that there is a distinction between the context of discovery and the context of justification, yet he holds that the process of discovery can be rationally reconstructed. (Doing this includes looking at the process of discovery as a process in which the scientist tries his best to justify his

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[178] Hanson says: "H-D (hypothetico-deductive) accounts ... suggest that the fundamental inference is from higher-order hypotheses to observation statements. This may be a way of setting out one's reasons for accepting an hypothesis after it is got, or for making a prediction, but it is not a way of setting out reasons for proposing of for trying an hypothesis in the first place." *ibid.*, p.71.

[179] *Ibid.*, p. 72, emphasis added.

discovery or theoretical invention.) We also saw that while Kuhn attacks this distinction, he holds that discovery and theoretical invention cannot be justified. In fact, he distinguishes the two contexts and emphasizes that the discovery is only a matter of psychology and sociology. Thus, the issue is whether discovery and invention relate to justification and theory evaluation and how they relate to existing knowledge. To my way of thinking, discovery and theory invention cannot be separated from justification and theory evaluation, and they both involve the use of existing knowledge. To demonstrate this point we must first consider how scientists go about their inquiries.

3. Kuhn on Education

Kuhn discusses this topic by examining the nature of scientists' education. I believe that what Kuhn said about education is helpful for our discussion and that by examining Kuhn's point of view about scientists' education we can understand what makes him take his position about the distinction between the two contexts. Besides, I hope that through the following discussion we can understand how established tradition can play a role in discovery and justification.

I mentioned above how it seems strange that Kuhn attacks the two-contexts distinction and emphasizes the importance of adhering to a tradition in science, while ruling out the role of existing knowledge in discovery and justification, i.e., the role of old tradition in the formation of new. What makes Kuhn take this position? I suggest that he does so because of his view about the nature of education. In what follows, I shall show that Kuhn rightly suggests that to learn something is to acquire an ability to discover similarities, but he overlooks the existence of an important sort of similarity and thus overlooks the important ability that scientists have of devising new theories using existing knowledge.

To start with, I first draw attention to Kuhn's discussion about the nature of education. After discussing what happens in the development of science, Kuhn says:

> Nothing in the preceding discussion proves that there is no set of correspondence rules adequate to explain the behaviour of the community under study ... But the discussion may lead us to take a bit more seriously some aspects of scientific training and behaviour that philosophers have often managed to look right through.[180]

Kuhn goes on to claim that "very few correspondence rules are to be found in science texts or science teaching" and asks "how can the members of a scientific community have acquired a sufficient set [of rules]?" He believes that there must be something else playing a role in scientific inquiry and suggests that during education, scientists not only learn but, more importantly, acquire "a learned similarity relationship", "an acquired ability to see resemblances between apparently disparate

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[180] Kuhn, *The Essential Tension*, p. 305.

problems".[181] It is this ability that "play(s) in the sciences a significant part of the role usually attributed to correspondence rules".[182]

Kuhn explains this by the example of a child's learning process. He believes that this example supports his view about the importance of paradigms.[183] For him, in normal science, scientists draw on this ability to solve problems, his view being that once a new problem is seen to be analogous to a problem previously solved, scientists will imitate the previous problem solution to solve the new one. I believe that Kuhn is right to think that this ability can play an important role in normal science, and I will come back to it in chapter 9 where I consider his community authority thesis.

4. What Kuhn Missed

Here I shall argue that Kuhn overlooks something important that occurs in the education in science, namely that scientists also acquire the ability to see similarities between paradigms, and I suggest that this ability plays a role in scientific revolutions.

To demonstrate this, I would like to make use of Kuhn's

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[181] See *ibid.*, pp. 304-306.

[182] *Ibid.*.

[183] See *ibid.*, p. 313. Kuhn says: "Johnny's case should suggest why I continue to insist that shared examples have essential cognitive functions prior to a specification of criteria with respect to which they are exemplary."

example of the child, whom he calls Johnny, learning how to identify swans, geese, and ducks.[184] Using this example, Kuhn attempts to show that there is another way, different from using rules, to acquire and store knowledge. In the course of their education, without definitions or correspondence rules, says Kuhn, people employ a learned but nonetheless primitive perception of similarity and difference. Therefore, the knowledge they learn through acquiring the perception are embedded, not in generalizations nor rules, but in the similarity relationship itself. I believe this is right.

Now let us imagine that Johnny has grown up and has learned how to identify the paradigms of science, the paradigms of art, and the paradigms of history. Using Kuhn's example, we can say, as Kuhn says, that definitions "may play a role, but they need not". We "shall for the moment omit them from consideration", our "object being to isolate a different mode of learning in its purest form". Johnny's education then proceeds as follows. Johnny's science teacher teaches him a paradigm of an individual science--say Newtonian mechanics--by saying in effect, "Look, Johnny, there's a paradigm of science, a way to do science." Johnny assimilates the idea of a scientific paradigm both by learning more and more scientific paradigms and by learning different things elsewhere, such as in art and history classes. After that, Johnny himself has learned the similarity among the paradigms of science and the difference between the paradigms of

[184] See *ibid.*, pp. 309-318.

science and the paradigms of art or history. In the end, Johnny's ability to identify these paradigms of knowledge is as great as his teacher's. He identifies these paradigms not by the definitions but by reference to the similarity relationship itself.

I believe that this description can cover much, if not all, of learning. Today, many people can point to the examples of science, art, and history without much trouble, yet philosophers of science and philosophers of art have a hard time drawing boundaries between them, and many philosophers have now given up trying. If this is so, the knowledge about paradigms of science, art, and history must be embodied, not in generalizations or rules, but in the similarity relationship itself. In other words, people know paradigms of science by knowing the similarity among the paradigms of science, and the difference between paradigms of science and the paradigms of other forms of knowledge.

From this, I draw the conclusion that during their education, scientists not only acquire the ability to see the similarity between problems, but also acquire the ability to see the similarity between the individual sciences. Kuhn suggests that the former plays an important role in normal science. However, he overlooks the latter. For example, he holds that a paradigm is the example of practice or the solution of a previous problem. To explain how paradigms play this role in normal science, he says:

> Once a new problem is seen to be analogous to a problem previously solved, both an appropriate formalism and a new way of attaching its symbolic consequences to nature follow. Having seen the resemblance, one simply uses the attachments that have proved effective before.[185]

Thus, we can say that his notion of paradigm is based on the acquired ability to see the similarity between the problems. But he never mentions the similarity between paradigms. On the contrary, he emphasizes the "incommensurability" of paradigms. Because of this, Kuhn does not see, I shall argue, the role that a previous paradigm can play in the scientific revolution.

What Kuhn should have said, I think, is that scientists take advantage of this latter ability as well as taking advantage of the former one. I mean that just as scientists use the solutions of typical problems as paradigms, and solve new problems by imitating the solutions to these typical problems in normal science, they also use paradigms of individual sciences as examples of science inquiry, and deal with crises in scientific revolutions by imitating previous paradigms, i.e., they form new paradigms by imitating the previous paradigms.

What Kuhn says about imitating the examples in normal science in the quotation just given also applies to the relationship between a new problem and a problem previously solved. Scientists use the attachments (which connect symbolic consequences of a formalism to nature) that have proved effective before. In scientific revolutions they imitate the

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[185] *Ibid.*, p. 306.

previous paradigms and attempt to find analogies between a new area and an area previously dealt with, exploiting the attachments that have proved effective in the already familiar area. (I will come back to this point later on.)

If this is true, we can say that paradigms play a role in innovation as well as in normal science. Since a paradigm is properly understood as a tradition, as I have shown in chapter 4, we can say that tradition can play a role in innovation too. I conclude therefore that scientists are able to proceed on the basis of models of existing knowledge in revolutionary science in the same way as they proceed on the basis of paradigms in normal science. In this sense, tradition plays a role during the shift from one kind of normal science to another, no less than in normal science itself, and justification in the case of new paradigms has the same nature as justification in normal science. It is not just a psychological process; it also has logical aspects.

Since Kuhn overlooks the ability to see similarities between paradigms, he fails to see the role of tradition in revolutionary science and thinks that there are no logical aspects to the process of theory invention. For him, the process of theory invention is a subject for psychology, and he attacks the two-contexts distinction conceived as distinguishing the logical from the empirical. Taking the two-context distinction to be based on the distinction between the empirical and the logical, he ends up claiming that justification in theory choice is a sociological or psychological matter.[186]

What I have been suggesting is that because Kuhn overlooks one important ability required in science education, he attacks the distinction between the context of discovery and the context of justification and rules out of the discussion the logical aspects of the process of discovery and the role of an existing paradigm in formation of new paradigms. Had he attended more closely to the nature of scientific eduction, he would have realized that tradition can play a role in scientific revolutions, there being an important parallel between the ability acquired to see the similarity of problems and the ability acquired to see the similarity of paradigms.

5. A Case Study

In what follows, I shall illustrate this point by considering a particular case in the history of science. I shall attempt to show how tradition plays a role in the discovery of fact, theory invention and evaluation, and I shall argue that discovery and justification cannot be separated, as both depend on the existing tradition and have logical aspects. I shall also show that scientists take advantage of the ability to see the similarity between paradigms and stress that scientists actually justify and evaluate theories in the process of discovery and theory invention using existing traditions.

Let us look at James Clerk Maxwell's work. As I mentioned

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[186] Cf Hoyningen-Huene, *op. cit.* pp. 501-515.

in the previous chapter, Maxwell used existing physical theory when developing his theory. Now let us examine this more carefully. First, I would like to point out that Maxwell was conscious of using existing theories in a particular way, i.e., he knew that what he was dealing with was not in the domain of the theory he used, but he believed that the old theory was helpful in a certain way, namely as a source of conceptions and analogies. He used the existing theory as an example to imitate. When Maxwell attempted to find a method to deal with a new area, he believed that scientists should make use of the existing tradition. He knew clearly that the existing theories cannot and should not be simply adopted in a new area, but he believed that we can and should obtain what he called "physical ideas" from existing theories. He says:

> In order to obtain physical ideas without adopting a physical theory we must make ourselves familiar with the existence of physical analogies. *By a physical analogy I mean that partial similarity between the laws of one science and those of another which makes each of them illustrate the other.*[187]

It is obvious that the method Maxwell refers to is to find out the similarity between the laws of one science and those of the other, so that conceptions can be borrowed from the old theory and even used to illustrate the new theory. In a later address Maxwell emphasizes this method to scientists.

> The mathematical processes and trains of reasoning in one science resemble those in another so much that his

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[187] J. C. Maxwell, *The Scientific Papers of James Clerk Maxwell* edited by W.D. Niven, Dover Publications Inc., New York, 1965, vol. I, p. 156, emphasis added.

knowledge of the one science may be made a most useful help in the study of the other.[188]

What Maxwell takes to be knowledge of a science is in fact very like what Kuhn takes to be a paradigm. He believes that by attempting to acquire and develop explanations of phenomena the scientist should "try to understand the subject by means of well-chosen illustrations derived from subjects with which he is more familiar."[189] This indicates that Maxwell holds that not only can scientists borrow concepts from old theories, they can also imitate the form of illustrations presented by old traditions. More to the point, Maxwell did this when he invented his new theory, the laws of electrodynamics, which led to a new tradition.

Maxwell's tradition is different from Newton's.[190] However, Maxwell builds his theory of electric and magnetic fields by finding the similarity between it and the Newtonian tradition. It is obvious that Maxwell used Newtonian theory differently from the way in which scientists use it in normal science. In what follows, I will show that Maxwell is not using existing theory (the law of fluid mechanics, which obviously belongs to the Newtonian tradition) as a paradigm to solve a problem in that area. Rather, he is using a paradigm theory as an example

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[188] *Ibid.*, vol. II, p. 218, emphasis added.

[189] *Ibid.*, vol. II, p. 219.

[190] Einstein says that Newton starts a tradition which took the world to comprise space and separated particles while Maxwell starts a tradition which takes it to comprise matter and continuous fields.

of a form of knowledge to find another paradigm.

Let us find out what actually happens in the process of Maxwell's theory invention. We see that he follows the method he described. Moreover, Maxwell's theoretical invention goes hand in hand with his justification, according to which existing knowledge plays an important role.

The invention of Maxwell's theory was a long process. There are three important papers (1856-1865) which present three steps in his invention and at each step Maxwell gives a reason for moving forward. The first paper is "On Faraday's Lines of Force" (1856), in which Maxwell sorts out a mathematical language used for existing electromagnetic theories and draws an analogy between electromagnetic and mechanical phenomena. Having discussed how he intended to proceed, Maxwell says:

> We may however obtain a different view of the subject, and one more suited to our more difficult inquiries, by adopting for the definition of the forces of which we treat, that they may be represented in magnitude and direction by the uniform motion of an incompressible fluid.[191]

As he states, he is not postulating an hypothetical fluid to explain actual phenomena; he is merely considering a collection of imaginary properties. Still, Maxwell uses the basic principle of fluid mechanics to obtain the crucial equation, $p=kS/4\pi r$, which describes the resulting pressure at r for a source formed by the coalition of S unit sources. (Here p is

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[191] Maxwell, *The Scientific Papers of James Clerk Maxwell*, vol. II, p. 155.

pressure, k is the impedance coefficient of medium, r is the distance, and s is the number of unit sources.) This is similar to U=e/r, where U is the electric potential, e is the electric charge, and r is the distance. Next, Maxwell uses the mathematical description of fluid flow to describe electromagnetic phenomena and to develop the basic equations about static electric fields, stable electromagnetic fields and instantaneous electromagnetic fields.

In this paper, we can see that Maxwell is not simply gathering existing knowledge about electromagnetism. Indeed, he is trying to find a new form to describe electromagnetic phenomena. This kind of work is a very important part of Maxwell's theoretical investigations. In doing this work he uses an analogy between electromagnetism and fluid flow. He then uses fluid mechanics to determine what further conclusion we can obtain. Since he clearly knows what he is dealing with is not a fluid and uses fluid mechanics in a different way, fluid mechanics is functioning as an analogy. Furthermore, Maxwell recognizes where the analogy is weak. He says:

> The idea of the electric-tonic state, has not yet presented itself to my mind in such a form that its nature and properties may be clearly explained without reference to mere symbols, and therefore I propose in the following investigation to use symbols freely, and to take for granted the ordinary mathematical operations. By a careful study of the laws of elastic solids and of the motions of viscous fluids, *I hope to discover a method of forming a mechanical conception of this electro-tonic state adapted to general reasoning.*[192]

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[192] *Ibid.*, vol. I, pp. 187-188. emphasis added.

Here Maxwell is pointing to a weakness that he should work on in the process of justification. Clearly this would help him achieve his aim, namely, to discover a mechanical conception for his theory. It is no doubt that what he is doing is preceding in accordance with the principles of the previous paradigms.

In the second paper, "On Physical Lines of Force" (1861-1862), Maxwell puts forward a new model, one that derives from his analysis of phenomena by imitating mechanics. Attempting to form a mechanical conception of electro-tonic states, Maxwell inquires into the physical connection of molecular vortices and electric current. "Assuming that our explanation of the lines of force by molecular vortices is correct", he continues, "why does a particular distribution of vortices indicate an electric current? A satisfactory answer to this question would lead us a long way towards that of a very important one, 'what is an electric current?'"[193] He then puts forward an "idle wheel" model in which electric currents are taken as phenomena of translation and magnetism is taken as a phenomenon of rotation. When he gives the reason for this, he says:

> The transference of electrolytes in fixed directions by the electric current and the rotation of polarized light in fixed directions by magnetic force, are the facts the consideration of which has induced me to regard magnetism as a phenomenon of rotation, and electric currents as phenomena of translation.[194]

In short, he reasons towards this model using his knowledge of

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[193] *Ibid.*, vol. I, p. 468.

[194] *Ibid.*, vol. I, p. 505.

mechanics.

This model not only explains the phenomena; it also becomes a tool of invention for the conception of "electric displacement". In part III of the paper, Maxwell first compares the different results of electromotive force in the conductor and in insulators such as the surrounding medium. He then uses this model to determine what happens in the insulators when there is an electromotive force. Drawing an analogy between conductors and insulators, Maxwell puts forward the conception of "electric displacement". He imagines that when electric particles are urged but cannot be propagated through insulators, they will distort each magnetic medium cell. As the force is removed, the cells will recover their form and the electric particles will return to their former position. He says:

> This action on the whole dielectric mass is to produce a general displacement of the electricity in a certain direction. This displacement does not amount to a current, ... but it is the commencement of a current.[195]

Next, employing further principles of mechanics, he builds the equation of electromagnetic theory in which "electric displacement" is included, which in the third paper will become part of the total current. Here, Maxwell introduces the idea of "the relation between electric displacement, true conduction, and the total current, compounded of both."[196] This shows how the process of discovery and justification can help each other.

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[195] *Ibid.*, vol. I, p. 491.

[196] *Ibid.*, vol. I, p. 534.

On the one hand, Maxwell argues that this model can explain the phenomena; on the other hand he uses this model to draw a new conception of electric displacement. All these occur in the cause of invention of the theory and all are rooted in existing knowledge.

Maxwell had already compared light and the vibrations of an elastic medium in his first paper (1856). In this paper he had suggested that

> The other analogy, between light and the vibrations of an elastic medium extends much farther, but, though its importance and fruitfulness cannot be over-estimated, we must recollect that it is founded only on a resemblance in form between the laws of light and those of vibrations.[197]

Now in his third paper, "A Dynamical Theory of The Electromagnetic Field" (1861), he draws an analogy between electromagnetic phenomena and vibrations of an elastic medium--the ether--and calculates the velocity of transverse undulation which equals 314,858,000,000 m/sec. Then he says;

> The velocity of transverse undulations in our hypothetical medium, calculated from the electro-magnetic experiments of M. Kohlrausch and Weber, agrees so exactly with the velocity of light calculated from the optical experiments of M. Fizeau, that we can scarcely avoid the inference that *light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena.*[198]

Thus, reasoning by analogy, Maxwell concludes that the electric-magnetic medium is the same medium as that involved in the

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[197] *Ibid.*, vol. I, p. 156.

[198] *Ibid.*, vol. I, p. 500.

transmission light. Finally, he takes one more step:

> The agreement of the results seems to show that light and magnetism are affections of the same substance, and that light is an electromagnetic disturbance propagated through the field according to electromagnetic laws.[199]

At this point, Maxwell can be said to have invented his theory. It is obvious that his justification and theory invention go together, at each step justification and evaluation guide the further progress of discovery and invention.

In Maxwell's case, we see how a scientist can take advantage of the ability to see the similarity between paradigms. Maxwell gains this ability from his education and scientific practice. He calls this ability being "familiar with the existence of physical analogies" and suggests that given this ability one can find the "partial similarity between the laws of one science and those of another". In his own case, he believes that because electromagnetic phenomena are partially similar to the phenomena of fluid, the laws of the two must be partially similar.

Kuhn describes how scientists take advantage from the acquired ability to see resemblances between problems in normal science as "having seen the resemblance, one simply uses the attachments that have proved effective before."[200] What Maxwell did is very similar to what Kuhn described. The difference is that Maxwell takes advantage of the ability to see the

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[199] *Ibid.*, vol. I, p. 580.

[200] Kuhn, *The Essential Tension*, p. 306.

resemblances between different paradigms in a paradigm change. What he did was to imitate the paradigm of fluid mechanics to establish the laws of another science. This case shows that traditions also play a role in the revolutionary science.

One may say that this argument is based on the analysis of the papers where Maxwell publishes his new theory, and point out that the manner of reasoning described in the paper may be different from the way in which he actually constructed his theory. To answer this criticism, I would like to make two points. First, not all ways in which scientists describe their theories must be different from the way of finding them. If a scientist's demonstration is built on the new theory and reasoning from theory to facts, then his way of demonstration is of course different from the way of finding this theory. But if a scientist's demonstration is built on the existing tradition and reasoning from phenomena to a new theory, his way of demonstration may reflect the way he found his idea.[201] In fact, Maxwell's reasoning is the one in the foundation of a new theory. It is not at all like what Kuhn suggests: the role of paradigms in Maxwell's choice of paradigm was not "necessarily

[201] I believe that this is why Reichenbach holds that the way in which "a physicist [publishes] his logical reasoning in the foundation of a new theory" would almost always correspond to Reichenbach's concept of rational reconstruction, i.e., it is almost the way in which thinking processes ought to occur. In other words, it is correspondent to the actual thinking. Cf. Reichenbach, *Experience and Prediction*, p. 6.

circular".[202]

Second, Maxwell's three papers are written over a long period of time (nine years). In the three papers, we can see the development of his ideas. Some of his ideas and methods are announced in advance and developed later; some notions, such as that of total current and electromagnetic field, are not clearly drawn at the beginning and become perfected only later on; some models are employed to advance his reasoning and later, after he obtains new notions, abandoned. All these suggest that these three papers reflect the actual way Maxwell found his idea.

I have shown what happened in the invention of Maxwell's theory, one of the most important revolutionary developments in the history of science. Maxwell borrowed some conceptions from the existing theories and drew an analogy between electromagnetism and fluid mechanics, then, imitating the existing theory, he developed a new model through which he approached his new theory. In the process, we can clearly see that justification and invention went hand-in-hand. By illustrating and evaluating what he obtained from existing theory at each step, Maxwell determined how to go on to the next one.

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[202] Kuhn says: "When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense." *The Structure of Scientific Revolutions*, p. 94.

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Tradition and Reasoning

In the last chapter I discussed the role of tradition in discovery and justification. In this chapter I will discuss tradition and reasoning. As I mentioned in the last chapter, theoretical invention itself involves justification, which is a process of reasoning. Thus, we can anticipate overlap between this chapter, in which the focus will be on reasoning, and the last one. However, here I am going to pay more attention to the relation between tradition and rationality. While it is commonly thought that these are in conflict, I believe that rationality is itself based on tradition and, conversely, that scientific traditions are themselves a result of reasoning. In this chapter I will first discuss whether following a tradition is a rational activity. Then I will discuss why we should think of changes of tradition as reasonable affairs. After that, I shall say something about the way in which scientists reason during revolutionary periods when traditions are changed.

1. Tradition and Rationality

Rationality was first understood as algorithmic, then as a matter of method, and now merely as reason. I shall show in this chapter that rationality cannot separated from objectivity. Stephen Toulmin rightly points out that rationality has much to do with how we change our beliefs and is not simply a matter of how logical ingenuity can enable us to stick to our most basic beliefs.[203] In accordance with this view, I will discuss rationality in the context of revolutionary science and concentrate on the role that tradition plays when we gain new knowledge.

Some people claim that following a tradition is not rational activity. They have a number of reasons for saying this.

First, they take rationality to be a matter of reasoning by

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[203] Cf. Thomas Nickles, "Introductory Essay", in *Scientific Discovery, Logic, and Rationality*, ed. by Thomas nickles, D. Reidel Publishing Company, Boston, 1978, p. 40.

individual persons. Thus, it is widely supposed that an individual person's accepting a tradition without deliberating over its merits is irrational.

Second, some people appeal to the origin of traditions to question the rationality of tradition. For example, J.G. A. Pocock says:

> A tradition ... may be thought of as an indefinite series of repetitions of an action, which on each occasion is performed on the assumption that it has been performed before; its performance is authorized--though the nature of the authorization may vary widely--by the knowledge, or the assumption, of previous performance. ... Such a tradition is without a conceivable beginning; each performance presupposes a previous performance, in infinite regress. Furthermore, it may well be that it is the assumption, rather than the factual information, of previous performance that is operative; each action provides the grounds from assuming that it had a predecessor. Traditions of this kind, then, are immemorial, and they are prescriptive and presumptive.[204]

Third, some people take tradition to be irrational since the criterion of "rationality" itself changes from time to time. For example, Samuel Coleman says:

> Traditional practices can be shown to have "reason": they provide satisfactions to some, at least. Yet, in time, they usually fall short of the criterion of "rationality" made more volatile by our racing science and technology. ... [because] "rationality" is itself relative to the time, culture, place, state of knowledge, and so on. The tradition as it is practised is very likely to be a makeshift, an adaptation, and not optimally efficient for its

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[204] J.G. A. Pocock, "Time, Institutions And Action: an essay on traditions and their understanding", in *Politics and Experience*, edited by P. King and B. C. Parekh, Cambridge University Press, Cambridge, 1968, p. 212.

practitioners.[205]

For these people, tradition is opposed to science, since tradition-following is opposed to rule-following, and they take tradition to be opposed to rationality. I shall argue against this view. In what follows, I shall first discuss the view that tradition is integral to science. Samuel Coleman in his article "Is There Reason in Tradition?" says:

> Since ongoing [traditional] practices are adapted, it may often be the case that although there is reason in the practice, it is not "rational", if by "rational" one means greatest output to input ratio in satisfying culturally determined wants.[206]

For him, reason only plays a role within a tradition and its only purpose is to satisfy the wants of its practitioners. He says:

> The reason in a traditional practice ... is a function of the extent to which that practice meets the wants of its practitioners.[207]

Thus, for him, reason refers not to objective causes and grounds, but rather to the practitioners' subjective desires. He supports his claim that the role of tradition is not rational but a "makeshift", by appealing to the relativity of rationality.[208]

Feyerabend's view of tradition in science is similar. He

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[205] S. Coleman, "Is There Reason in Tradition?", in *Politics And Experience*, edited by P. King and B. C. Parekh, Cambridge University Press, Cambridge, 1968, P. 282.

[206] *Ibid.*, p. 282.

[207] *Ibid.*, p. 281.

[208] Cf.*ibid.*, p. 282.

suggests that people can justify events in one tradition but there is no criterion that they can use to judge the relative merits of traditions. For him, criteria seem to be "makeshifts", the fitness of which is constituted by research. He says:

> All we have is the process of research and, side by side with it, all sorts of rules of thumb which *may* aid us in our attempt to further the process but which may also lead us astray. (What are the criteria that inform us that we have been misguided? They are criteria which seem to fit the situation at hand. How do we determine fitness? We *constitute* it by the research we do: criteria do not merely judge events and processes, they are often constituted by them and they must be introduced in this manner or else research could never get started: AM. 26).[209]

Besides, for him, the relation between rationality and objectivity is unimportant.

I suggest that Coleman and Feyerabend are right to think that standards of rationality are relative, but they are wrong to think that tradition provides no help in the judgment of "events and processes". There are many things in tradition that are a result of reasoning; and people are influenced by a tradition of rationality so deep that no one can judge anything without it. Let me deal first with the irrationalist's position.

2. Feyerabend's Procedure

While Feyerabend claims to reject rationalism, what he actually does is rather different. For far from avoiding the

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[209] Feyerabend, *op. cit.*, p. 283.

rationalist's methods, he uses the same general procedure as the rationalist. For him "to be rational in the formal sense ... means to follow a certain procedure",[210] and "being rational or using reason means using these ways [tradition-independent ways of finding truth] and accepting the results".[211]

Keeping these criteria in mind, we may compare Feyerabend's method with the critical rationalist's. What we find is that they are very similar. Feyerabend follows a similar procedure to that of critical rationalists, i.e., he too proceeds rationally. Feyerabend says that what he is attempting to do is to prove that any method of science is limited. He holds that if he can show this--that all methods are limited-- he can say that none is rational. He says:

> My intention is... to convince the reader that *all methodologies, even the most obvious ones, have their limits*. The best way to show this is to demonstrate the limits and even the irrationality of some rules which she, or he, is likely to regard as basic. In the case of induction (including induction by falsification) this means demonstrating how well the counter inductive procedure can be supported by argument. Always remember that the demonstrations and the rhetorics used do not express any "deep convictions" of mine. They merely show how easy it is to lead people by the nose in a rational way. An anarchist is like an undercover agent who plays the game of Reason in order to undercut the authority of Reason (Truth, Honesty, Justice, and so on).[212]

But this is not significantly different from what Popper argues

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[210] Feyerabend, *Farewell to Reason,* p. 10.

[211] *Ibid.*, p. 8.

[212] Feyerabend, *Against Method*, Third edition, Verso, New York, 1993. p. 23.

in the case of scientific theories.

As a critical rationalist, Popper's procedure is to find errors for any hypothesis or theory of science. Although Feyerabend deals with method rather than theory, he seems to be committed to a similar procedure. If we note that, like a theory of science, a method has to be examined to determine whether it is scientific or not, we will see that what Feyerabend thinks about method is exactly what Popper thinks about it. Whereas Popper takes an error in a theory to show it to be unscientific, Feyerabend takes a limit in a method to show that it is not rational. Feyerabend's approach is thus similar to Popper's. While he holds that he differs from the rationalist, he has, like Popper, adopted a "special and standardized way of arguing".[213]

3. Feyerabend's Shifting Standpoint

Let me now turn to the second point. In what follows I attempt to show how we should understand rationality by analyzing Feyerabend's shifting standpoint. I attempt to show that rationality cannot be separated from objectivity.

I suggest that Feyerabend shifts his standpoint when he argues against rationalism. For example, when Feyerabend argues against "Reason", which he takes to be one of the rationalist's weapons, he seems to use another weapon of the rationalist--Objectivity--one that he also rejects. At the

[213] Feyerabend, *Farewell to Reason*, p. 8.

beginning of his book, *Farewell to Reason*, he says:

> More especially, I shall criticize two ideas that have often been used to make western expansion intellectually respectable---the idea of Reason and the idea of Objectivity.[214]

It is obvious that Feyerabend takes both the idea of Reason and the idea of Objectivity as his targets. But when he criticizes the idea of Reason, he relies on the idea of Objectivity. For example, when he attempts to criticize the idea of Reason by saying that the events and results that constitute science have no common structure, he uses the idea of Objectivity to prove it. He says:

> Successful research does not obey general standards; it relies now on one trick, now on another, and the moves that advance it are not always known to the movers.[215]

In his mind, there are no reasons that we can rely on since it is an objective fact that there is no element that occurs in every scientific investigation.

I would like to make two points about this. First, the idea of Reason does not presuppose that there is only one common structure of scientific reasoning; it does not stand opposed to there being different sorts of scientific reasoning. It is one thing to say that there is no one sort of reason that one can appeal to in all contexts, quite another to say that there are never appropriate reasons in any context. Although Feyerabend is right to maintain that there is a variety of scientific ways

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[214] *Ibid.*, p. 5.

[215] *Ibid.*, p. 281.

of reasoning, he does not establish his main view that there is no reason we can rely on. Secondly, when Feyerabend argues that there is a variety of styles of scientific reasoning, what he offers as support for his preferred doctrines is based on the idea of Objectivity, which he rejects.

Here, it is important to notice that for Feyerabend the cause of variety of scientific reasoning is objective. When he says "the moves that advance it [successful research] are not always known to the movers",[216] he means that since movers do not know that they move from one trick to another, their research is not rational. But this is to say that there is something--other than the mover's subjective desire--determining this person's moves, something not known by him or her and having nothing to do with his or her desires.

Moreover, when Feyerabend wonders "what can we do for scientists and why can we only do that", he is, in fact, talking about how we should deal with this topic in philosophy of science, i.e., about what method we should take and why we should adopt it when doing philosophy of science. He says:

> The most we can do for them [i.e., scientists] from afar is to enumerate rules of thumb, give historical examples, present case studies containing diverging procedures, demonstrate the inherent complexity of research and so prepare them for the morass they are about to enter. Listening to our tale, scientists will get a feeling for the richness of the historical process they want to transform, they will be encouraged to leave behind childish things such as logical rules and epistemological principles and to start thinking in more complex ways--and this is all

[216] *Ibid.*, p. 281.

we can do.[217]

This is, he says, *"because of the nature of the material"*.[218] But if this is right, the method which we should use in philosophy of science is objectively determined by the nature of this discipline itself. If we do not use it, we will fail for the same reason. Feyerabend says that "it is impossible to climb Mount Everest using the style of classical ballet."[219] Thus, the reason that the structure of scientific reasoning is so variable is that there are many different kinds of things in the world. According to rationalism, the scientific method is objective. No doubt the most important quality of the objectivity is that the methods are objectively decided by the nature of objects, not by human desires or wishes.[220] It thus seems to me that when Feyerabend criticizes the idea of reason, he tacitly accepts the idea of Objectivity and uses it as his weapon to argue against rationalism. In short he shifts his standpoint. The idea of reason and the idea of objectivity cannot be separated from each other, and Feyerabend cannot reason that there is no Reason by appealing to the objectivity of the nature of material.

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[217] *Ibid.*, p. 281.

[218] *Ibid.*, p. 281.

[219] *Ibid.*, p. 281.

[220] Cf. *ibid.*, p. 5, where Feyerabend contends that "to say that a procedure or a point of view is objective is to claim that it is valid, irrespective of human expectations, ideas, attitudes, wishes".

In fact, objectivity is closely linked with reason; they cannot be separated from each other. In this sense, tradition embodies rationality if only because it is formed under continuous tempering by the objective world. Scientists and intellectuals rightly talk of rationality and objectivity at the same time and believe that both must be present in scientific research. For example, Einstein put forward two criteria of theory which in fact are those of objectivity and reason. Although Einstein, as Feyerabend mentions, held that scientific theories and concepts are "free creations of the human mind", he believed that there are two principles that scientific theories must meet. A theory must tally with the external world and it must be coherent. The first criterion concerns the relation between theory and facts, while the second concerns reason. "The first point of view", Einstein said, "is obvious: the theory must not contradict empirical facts".[221] "The second point of view", Einstein said, "is not concerned with the relation to the material of observation but with the premises of the theory itself, with what may briefly but vaguely be characterised as the 'naturalness' or 'logical simplicity' of the premises (of the basic concepts and of the relations between these which are taken as a basis)".[222] According to Einstein,

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[221] A. Einstein, "Autobiographical Note", in *Albert Einstein: Philosopher-scientist*, edited by Paul Arthur Schilpp, The Library of Living Philosophers, Evanston, Illinois, 1949, p. 21.

[222] *Ibid.*, p. 23.

the objectivity of theories is not that they must be absolutely independent from "subjective" elements, but that they must ultimately tally with the external world. For him, Reason is actually associated with objectivity and he allows that not all scientific reasoning need have the same structure.

I hope that I have shown that Feyerabend shifts his standpoint when criticizing scientific rationality and that he is wrong to think that all scientific reasoning has the same form. I have argued that reason and objectivity cannot be separated from one another, and that tradition is rational because it depends on interactions with the objective world over a long period of time.

4. Beyond Scientific Theory

In what follows, I am going to show how following a tradition should be seen as rational activity by considering how scientists proceed during scientific revolutions. Reasoning on the basis of existing tradition, they invent a new theory and use this to change the tradition itself.

First, I discuss briefly what happened in the development of quantum mechanics. I consider how scientists reasoned to their conclusion. The adoption of quantum mechanics is viewed by many people as a more revolutionary event than the transition from classical physics to the theory of relativity. This is because the theory of relativity seems to emerge from continuous medium mechanics while quantum mechanics is a really different tradition since it involves discontinuities. To find out what happened in this case is thus likely to be very helpful for understanding scientific reasoning. In the following discussion, we will again see that theory evaluation accompanies theory invention.

The development of quantum mechanics shows that there is something in scientific tradition beyond individual theories that plays a role in theory evaluation. Before scientists build the new theory they have already drawn a map for establishing it; they build it using not only knowledge contained in the old theory but also knowledge about the character of such theories. For example, researching the theory of the atom, Niels Bohr put forward a correspondence principle, which set up a criteria for looking for new theory. He believed that while the new theory would have new concepts and a new descriptive method, it would have to reduce to the old theory when applied under the conditions in which the latter was known to work. This principle played a significant role in the development of quantum mechanics, but it is obviously not part of any individual theory. It implies that the old theory still can apply in certain domains and that at certain points the new and the old must coincide. Hence, it concerns the relation between the two theories. It is at a higher level than either theory, and thus has a wider function.

Heisenberg's view about observable quantities likewise shows that analyses of an old theory's development can guide the formation of a new one. Having recognized that Bohr's theory of the atom was in difficulty, and having analyzed how Einstein developed the theory of relativity, Heisenberg put forward the idea that a physical theory should contain only quantities which can be directly observed (the so called, "principle of employing only observable quantities"). This idea guided him to choose the quantities forming the theory. As Heisenberg says,

> The following-out of this notion led to a mathematical formalism which really seemed to fit the atomistic phenomena. In conjunction with Born, Jordan and Dirac, it was then elaborated into a closed quantum mechanics, and appeared so convincing that there really could be no further doubt of its correctness.[223]

Still, both Einstein and later Heisenberg abandoned this principle. (I shall come back to this point later.)

A third example, E. Schrödinger's work, also shows that scientists use the knowledge about the relationship between the new and the old, or in other words, about the new theory's position in the whole theoretical system. In the light of this kind of knowledge, scientists imagine the content and the form of the new theory to determine which existing paradigm should be imitated. In the history of development of science, Hamilton drew an analogy between ordinary mechanics and geometrical optics and used the method of the latter to inquire about the former. However, he considered only the mathematical form; he did not consider the physical nature of the phenomena. This was left to Louis de Broglie who was impressed by research on the

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[223] Heisenberg, *op. cit.*, p. 112.

nature of X-rays in 1924, when X-rays was recognized as waves. De broglie put forward the view that every movement of a mass point goes with a wave whose wavelength can be computed from the mechanical impulse of the particle by means of the Planck's constant *h*.

From all these considerations Schrödinger was able in 1926 to derive the fundamental idea of wave-mechanics. He said:

> Perhaps our classical mechanics is the *complete* analogy of geometrical optics and as such is wrong and not in agreement with reality; it fails whenever the radii of curvature and dimensions of the path are no longer great compared with a certain wave length, to which, in *q*-space, a real meaning is attached. Then it becomes a question of searching for an undulatory mechanics, and the most obvious way is the working out of the Hamiltonian analogy on the lines of undulatory optics.[224]

Afterwards, Schrödinger described what he had achieved this way:

> The step which leads from ordinary mechanics to wave mechanics is an advance similar in kind to Huygens' theory of light, which replaced Newton's theory. We might form the symbolic proportion:
> Ordinary mechanics : Wave mechanics
> = Geometrical optics : Undulatory optics.
> Typical quantum phenomena are analogous to typical wave phenomena like diffraction and interference.[225]

This shows that by using ideas about the relationship between the old theory and the anticipated new theory, Schrödinger was able to set up a map giving directions for obtaining the future

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[224] E. Schrödinger,"Quantisation As A Problem of Proper Values (Part II)", in *Collected Papers on Wave Mechanics*. Chelsea Publishing Company, New York, 1982, p. 18.

[225] E. Schrödinger,"Four Lectures on Wave Mechanics", in *Ibid.*, p. 162.

theory. This map gave him the information that he needed to establish a new mechanics which would describe phenomena in terms of the motion of waves. Thus, clearly he knew the content and the form of the new theory in advance, and he knew which existing paradigm he should imitate. In fact, his subsequent work can be seen as starting from the classical wave theory of elastic bodies. By substituting quantities in the wave equation by quantities derived from Bohr's stationary energy-levels and using the Hamiltonian analogy between mechanics and optics, he obtained the so-called Schrödinger Equation.[226]

Similarly in revolutions scientists sometimes base their reasoning on the mathematical relations. A good example of this is Dirac's discovery on reading Heisenberg's first paper in matrix mechanics that he had added a condition to matrix multiplication which can be written in the form $pq-qp=h/2\pi i$. He then drew a parallel between this and the Poisson bracket, and established a transformation theory for quantum mechanics. One may say that Dirac's work is only relevant to the development of the mathematical scheme of quantum mechanics, rather than to the idea or concept of quantum mechanics. However, when scientists are establishing a new theory, at a certain point they can leave the physical content for a while and reason strictly mathematically, and connect the solution with the physical world later. In this way, they also can develop new ideas, as for example, Dirac did in his many works leading to new physical

[226] Cf. *ibid.*, pp. 155-165 and pp. 13-30.

ideas that started from mathematical work.

I also would like to mention a different sort of example from the development of quantum mechanics. Soon after Heisenberg and Born obtained matrix mechanics, they called on the famous mathematician D. Hilbert to consult him on matrix calculation. Hilbert answered that he had only met the matrices once as a side product of a question about boundary conditions on a differential equation, and he pointed out that if Heisenberg and Born could find a differential equation which had the solution as their matrixes they would solve more questions using it. We can see from this that Hilbert's suggestion was based totally on mathematical knowledge. However, his reasoning actually pointed in a fruitful direction, (even though Heisenberg and Born wrongly believed that Hilbert did not know what he was saying and did not take his advice). In fact, Schrödinger's equation is what Hilbert expected (and many years later, Hilbert said in a speech that had Heisenberg and Born taken his suggestion they would have discovered wave mechanics six months earlier than Schrödinger). These positive and negative considerations again show that mathematical knowledge stored in the old tradition can play an important role in the reasoning.

From these examples we can see that reasoning accompanies theory evaluation and that in so far as scientists evaluate theories in the course of establishing them, their reasoning accompanies their theoretical innovations. From all these examples we can also see that although scientists may reason from different angles during a scientific revolution they all are reasoning in terms of the existing tradition, that they are all using the knowledge of the old theory and of the relationship between the old and the anticipated new theories.

Tradition in science thus includes much more than the theories around which the tradition is built. In particular it includes elements that more tenacious than theories. In each of the examples I have given of what scientists did there is obviously something different from what Kuhn, Feyerabend and Coleman maintain. Scientists did not argue in a circle as Kuhn thinks they must do.[227] And while their criteria are indeed constituted, they are, contrary to what Feyerabend says,[228] not temporally constituted by the research on hand; rather they are based on the knowledge of the old theories; they judge events and processes. In particular, criteria are not temporarily pieced together for the work on hand. For example, Heisenberg's principle of employing only observable quantities had been used in Einstein's work; this was not a "makeshift".[229]

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[227] Cf. Kuhn, *The Structure of Scientific Revolutions*, p. 94, where Kuhn says: "When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense."

[228] Feyerabend says: "criteria do not merely judge events and processes, they are often constituted by them." *Farewell to Reason*, p. 283.

[229] Samuel Coleman says: "The reason in a traditional practice ...is a function of the extent to which that practice meets the wants of its practitioners." *op. cit.*, pp. 239-282, p.

5. Change of Criteria

I now turn to the question of how criteria change in this kind of reasoning and consider whether a change of criteria shows that scientists' reasoning is not rational. To answer this question, I shall consider how Einstein and Heisenberg changed their view about the "principle of employing only observable quantities". When establishing their theories, both Einstein and Heisenberg used this principle. However, later they both changed their minds about it. In what follows I attempt to show how I understand this very interesting fact.

In the spring of 1926, in discussion with Heisenberg, Einstein questioned this idea. He argued that the principle of employing only observable quantities simply cannot be consistently carried out. When Heisenberg objected that in this he had merely been applying the type of philosophy that Einstein had made the basis of his special theory of relativity,[230] Einstein answered: "Perhaps I did use such philosophy earlier, and also wrote it, but it is nonsense all the same."[231] Heisenberg believed that Einstein had meanwhile revised his philosophical position on this point and that for Einstein it is

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281) and " The tradition as it is practised is very likely to be a makeshift, an adaptation, and not optimally efficient for its practitioners." *Ibid.*, p. 282.

[230] This reminds us of Polanyi's point about new practitioners watching how masters do something. In this case, what Heisenberg learned is not the content of a theory but the knowledge about how to form a new theory.

[231] Cf. Heisenberg, *op. cit.*, pp. 113-114.

theory which first determines what can be observed. Many years later, Heisenberg says:

> These considerations were quite new to me, and made a deep impression on me at the time; they also played an important part later in my own work, and have proved extraordinarily fruitful in the development of the new physics.[232]

He says that when he was young, he believed that this was just the philosophy which Einstein had followed in his theory of relativity, and that he had tried to take a corresponding step in quantum theory by introducing matrices. After talking with Einstein, Heisenberg concluded that

> When we go from the immediate observation-a black line on a photographic plate, a discharge in a counter or suchlike-to the phenomena we are interested in, we must make use of theory and of theoretical concepts. We cannot separate the empirical process of observation from the mathematical construct and its concepts. The most conspicuous demonstration of this thesis of Einstein's was the later discovery of the relations of uncertainty.[233]

It is very interesting that Einstein and Heisenberg both took advantage of the "principle of employing only observable quantities" at the beginning of their work and that both came to recognize that in theories there are always some quantities which cannot be observed directly. This is a good example for showing that scientists change their views about criteria. Now I am going to show that this does not mean that what they did at the beginning is not rational.

First, what Einstein and Heisenberg did is proper to the

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[232] *Ibid.*.

[233] *Ibid.*, pp. 10-11.

objective situation that they faced. Given that observations depend on theories, this idea should work for both the new and the old theories. Thus, while scientists are establishing a new theory, they are in a dilemma. On one hand, they seem to have to give up all the observations that depend on the old theory. However, to establish the new theory they need the concepts which link with the phenomena, and thus need observations that should be determined by the new theory. On the other hand, if they rely on observations and concepts that depend on the old theory, they must accept the old theory. Since the new phenomena have been discovered on the assumption that the old theory is satisfactory, they have been observed under the concepts of observation determined by the old theory, and the observation of the new phenomena has to be expressed in the concepts of the old theory. This is where scientists start in scientific revolutions; at the beginning of establishing new theories they have to retain at least some existing concepts and observations. But since they intend to reject the old theories, it is rational for them to try to avoid using too many concepts of the old tradition. The natural way to do this is clearly to employ only observable quantities.

Second, scientists do not change their minds opportunistically to fit their desires. Rather, they change them after using their old views to judge events and processes. Contrary to Feyerabend, the criterion under discussion is not an artificial assumption to make existing theory fit the facts. It was helpful for both Einstein and Heisenberg in the process of establishing their views.

Third, regarding the change in these philosophers' thinking about this principle, it is one thing to try to employ as few as possible of the observable quantities cast in terms of the old theory *at the beginning* of the approach to the new theory, it is quite another to hold that the theory should only include directly observable quantities. Einstein and Heisenberg, rightly changed their minds about the latter, but not the former. In fact, in the history of the development of science, this idea has clearly played an important role. Heisenberg points out that in his case it provided a guarantee of connections between the mathematical formulae and the phenomena, and that it led to a mathematical formalism that fitted atomic phenomena.[234] In addition, we can see that this idea can help in some cases, because examining whether the quantities used in the formulae are observable can open the way to new ideas.[235]

I conclude therefore that the fact that criteria may change in the course of scientific inquiry does not in and of itself lead to the view that following such criteria is never rational.

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[234] Cf. *ibid.*, p. 112.

[235] In the development of mechanics, Lorentz got what is now called "Lorentz transformations" earlier than that Einstein got them in his relative theory. We now can see that if Lorentz had found the principle we discussed above and had noted that the time called "mathematical time" by him is time that can be observed directly, which, should be concerned as a basic notion, (in contrast to Newton's time, which cannot be observed directly), he could well have obtained the idea before Einstein.

9

Tradition and Community Authority

In this chapter, I discuss Kuhn's "community-authority thesis". This thesis figures in the discussion of tradition in various ways. First of all, traditions function as authorities and when we discuss tradition we cannot avoid also discussing authority. As J. C. Nyiri says, "the term 'tradition' is surrounded by a family of related terms. This family would include terms like 'authority', 'convention'...".[236] For Kuhn the relevant notion of authority is that of community authority, his view being that there is a relation between the authority of the community and the authority of tradition. Secondly, community authority directly connects to the ideas of paradigm and theory choice. When the role of scientific tradition in a scientific revolution, i.e., in theory invention and evaluation, is considered, the authority of the community must be considered as well. Third, a discussion of community authority is helpful for understanding tradition and rationality. This is linked with the discussion of the last chapter, since it concerns the question of whether the community's decision depends on reasoning. In what follows, I shall first clarify what Kuhn's community authority thesis really involves; then I shall proceed to question his discussion of community authority. I shall argue that scientists do not arbitrarily choose paradigms but choose them in the light of the requirements of the tradition with which they are associated. In addition, I intend to argue that objective factors are decisive in the development of science, contrary to what Kuhn claimed when he proposed his community authority thesis.

1. Kuhn's Community Authority Thesis

Kuhn puts forward the notion of community authority in

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[236] J. C. Nyiri, "Tradition and Practical Knowledge", in *Practical Knowledge,* J.C. Nyiri and B. Smith (eds), Croom Helm. London, 1988, p. 27.

discussing scientific revolutions. He draws a parallel between a scientific revolution and a political revolution. His view is that

> the historical study of paradigm change reveals very similar characteristics in the evolution of the sciences. Like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life.[237]

So for Kuhn, while in normal science the paradigm provides criteria for choosing questions and for deciding on the merits of answers to them, in revolutionary science, such criteria are lacking. Kuhn says:

> When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense.[238]

On this basis, Kuhn believes that paradigm choice is not solely a matter of logic and experiment; rather, it is a matter of belief and techniques of persuasive argumentation. He holds that "in paradigm choice-there is no standard higher than the assent of the relevant community".[239]

For Kuhn, the community has the authority to choose a paradigm at least partly in accordance with what it prefers rather than in accordance with objective considerations. Indeed, he holds that competing paradigms cannot be compared, from which it follows that paradigm choice is by no means based

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[237] Kuhn, *The Structure of Scientific Revolutions*, p. 94.

[238] *Ibid.*.

[239] *Ibid.*.

on reason. However, Kuhn denies that the decision of a scientific group to adopt a new paradigm cannot be based on good reasons of any kind, factual or otherwise.[240]

His defense has two main parts. One is that "the choices scientists make between competing theories depend ... on shared criteria--those my critics call objective--[as well as] on idiosyncratic factors dependent on individual biography and personality".[241] Another is that "the criteria of choice with which [he] began function not as rules, which determine choice, but as values, which influence it."[242] So even in his later writings Kuhn holds that the community has authority to choose a theory.

However, he now seems to move his focal point from incommensurability to two others, namely: idiosyncratic factors dependent on the individual scientist and value criteria rather than rules. I believe that this move is very problematic. Claiming that he now believes that a scientific group adopts a new paradigm based on good reasons, Kuhn puts himself in the awkward position that I argued in chapter 5 Feyerabend is in. For he has to hold that there are good reasons for paradigm choice and hence that competing paradigms can be objectively compared contrary to his incommensurability thesis. Here I will not go over this again. Instead, I shall demonstrate that

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[240] Cf. Kuhn, *The Essential Tension*, pp. 320-339.

[241] *Ibid.*, p. 329.

[242] *Ibid.*, p. 331.

authority lies not so much with the community of scientists as with the traditions of science itself. After that I shall discuss Kuhn's later view on theory choice.

2. The Authority of Tradition and The Authority of Community

First, however, I would like to call attention to the relation between the authority of tradition and the authority of community. When we regard tradition and community authority, we must deal with them carefully. On one hand, as I mentioned before, Kuhn emphasizes the adherence to tradition. However, tradition is associated with authority. So it seems that Kuhn must emphasize traditional authority. On the other hand, Kuhn puts forward community authority, which does not come from or represent tradition at all. Rather, he seems to hold that this is independent of traditional authority, even to be definitive of a tradition.

Some philosophers, such as Barker, take Kuhn's conception of community authority to be similar to Wittgenstein's conception of consensus of practitioners.[243] But it seems to me that Kuhn's and Wittgenstein's conceptions play very different roles. For Kuhn, authority of community plays a role in scientific revolutions when the system changes significantly, whereas for Wittgenstein consensus of practitioners plays a role in the language used when the system develops normally.

According to Kuhn, community authority thus becomes

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[243] Cf. Barker, *op. cit.*, p. 166.

significant when scientists come face to face with the problem of choosing a new paradigm. He suggests that the assent of community is the highest standard for choosing a paradigm. Community authority plays a role in paradigm choice at the time that scientists decide which paradigm can guide scientific inquiry. By contrast, Wittgenstein is arguing that consensus is crucial for determining whether people use language correctly or not. This is more like what Kuhn takes to be the "normative function" of paradigms in normal science.

Moreover, Kuhn holds that the notion of paradigm displaces the notion of consensus among the members of a scientific community. In fact, Kuhn initially took consensus to be necessary for normal science in contrast to scientific revolutions. In the preface of *The Essential Tension*, Kuhn says:"[In] 1958/59 ... I conceived normal science as the result of a consensus among the members of a scientific community."[244] He thought that he would not be able to write the chapter on normal science without it.[245] Indeed early in 1959 he even used "consensus" rather than "paradigm" when discussing normal science. But later "between the summer of 1959 and the end of 1960, paradigm displaced the previous talk of consensus".[246] In other words, for Kuhn "paradigm" corresponded to "consensus"; he took paradigms to serve the function in normal science commonly

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[244] Kuhn, *The Essential Tension*, p. xviii.

[245] *Ibid.*, p. xix.

[246] *Ibid.*, p. xix.

attributed to consensus among the members of a community.

More to the point, Kuhn subsequently took paradigms to be more fundamental because he came to think that consensus do not exist in normal science and to reject the idea that consensus is necessary for normal science. His new view was that it is often exceedingly difficult to specify what defines a tradition by enumerating what its members agree on. Kuhn says: "Apparently, the consensus I had been seeking did not exist" and "that no consensus of quite that kind was required."[247] As I read Kuhn, where Kuhn differs from his predecessors was his taking the concept of paradigm as a substitute for consensus. Thus, since he emphasizes that the notion of paradigm is a substitute for consensus, and rejects the usual conception of consensus as shared assumptions, it is unreasonable to take his notion of community authority, as Barker does, to be similar to Wittgenstein's consensus conception of practitioners.

The more plausible view, I think, is to take Wittgenstein's consensus of practitioners to represent the authority of tradition. As I pointed out before, for Kuhn traditions are embodied in paradigms. Thus, for Kuhn, the authority of tradition is embodied in the "normative function" of paradigm. While he takes the development of science as the replacement of traditions, he takes the authority of community to be higher than that of tradition.

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[247] *Ibid.*, p. xix.

3. The Authority of Tradition

Here I want to suggest that community authority depends on tradition and that when a community decides on a new paradigm the scientists are guided by the existing tradition. I shall contend that a community cannot choose a paradigm or tradition, freely or arbitrarily. By this I mean that, first, all scientists have been influenced by some tradition already when starting their research; and, second, they do not choose a paradigm by belief, rather by their actual practice; third, their choices are limited by the objective world, thus they seem to be forced to accept rather than choose. In doing this, I shall first argue that scientists do not choose which paradigm to espouse, drawing on some points mentioned in previous chapters; then I shall show that paradigms are not chosen freely even when they are being inaugurated in the first place.

First of all, scientists in a community cannot choose the tradition that they are associated with. As I mentioned before, scientists start their work in the surroundings in which they happen to be rather than in surroundings that they desire or choose. In other words, scientists cannot choose a paradigm when they obtain their training; they cannot but align themselves with the existing tradition. As Wittgenstein said:

> Tradition is not something a man can learn; not a thread he can pick up when he feels like it; any more than a man can choose his own ancestors.[248]

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[248] L. Wittgenstein, *Culture and Value*, G.H. von Wright (ed)), University of Chicago Press, Chicago, 1984, p. 76.

Secondly, when scientists accept a tradition, they also accept a way of thinking. As I argued in the last chapter, although a tradition is embodied in a paradigm theory, it includes much more than a theory. When scientists accept a theory, they also learn how to think and act. These parts of knowledge--like a frame--have authority which must be acknowledged. This is the particular situation of authorities in science. More to this point, as Wittgenstein notes concerning authorities in general, "[one] must recognize certain authorities in order to make judgment at all".[249] Maxwell, as a scientist, expressed a similar point of view. He said:

> In our daily work we are led up to questions the same in kind with those of metaphysics; and we approach them, not trusting to the native penetrating power of our own minds, but trained by a long-continued adjustment of our modes of thought to the facts of external nature.[250]

Here, I take it, the "long-continued adjustment of our modes of thought to the facts of external nature" is embodied in the traditions that scientists must come to accept (rather than choose). When scientists evaluate a new theory, they follow a mode of thought that is led up to by the facts of external nature.

Third, the selection of problems depends on the existing tradition. As Heisenberg has said:

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[249] L. Wittgenstein, *On Certainty,* edited by G. E. M. Anscombe and G. H. von Wright, translated by D. Paul and G. E. M. Anscombe, Blackwell, Oxford, 1969, p. 161.

[250] Maxwell, *op. cit.*, vol. II, p. 216.

> Looking back upon history ..., we see that ***we apparently have little freedom in the selection of our problems.*** We are bound up with the historical process, our lives are parts of this process, and our choice seems to be restricted to the decision whether or not we want to participate in a development that takes place in our time, with or without our contribution.[251]

Moreover Kuhn himself explicitly states that problem selection is normally dependent on paradigms.[252] In fact, while scientists often think that their problems can be solved by using the resources of the existing tradition, it can turn out that some problems can only be solved by changing paradigms. In this sense, it is tradition that gives us the chance to find out what aspect of the tradition has to be changed. Noting that all changes of tradition in science are caused by attempts to solve these problems selected under existing traditions, we have to say again that this is not scientists' free choice.

Even during the process of paradigm change, scientists are guided by the existing paradigm. As Kuhn himself acknowledges, following a tradition is the best way to change it. He writes:

> At least for the scientific community as a whole, work within a well-defined and deeply ingrained tradition seems more productive of tradition-shattering novelties than work in which no similarly convergent standards are involved.[253]

But Kuhn overlooks the fact that this implies that new theories

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[251] Heisenberg, *op. cit.*, p. 3. emphasis added.

[252] See Kuhn, *The Structure of Scientific Revolutions*, p. 109: "When paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions".

[253] Kuhn, *The Essential Tension*, p. 234.

are formed within the framework of existing traditions and established theories. As we know, even the most revolutionary new theory has to be compatible, at least, with some observations made using and must correspond to some existing theories. This is related to the objectivity of the theory and I will come back to the point later.

From this we can say that tradition has some authority in the historical process of science and that in theory choice it is not so much a matter of community authority but a matter of following a tradition.

4. Do We Choose at the "Beginning" ?

One might argue that while this may be right when a science is under way, it is less clear regarding the existing tradition itself. Surely, it might be argued, this at least must have been chosen freely by ancient scientists at the beginning. I believe that this argument is far from Kuhn's original community authority argument but the question is interesting and helpful for the discussion of traditional authority and its objectivity, as it relates to the nature of human cognition and the relation between the objective and subjective sides of cognition. In what follows, I shall begin my discussion by reviewing one of Kuhn's views about the learning process; then evaluate it in light of some of Pierre Duhem's remarks. I shall attempt to show that Kuhn is right to suggest that people learn from nature by detecting the similarities between things and knowledge can be thought of as being embedded in the similarity relationship itself. However I shall also contend this shows that it is wrong to emphasize the community authority thesis.

It is hard to imagine that people once did not have any tradition, that they "began" to know something without knowing anything. However, assuming that this is possible, let us see how Kuhn thinks people learn from nature. I think Kuhn has a good point to make here but overlooks that, as long as people learn from nature in the way he describes, the knowledge they obtain must be determined by objective similarities. This means that they are not free to choose whatever theory they wish.

First, let me review what Kuhn says about how people learn from nature. Kuhn holds that the sense of similarity can play a role in scientific inquiry; Indeed he suggests that the

> basic criterion is a perception of similarity that is both logically and psychologically prior to any of the numerous criteria by which that same identification of similarity might have been made.[254]

Next, Kuhn suggests that students learn to apply symbolic labels to nature without anything like definitions or correspondence rules in much the same way as a small child learns to identify swans, geese, and ducks. Kuhn believes that in the absence of definitions and correspondence rules, one can employ a learned but nonetheless primitive perception of similarity and difference. By such perception, one can learn something about nature and "this knowledge can thereafter be embedded ... in the

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[254] Kuhn, *The Essential Tension,* p. 308.

similarity relationship itself."[255] Notice that in Kuhn's example, he emphasizes that small children start learning without anything like definitions or correspondence rules. From this, we can assume that he would agree that this method would apply at the very beginning of human inquiry.

Clearly, the knowledge attained in this way is objective because the similarity and the differences of the natural things are objective. People may use different terms to express these similarities and differences, but these are incidental. More to the point, if this is right, people cannot be said to choose theories freely; their theories must initially express similarities and differences in the objective world.

To show that ancient people started to learn things in the way described by Kuhn, I would like to mention Chinese traditional medicine. To discuss whether Chinese traditional medicine is scientific is not relevant here. However, it is an ancient knowledge system. We can learn from it how ancient people learned their knowledge from nature and stored it. In ancient China people learned the medical function of plants and other herbs by tasting them and retaining their knowledge according to the similarity relationship among function and colour, smell, and taste, i.e., by noting that plants having the same colour, smell and taste have the same medical function. For example, people found that most plants that are suitable for treating blood and the heart are the same colour--red. It is

[255] *Ibid.*, p. 312.

very interesting that today, people have found, through modern technical methods, that in those plants there are the same compounds which serve the medical function.

It is evident that traditional medicine needs to be developed. However, it is difficult to deny that ancient people acquired their medical knowledge by finding the similarity relationship and that the knowledge they acquired is expressed and passed by in the form of similarity relationship. The reason that the tradition of ancient medicine has lasted for so long is not only because most of ancient medicine's knowledge correctly reflects facts of nature but also because the tradition provides us with a way to develop this knowledge. For example, in its theory of diagnosis, there is a principle called "focus on similarities, classify things, and model similar phenomena", while in practice there is a principle about the relation between function and colour-- namely "four smells, five tastes"--to go by. People classify things and phenomena and find the similarities among them.

At the beginning of modern science, some scientists mentioned something like this. For example, Galileo believed that his investigation is led by the hand by nature herself, through similarities among "works" of nature. He says:

> It is as though we have been led by the hand to the investigation of naturally accelerated motion by consideration of the custom and procedure of nature herself in all her other works, in the performance of which she habitually employs the first, simplest, and

easiest means.[256]

One may argue that people may first choose different similarities. However, according to Kuhn, that does not happen before they gain knowledge, his view being that

> there is a means of processing data into similarity sets which *does not depend on a prior answer to the question, similar with respect to what?*[257]

According to Kuhn's view about learning from nature, then, a theory is not chosen by the community but rather the community simply adopts it. Kuhn rightly takes the character of learning to be a reason for thinking that "shared examples have essential cognitive functions prior to a specification of criteria with respect to which they are exemplary".[258] But he fails, as I shall show in what follows, to appreciate that such examples of essential cognitive functions also implies that community does not choose these examples but rather assimilates them.

5. Kuhn's and Duhem's Views of Knowledge

I have argued that on Kuhn's view about learning, people's knowledge, and therefore their theories, are not chosen at the "beginning". In what follows, I attempt to reinforce this point by comparing Kuhn's view with Duhem's.

What Kuhn says about the way in which people learn from

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[256] Galileo Galilei, *Two New Science,* translated with introduction and notes by Stillman Drake The University of Wisconsin Press, 1974, p. 153.

[257] Kuhn, *The Essential Tension*, p. 308.

[258] *Ibid.*, p. 313.

nature and about the form in which their knowledge is embodied in the similarity relationship is similar to Pierre Duhem's idea about the nature of theory and natural classification.[259] Drawing on this idea, I would like to focus attention on the similarities between Kuhn's and Duhem's discussions. I want to suggest that the similarity between the two shows that Kuhn cannot hold his view about the way in which scientists learn things from nature as long as he continues to hold his view that community authority is the highest criterion of theory choice.

First of all, neither Kuhn nor Duhem think that theories are artificial. Duhem says: "physical theory is not merely an artificial system ... but an increasingly more natural classification and an increasingly clearer reflection of realities".[260] And similarly for Kuhn, since he takes theory invention to be closely linked with discovery. He believes that the distinction between discovery and invention or between fact and theory cannot be made out and that "both observation and conceptualization, fact and assimilation to theory, are inseparably linked in discovery".[261] By this he means that a theory cannot be totally artificial but must reflect something of the facts.

Moreover, both Kuhn and Duhem hold similar points of view

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[259] Cf Andrew Lugg, "Pierre Duhem's Concept of Natural Classification", *Synthese*, 83:409-420, 1990.

[260] P. Duhem, *The Aim And Structure Of Physical Theory*, New York, Atheneum, 1974, p. 270.

[261] Kuhn, *The Structure of Scientific Revolutions*, p. 55.

about the nature of theory. For Duhem, theory "assumes, while being completed, the characteristics of a natural classification. The groups it establishes permit hints as to the real *affinities of things*".[262] And for Kuhn the knowledge is "embodied in the *similarity relationship itself*".[263] Kuhn's "similarity relationships" are like Duhem's "affinities of things": both are factual in nature. Indeed, for Duhem, theories are a reflection of similarity relationships in nature. More to the point, since Kuhn holds that "knowledge" is encapsulated in paradigms and that paradigms and theories are closely linked, we can say that Kuhn should hold the same point of view about the nature of theory as Duhem does.

In fact, Kuhn's description of the way in which people learn from nature can be seen as supporting Duhem's point of view about theories. I mean that the way of acquiring and storing knowledge described by Kuhn leads directly to Duhem's point of view that theories are natural classifications. Put another way, Kuhn can be thought of as explaining why theories are natural classifications by investigating how people learn from nature. This point leads us to think of language as a net of concepts which reflects the nature of things, or in Kuhn's words similarity relationships between them. If I am right about this, we can conclude that people do not establish an artificial theory even at the beginning and hence that they do

[262] Duhem, *op. cit.*, p. 30, emphasis added.

[263] Kuhn, *The Essential Tension*, p. 312.

not chose a scientific tradition arbitrarily even to start with.

A similar point of view is defended by many scientists. For example, Maxwell believes that the formation of language was determined by nature at the beginning and that theory is embodied in our language. There is no room for choice:

> Intellectual processes of this kind (long-continued adjustment of our modes of thought to the facts of external nature) have been going on since the first formation of language, and are going on still. No doubt the feature which strikes us first and most forcibly in any phenomenon, is the pleasure or the pain which accompanies it, and the agreeable or disagreeable results which follow after it. A theory of nature from this point of view is embodied in many of our words and phrases, and is by no means extinct even in our deliberate opinions.[264]

So, we can see that even at the beginning tradition has more authority, and the scientific community must accept the knowledge embodied in language first.

Let me summarize what I have done, and draw some conclusions. I first clarified what Kuhn's community authority means; then I argued that traditions are not chosen freely by scientists when handed down. Also I have drawn a parallel between Kuhn and Duhem's points of view. This indicates that Kuhn's community authority thesis is suspect. As I pointed out at the beginning, community authority for Kuhn does not come from or represent tradition. Rather, it is as he see it, pitted against tradition's authority (or determines a tradition). The parallel he draws between scientific and political revolutions shows that he thinks that the development of science does not

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[264] Maxwell, *op. cit.*, vol. II, p. 217.

have any order, but only depends on the community's choice. From the discussion above, however, we can see that when a tradition is formed and handed down, the community does not choose it freely; rather the community's choice is guided by nature and influenced by tradition. Therefore, authority lies not so much with the community of scientists as with the traditions of science itself.

6. Kuhn's Later View of Theory Choice

Later Kuhn talks of the authority of community over paradigm choice in the terms of theory choice. In the above discussion, I emphasized the objective nature of paradigm choice. Kuhn, later, builds his community authority thesis on the basis of the claim that individual scientists may make different choices. In what follows I discuss this emphasizing what Kuhn said about subjective matters in theory choice. My object is to show that when scientists make different choices, usually they are following different traditions, and that, more importantly, although they may choose different approaches, they may reach the same results because of the constraints imposed by the objective world. Just as individual scientists do not have the authority to decide which theory to accept, neither does the community as a whole.

Kuhn claims that he "agree[s] entirely with the traditional view that [standard criteria for evaluating the adequacy of a theory] play a vital role when scientists must choose between an established theory and an upstart competitor. Together with others of much the same sort, they provide the shared basis for theory choice."[265] This position is already far from his original view encapsulated in his question "What better criterion could there be than the decision of the scientific group?"[266] Whereas originally Kuhn opposed the idea of any criterion higher than the scientific group, now he accepts that there are shared, standard criteria for evaluating competing theories and concedes the these criteria play a role in theory choice.

This is a big change. However he does not change his original point of view about theory choice completely. Claiming that theory choice "depends on a mixture of objective and subjective factors",[267] he attempts to say that because there are subjective factors in theory choice, what is decided depends on the individual scientists involved and ultimately on the community as a whole. For him, nature thus depends on what "box" (paradigm) community choice to put it in. In what follows, I attempt to show that we should recognize that these subjective factors are restricted by objective factors after all, i.e., I want to say that nature forces scientists to change their "box".

First, let us see what Kuhn's subjective factors are. He

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[265] Kuhn, *The Essential Tension,* p. 322.

[266] Kuhn, *The Structure of Scientific Revolutions,* p. 170.

[267] Kuhn, *The Essential Tension,* p. 325.

says:

> The choices scientists make between competing theories depend not only on shared criteria--those my critics call objective --but also on idiosyncratic factors dependent on individual biography and personality. The latter are, in my critics' vocabulary, subjective.[268]

Kuhn believes that because there are subjective factors, "two men fully committed to the same list of criteria may nevertheless reach different conclusions".[269] Moreover, he goes on, the existence of subjective factors has philosophical import. He says: "What the tradition sees as eliminable imperfections in its rules of choice I take to be in part responses to the essential nature of science".[270] So for Kuhn, theory choice depends on the individual scientist because shared criteria are not "sufficiently articulated to dictate the choice of each individual who subscribes to them".[271] The criteria function as values not as rules, values that only influence choice; ultimate authority in theory choice resides with the individual scientist.

I think that Kuhn is right in saying that sociological and psychological elements play a role in the development of science. But I believe that they are secondary. Although the ways in which individual scientists conduct their inquires are

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[268] *Ibid.*, p. 329.

[269] *Ibid.*, p. 324.

[270] *Ibid.*, p. 330.

[271] *Ibid.*

different, it seems to me that objective factors play a decisive role in theory choice. By saying this I am not saying that there is only one theory for a set of facts, nor that there is only one way to establish a theory. Rather, I am saying that the theory selected reflects something that does not depend on the scientists themselves notwithstanding the fact that they choose the concepts and the way to establish this theory. In what follows I try to show this by considering a case from the history of science.

7. Different Routes to The Same Goal

First, when individual scientists conduct their inquiries they are already influenced by the prevailing tradition. For example, in the development of quantum mechanics Heisenberg and Schrödinger proceeded differently. This was because they did not belong to the same tradition, and their research situations were different. While both accepted the main principles of physics, they were impressed by the contents of different parts of physics and influenced by different scientists' points of view. Thus Heisenberg was carrying on the work of C. J. Davisson, A. Sommerfeld, and Niels Bohr, whereas Schrödinger was carrying on de Broglie's work. (This incidentally also shows that when scientists start their works, they have already immersed in ideas originated by others.) Still, when we examine what Heisenberg and Schrödinger established, we can see that their "choices" are determined objectively.

Not only did Heisenberg accept the results of C. J. Davisson, A. Sommerfeld, and Niels Bohr, he also followed their method.[272] Heisenberg himself talks about how Bohr influenced him, pointing out in connection with his discussions with him in Gottingen in the summer of 1922 that "for the first time I understood that Bohr's view of his theory was much more sceptical than that of many other physicists--e.g. Sommerfeld--at that time, and that his insight into the structure of the theory was not a result of a mathematical analysis of the basic assumptions, but rather of an intense occupation with the actual phenomena, such that it is possible for him to sense the relationships intuitively rather than derive them formally." Thus, he continues, "I understood: knowledge of nature was primarily obtained in this way, and only as the next step can one succeed in fixing one's knowledge in mathematical form and subjecting it to complete rational analysis."[273] For Heisenberg this discussion certainly had a decisive influence on his later career.[274] In fact, Heisenberg in his work makes considerable use of Bohr's method in his analysis of the phenomena. In particular he believed that Bohr's orbits are not very important, because the things

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[272] This method can be seen as an example of learning from nature in the way that Kuhn suggest: knowledge comes by perceiving the relationship itself.

[273] B. L. Van Der Waerden (ed), *Sources of Quantum Mechanics*, North-Holland Publishing Company, Amsterdam, 1967, p. 22.

[274] See *ibid.*, p. 22.

observed are connected closely with two Bohr orbits and not just with one. He ended up formulating matrix mechanics although he did not know he was making use of a well known mathematical theory.

Schrödinger, for his part, was working independently of Heisenberg. His theory of wave mechanics was based on the previous work of de Broglie, who showed how one could introduce waves connected with particles. While his work was also based on Hamilton, Planck and Bohr's works, de Broglie's view influenced him most. Moreover though de Broglie, who was influenced by the discussion of the nature of X-rays, put his view by corresponding considerations, we cannot say that his theory was not influenced by objective factors. Besides Hamilton, Planck and Bohr's works are obviously based on experiments.

In the development of science there are many episodes, such as the discovery of the conservation of energy (Kuhn rightly calls it simultaneous discovery) and the establishment of quantum mechanics, which show that different approaches get the same results. This shows that there must be some objective factor which decisively influence individual scientists. For example, despite the fact that Heisenberg and Schrödinger began their work from different points of origin, and despite the fact that their theories, at first sight, appeared to be completely different, Schrödinger's and Heisenberg's theories are really equivalent. In fact, the two theories entail the same relationships among phenomena.

So, the variety of appearances that the object has provided a possibility for different interpretations. Individual experience provides the scientist with an option of doing his inquiry in a different way. But while scientists, at first sight, "choose" their theories, they are in fact constrained to choose within a very limited range. Despite this, it seems that in every individual case, scientists have some "authority" for making different choices. The objective contents of science however actually play a role in every case and lead scientists to make the same "choices". While these objective factors do not exclusively determine the approach, they eventually force scientists to the same conclusion.

Maxwell put this very well. He believed that a natural phenomenon is very complex and that it looks different to different people. However, no matter with what feature a work begins, the work has to combine with other features of that phenomenon later. Maxwell says:

> Every natural phenomenon is, to our minds, the result of an infinitely complex system of conditions. What we set ourselves to do is to unravel these conditions, and by viewing the phenomenon in a way which is in itself partial and imperfect, to pick out its features one by one, beginning with that which strikes us first, and thus gradually learning how to look at the whole phenomenon so as to obtain a continually greater degree of clearness and distinctness. In this process, the feature which presents itself most forcibly to the untrained inquirer may not be that which is considered most fundamental by the experienced man of science; for the success of any physical investigation depends on the judicious selection of what is to be observed as of primary importance, combined with a voluntary abstraction of

> the mind for those features which, however attractive they appear, we are not yet sufficiently advanced in science to investigate with profit.[275]

According to Maxwell, then, scientists may pick out different features of a phenomenon to begin with. In the selecting of what is to be observed as of primary importance, there are differences between untrained inquirer and experienced man of science. However, as time goes by they have to consider other features of the phenomenon to understand it as a whole. This leads them to adopt the same view. This is helpful for our understanding of how the results of the different ways in which people inquire after the same phenomenon are the same or practically equivalent.

The role of objective factors for scientific development has also been noticed by scientists who took part in the development of quantum physics. For example, when David Bohm talks about the Bohr's correspondence principle, he says:

> This principle states that the laws of quantum physics ***must be so chosen*** that in the classical limit, where many quanta are involved, the quantum laws lead to the classical equations as an average ... The problem of satisfying the correspondence principle is by no means trivial. In fact, the requirement of satisfying the correspondence principle, combined with indivisibility, the wave- particle duality, and incomplete determinism, will be seen to define the quantum theory in an almost unique manner.[276]

Here Bohm expresses his feeling that the later development had almost been determined by this principle already. In fact this

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[275] Maxwell, *op. cit.*, vol I, p. 217.

[276] D. Bohm, *Quantum Theory*, New York, Prentice Hall Inc. 1952, p. 31.

principle is just a reflection of the relationship of the phenomena of classical physics and quantum mechanics. It is an objective factor that plays the decisive role.

I hope I have shown that Kuhn's defense does not stand. Subjective factors may indeed result in scientists pursuing different approaches, however, objective factors play the final role. This means that how the content of a theory comes out is by no means a matter of choice, but is decided by the tradition and the state of development of science. Thus Kuhn's later view of community authority thesis must also be rejected. In what follows, I will support the conclusion by showing the limiting role of objective factors in scientific research.

8. The Limiting Role of Objective Factors

Kuhn denies that he makes theory choice a subjective matter. He says that by "subjective" he does not mean that science is subjective; rather it is a "limitation of objectivity" (or of the "meaning of objectivity").[277] But his emphasis on the limitations of objectivity cancels out a decisive role for objective factors of the sort I have identified.

Talking about limitations, I would like to mention another one, the limitation of the development of science. I believe that the state of the development of science at certain time limits how science can develop and that this is why science

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[277] Cf. Kuhn, *The Essential Tension*, pp. 337-338.

maintains itself as a coherent whole. In fact, as long as a scientist works in a scientific tradition, the direction and results, even those which will lead to the change in tradition, are almost objectively determined by the actual state of science at that time. By saying this, I am saying not only that the problem of selection depends on the tradition (as was argued above), but also that scientific results, even when they are right, cannot be accepted if the whole of science has not yet developed enough for them to be needed and to be retained.

Now let us look at the history of science. We can see that if scientists choose a problem that is not required at that time, their work will not be recognized. For example, consider the science of crystals, which was pursued by many people from 1611 to 1879. However, as Max von Laue says, "these studies had no effect on physics because no physical phenomenon required the acceptance of the space lattice hypothesis".[278] This situation changed only when the wave theory of X-rays and the atomic theory of crystals were formulated. As Von Laue also points out, this event was "one of those surprising events to which physics owes its powers of conviction".[279] This illustrates the fact that while some new ideas, or new theories, are already invented, the scientific community will ignore them until they are forced by the state of development of science to accept

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[278] Cf. M. v. Laue, *History of Physics*, Academic Press Inc., Publishers, New York 1950, pp. 116-120.

[279] *Ibid.*, p. 121.

them.

At first sight, this point seems to support the community authority thesis. However, if we look at it closely, we can see that what the scientific community has been doing is what is required by the actual development of science. This is not so much a matter of choice as a matter of passive acceptance. When scientists "choose" a new theory, they are actually forced to do so by the development of science, not as a result of their "biography" and "personality" as Kuhn would have us believe.

Partly because scientists wait until they are forced by the development of science to embrace a new theory, and partly because a new theory needs time[280] to be developed, the establishment of a new theory is a historical process, in which we can barely see the free choice of the scientists. Their work has been limited by their predecessors' or previous generations' work, i.e., by a tradition. In fact, in the development of science we can almost always find previous resources for a new scientific idea. Pierre Duhem has stressed this point. He says:

> History shows us that no physical theory has ever been created out of whole cloth. The formation of any physical theory has always proceeded by a series of retouchings which from almost formless first sketches have gradually led the system to more finished states; and in each of these retouchings, the free initiative of the physicist has been counselled, maintained, guided, and sometimes absolutely dictated by the most diverse circumstances, by the opinions of men as well as by what the facts teach. A physical theory is not

[280] As I pointed out in chapter 5, sometimes Kuhn rightly claims that paradigm changes "must take time".

> the sudden product of a creation; it is the slow and progressive result of an evolution.[281]

In this sense, new theories are more like necessary results than free choices.

The fact that tradition plays a very important role in the development of science has also been stressed by Heisenberg:

> Without (historical) favourable development, our activity would probably be lost. If Einstein had lived in the twelfth century, he would have had very little chance to become a good scientist. And even within such a fruitful period the scientist ***has not much choice*** in selecting his problems.[282]

Moreover in view of Lorentz and others' works, if Einstein had not chosen to work in physical theory, there would have been someone else who would have done the same job; for

> when electrodynamics compelled the relativity principle that is connected with the Lorentz transformation, it also compelled the change from Newtonian to relativistic dynamics.[283]

P. A. M. Dirac, another famous physicist, seems to have held a similar view, since he too insists that what scientists can do depends on when they are working. Talking about the 1926-1927 period, he says:

> It was very easy in those days for any second-rate physicist to do first-rate work. There has not been such a glorious time since then. It is very difficult now for a first-rate physicist to do second-rate work.[284]

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[281] Duhem, *op. cit.*, p. 221.

[282] Heisenberg, *op. cit.*, p. 3. emphasis added.

[283] M. v. Laue, *op. cit.*, p. 59.

[284] P. A. M. Dirac, "The Development of Quantum Mechanics", in *Directions in Physics*, John Wiley & Sons, New York. 1978, p.

So while Kuhn is right to emphasize the limitations of the shared criteria giving individual scientists authority to choose a theory, he overlooks the "objective" role of the state of the development of the science which dictates the nature of the upstart theory that must be chosen.

All this suggests that science has its own order and that the choice of the individual scientist is only a superficial phenomenon. In fact, some scientists are so impressed by the facts about the development of science that they believe science has its objective nature. For example, Max von Laue has criticized the point of view according to which the whole of science is dependent on all possible environmental factors, including mental and biological ones. He accepts that there are many changes in the development of science, but he points out that

> it (scientific knowledge) itself furnishes proof of its objective truth, proof that has overwhelming power of conviction. A study of the history of this science reveals repeatedly that two trains of physical thought, e.g., optics and thermodynamics or the wave theory of X-rays and the atomic theory of crystals, pursued up to then by different sets of workers, who were quite independent of each other, unexpectedly meet and fit together with no compulsion. Whoever has been privileged to live through such an extremely surprising event, even at a considerable distance, or, at least, to survey it after it has occurred, can no longer retain any doubt that the confluent theories certainly contain, if not complete truth, a substantial core of objective truth that is devoid of human embellishment.[285]

7.

[285] M. v. Laue, *op. cit.*, pp. 8-9.

Because of the objectivity of scientific theory, we can say what is right and what is wrong in a scientific development. In fact Kuhn himself sometimes even talks about the rights or wrongs of community choice. Thus he says:

> Any account of the cognitive apparatus of a scientific community may reasonably be asked to tell us something about the way in which the groups members, in advance of *directly* relevant empirical evidence, identify the special formalism appropriate to a particular problem - especially to a new problem. That clearly is one of the functions which scientific knowledge does serve. *Of course, it does not always do so correctly.* There is room (indeed a need), for empirical checks on a special formalism proposed for a new problem. The deductive steps and the comparison of their end products with experiment remain prerequisites of science.[286]

But if this is right, he must forgo the community authority thesis and recognize that there is something beyond the community's decision that determines scientists' choices. It is this that I have been attempting to show in this chapter.

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[286] Kuhn, *The Essential Tension*, p. 301.

10

Conclusion

From the above discussion, we can see that tradition in science has been mentioned more by historians of science than philosophers of science. This is partly because the role of tradition shows itself more clearly in historical studies than in philosophical ones. Looking back on the history of science, we are impressed by the development of science and by the fact that in spite of the existence of revolutionary changes of ideas, science remains as a coherent whole. Tradition in science exists as part of a common cultural tradition. Science, as Mason suggested, is developed by the combining of two traditions: the technical and the spiritual.

When scholars talk about tradition, they refer to a variety of different things including worldviews, ideas, methods and styles of thoughts. On the one hand, traditions influence the form and style of the knowledge gained by following them. On the other hand, the growth of knowledge may leads to a change of tradition.

When philosophers talk about tradition, they usually have in mind the idea of scientists doing things without reason, of their simply following in the footsteps of the previous generation. Thus "tradition" is often opposed to "reason" and to "preceding in accordance with methodological rules". This is a mistake and all too often tradition has--at least since Francis Bacon attempted to articulate what the method of modern science is--been left out in the cold. Had the attempt to find a special method for science been successful this would not have been a problem, but this has not been the case.

Kuhn calls attention to the tradition by introducing the notion of a paradigm. For him, traditions are embodied in paradigms. By examining his discussion of paradigms and tradition in science, we can see that a tradition in science should be understood as something more concrete, as embodying both theoretical and practical knowledge. Indeed when we investigate the role of tradition in science, we see much more clearly how existing knowledge plays a role in the development of science.

Since Kuhn holds that tradition can play no role in the formation of new traditions in scientific revolutions, and that pre- and postrevolutionary normal-scientific traditions are incommensurable, he ends up embracing relativism and "arationalism". Worse still, when Feyerabend develops a similar point we are finding advocating irrationalism.

A clear understanding of relation between practice, theories and rules is important for the understanding of tradition. Kuhn's view that paradigms have priority is interpreted by some philosophers as giving priority to practice. From the discussion in chapter 4 I conclude that it is a misleading question to ask in philosophy of science whether practice or rule has priority in scientific research. Whenever scientists do their research they are following the rules, and when they are following rules they are engaged in practice. Scientific research involves theory and rules as much as practice.

On the basis of these points, I argued that just as the discovery of scientific facts generally requires theoretical developments, theory invention generally requires the discovery of new scientific facts or the reconsideration of the known ones. In other words, the development of science is based on both factual and theoretical considerations. Therefore, it is a mistake to describe different theories as incommensurable. To say that there is an incommensurability of paradigms or theories is to suggest that theories are independent of facts. Indeed in the development of science, it is the discovery of scientific facts that leads to the uncovering of anomalies in an existing theory and the adjustments of candidate theories. Only after a long process in which it is tempered by facts, is a theory established. It should not therefore be said that the theory is decisive and that all facts have to be restated in accordance with the theory. Though different theories deal with different facts, it is usually possible to find enough features to compare them. It is not true, as Kuhn and Feyerabend allege, that there is no continuity of traditions, and changes of tradition are sudden and unstructured events, making theory choice depend on the authority of community.

Tradition-following and rule-following are intimately related. When scientists follow a tradition they are not doing something contrary to rule-following, nor are they proceeding without any guidance from theories. They follow it in a way that involves a "whole experience". If this is right, scientists engaged in research follow rules embodied in paradigms. When they discover new scientific facts or invent new theories, theoretical and practical knowledge may both play a significant role.

In the course of discussing how tradition functions in changes of paradigm, I showed that discovery cannot be separated from justification. In fact, scientists are able to proceed on the basis of models of existing knowledge in revolutionary science in much the same way as they proceed on the basis of paradigms in normal science. Extending Kuhn's view on education, I suggest that scientists acquire the ability to see similarities between individual sciences. In scientific revolutions, they imitate the previous paradigms and attempt to find analogies between a new area and an area previously dealt with, exploiting attachments that have proved effective in the already familiar area. Maxwell's case and the history of the development of Quantum Mechanics show that scientists may form new theories by finding analogies between new and existing theories, and by reasoning using those theories to form the new ones. Moreover, this kind of development is rational because, no matter whether scientists change some of the content of existing theories or not, they take advantage of existing knowledge as much as possible.

If science develops as I have described, it always keeps some aspects of earlier views. These provide a basis for justifying its development and judging its progress. Scientific theories are, I have also argued, closely linked with the objective world. Since objective factors deeply effect the development of science, science has its own order of development, and the choice of the individual scientist may be properly regarded as a somewhat superficial phenomenon. So, acknowledging that tradition plays an important role in science is not at all to hold that it is arational or relativistic.

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